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The American Political Science Review

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Robert E. Ward
APSA Presidential Address: Culture and the Comparative Study of Politics

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CONTENTS

11 Thucydides' View of Athenian Imperialism — Christopher Bruell

18 Politics and the Career Mobility of Bureaucrats — Eugene B. McGregor, Jr.

27 Changing the Rules Changes the Game: Party Reform and the 1972 California Delegation to the Democratic National Convention — William Cavala

43 Comment — Austin Ranney

45 Social Mobilization and the Russification of Soviet Nationalities — Brian Silver

67 The Divisive Primary Revisited: Party Activists in Iowa — Donald Bruce Johnson and James R. Gibson

78 Virtue, Obligation and Politics — Stephen G. Salkever

93 Generational Change in American Electoral Behavior — Paul R. Abramson

106 Membership Stability in Three State Legislatures: 1893–1969 — David Ray

113 The 3/2's Rule in Presidential Campaigning — Steven J. Brams and Morton D. Davis

135 Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives — Peter H. Aranson, Melvin J. Hinich and Peter C. Ordeshook

153 Helvétius and the Roots of the "Closed" Society — Blair Campbell

169 Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision-Making — James T. Murphy

187 APSA Presidential Address

188 Introduction — Lucian W. Pye

190 Culture and the Comparative Study of Politics, or The Constipated Dialectic — Robert E. Ward

202 Communications

214 Editorial Comment

217 Book Reviews

217 The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left — Ole R. Holsti

243 The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science — Ted Robert Gurr

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ARTICLES

11 **Thucydides' View of Athenian Imperialism.** The paper attempts, through an examination of Thucydides' treatment of a significant problem, to illustrate the character of an older approach to the understanding of politics and to argue, implicitly, for the validity of that approach. The approach in question is shaped by the thoughts that the barriers standing in the way of a "scientific" approach to politics are much greater than today is sometimes assumed and that there is no more direct path to the removal of those barriers than the painstaking elaboration and subsequent refinement of the assumptions which shape and frame our view of the issues of politics. Thucydides' "history" is seen as intended to facilitate such an elaboration and refinement; his treatment of Athenian imperialism is accordingly presented in various stages, which first appeal to the prejudices with which the reader is likely to approach the work and then lead him gradually, through an examination of these, to Thucydides' own understanding, which is much more heterodox than is ordinarily supposed.

By Christopher Bruell, Assistant Professor of Political Science, Boston College.

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18 **Politics and the Career Mobility of Bureaucrats.** Career mobility is conceptualized in terms of the *amount* and *bias* of organizational, occupational, vertical and geographical movement. An operational measure is offered for the amount of mobility (not the bias) which permits comparative generalizations to be made about officials based on simple sampling procedures. For illustrative purposes, the amount of organizational mobility is measured for two samples of federal officials: David T. Stanley's sample ($N = 557$) of higher civil servants and a sample ($N = 300$) of foreign affairs officials. Several dichotomous background variables are included for both samples of officials including: age, rank, and education. Varying background characteristics is found to make only a small mobility difference for higher civil servants and a much greater difference for foreign affairs officials. Several speculations are offered to explain the political significance of the empirical findings, and a comparative typology of career mobility is developed.

By Eugene B. McGregor, Jr., Associate Professor of Public and Environmental Affairs, Indiana University.

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27 **Changing the Rules Changes the Game: Party Reform and the 1972 California Delegation to the Democratic National Convention.** Rules changes in the Democratic party have substantially altered one of the fundamental institutions of American politics. This article is an effort to assess some of the effects of the new rules on the selection of delegates to the Democratic National Convention on the strategic environment faced by presidential candidates. The article outlines the basic aspects of those rule changes, describes the problems involved in the implementation of the rules, and attempts to evaluate some of the consequences faced by the presidential campaign of Senator George McGovern in the context of the California primary of 1972.

By William Cavala, Acting Instructor in Political Science, University of California, Berkeley.
Comment. By Austin Ranney, Professor of Political Science, University of Wisconsin, Madison.

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45 **Social Mobilization and the Russification of Soviet Nationalities.** This paper examines certain major demographic bases of ethnic identity change among the mass populations of the non-Russian nationalities of the USSR. Ethnic identity is defined here as an individual's affective attachment to certain core symbols of his nationality group: the group name and its historic language. The hypotheses tested concern the impact of social mobilization, contact with Russians, and traditional religion on ethnic identity change. The levels of Russification of 46 indigenous nationalities whose official national homelands have Autonomous Oblast status or higher are examined on the basis of 1959 Soviet census materials. By use of regression analysis it is shown that: (a) social mobilization is strongly conducive to the Russification of non-Russian nationalities residing in their official areas; (b) exposure to Russians is conducive to the Russification of both mobilized and unmobilized local populations, but the Russification effect of exposure to Russians is much smaller for the unmobilized than for the mobilized populations; (c) even where exposure to Russians is extensive and enduring, both socially mobilized and unmobilized Muslim ethnic groups are much less likely to be Russified than non-Muslims; it is proposed that a Muslim ethnic ideology mediates between the dynamic demographic influences on Russification and the actual manifestation of Russification.

By Brian Silver, Assistant Professor of Government, Florida State University.

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67 **The Divisive Primary Revisited: Party Activists in Iowa.** This study was conducted to test the frequently made assertion that primary elections are divisive among party activists who participate

in primary and subsequent general election campaigns. Analysis of data collected from 209 campaign workers personally identified by five candidates in two vigorously contested races for Congress in Iowa's First District indicated that the activists were an elite group on whom the primary had a divisive impact that was particularly notable among those who supported candidates who lost the primary. The alienation felt by many respondents who worked for losers in both parties adversely affected their general party support, voting, and the willingness to perform the many conventional activities necessary to a general election campaign, but it did not appear to have a substantial influence on the general election. Generally, those who defected were less experienced and less strongly identified with their parties than those who remained active. Moreover, most of the antagonism may be limited to the single general election; eighty per cent of all participants in the study expected to be as active or more active in future campaigns.

By DONALD BRUCE JOHNSON, Professor of Political Science and JAMES R. GIBSON, Graduate Assistant in Political Science, University of Iowa.

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78 **Virtue, Obligation and Politics.** This paper presents a conceptual analysis of the terms "virtue," "obligation," and "politics," a project suggested by similar analyses currently underway in the field of ethics. The essence of the study is the contrast between politics understood in terms of virtue (as by Plato, Aristotle, and, in a way, Rousseau) and politics understood in terms of obligation (as by Hobbes, Locke, John Rawls and, in a way, Rousseau). The paper argues that obligation and virtue form the center of two separate languages or paradigms for the formulation and discussion of basic political questions, and discusses the theoretical grounds for the neglect of the language of virtue by the greater part of modern political thought. This discussion, while pointing to the possible weaknesses of the language of virtue, also serves to indicate (directly and by contrast) the limitations of the language of obligation as a way of understanding politics.

By STEPHEN G. SALKEVER, Assistant Professor of Political Science, Bryn Mawr College.

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93 **Generational Change in American Electoral Behavior.** The relationship of social class to partisan choice in the United States has declined during the postwar years. Through an analysis of presidential election surveys conducted by the Survey Research Center of the University of Michigan from 1948 through 1968, it is demonstrated that this decline is largely a result of generational change. Strong relationships between class and partisan choice persist among older voters, but among younger voters these relationships are weak. A time-series cohort analysis provides considerable support for an historically based generational explanation for age-group differences and permits examination of one process through which partisan realignments may occur.

By PAUL R. ABRAMSON, Associate Professor of Political Science, Michigan State University.

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106 **Membership Stability in Three State Legislatures: 1893–1969.** It is often asserted that state legislatures are characterized by low membership stability, attributed to low career commitment on the part of individual legislators. Such assertions are usually based on the high proportion of first-term members in most legislatures, even though the available data on this subject are limited and contradictory. Two better measures of career commitment and membership stability are: (a) the number of incumbents seeking re-election, and (b) average prior legislative service. Data for these variables are presented for 20 sessions of the Connecticut, Michigan, and Wisconsin legislatures. The data show a gradual but substantial decline in the number of first-term legislators and a gradual but substantial increase in membership stability. The computation of such data is an essential first step in documenting the causes and consequences of membership stability.

By DAVID RAY, Teaching Assistant in Political Science, Stanford University.

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113 **The 3/2's Rule in Presidential Campaigning.** The purpose of this article is to assess the effect of the winner-take-all feature of the Electoral College on the allocation of resources by candidates to the states in a presidential campaign. Conceptualizing the campaign as a two-person zero-sum infinite game, it is found that the main effect of this feature is to induce candidates to allocate campaign resources roughly in proportion to the 3/2's power of the electoral votes of each state, which creates a peculiar bias that makes voters living in the largest states as much as three times as attractive campaign targets as voters living in the smallest states. Empirically, it is shown that the 3/2's rule explains quite well the time allocations of presidential and vice-presidential candidates in the 1960, 1964, 1968, and 1972 campaigns; for presidential campaigns in 1976 and 1980, optimal allocations are indicated for all fifty states and the District of Columbia. A comparison with optimal allocations under a system of direct popular-vote election of the president reveals that

such a system would be less susceptible to manipulative strategies than the Electoral College as well as being compatible with the egalitarian principle of "one man, one vote."

By Steven J. Brams, Associate Professor of Politics, New York University and Morton D. Davis, Associate Professor of Mathematics, City College of New York.

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135 **Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives.** This essay ascertains some general conditions for equivalence and nonequivalence among six election objectives: 0_1, maximizing expected plurality; 0_2, maximizing proportion of expected vote; 0_3, maximizing expected vote; 0_4, maximizing probability that plurality exceeds some level; 0_5, maximizing probability that proportion of vote exceeds some level; 0_6, maximizing probability that vote exceeds some level. The major findings are these: (1) 0_1, 0_2, and 0_3 are equivalent if the election is zero-sum-like in expected vote; (2) 0_1 and 0_2 are equivalent if competition is strongly symmetric. A necessary condition for this equivalence is also presented for 2-candidate elections: (3) 0_1 and 0_4 are equivalent, as are 0_3 and 0_6, if the candidate's forecasting error is independent of all strategies; (4) 0_1 and 0_4 are equivalent for two-candidate elections, and for n-candidate elections 0_2 and 0_5 are equivalent, as are 0_3 and 0_6, if the distribution of a candidate's forecasting error is multivariate normal, and if the level of plurality, proportion, or vote to be exceeded is the minimax value of the election game under 0_1, 0_2, or 0_3; (5) findings of equivalence and nonequivalence depend upon the definition of equivalence (findings 1 and 2 rely upon an election with all candidates at equilibrium strategies, while findings 3 and 4 do not); (6) equivalence and nonequivalence among election objectives may be sensitive to the candidate's attitude toward risk, i.e., to the functional form of his utility function in pluraliy, vote proportion, or vote; election objectives depend on information, competitive environment, and constitutional arrangements. Hence, statements of preference for alternative election systems, laws, and reforms perforce entail reasonable theoretical expectations about the way in which these systems, laws, and reforms affect the candidates' campaign objectives, as well as about equivalence and nonequivalence among these objectives.

By Peter H. Aranson, Associate Professor of Industrial Management, Georgia Institute of Technology, Melvin J. Hinich, Professor of Statistics and Political Economy, Carnegie-Mellon University, and Peter C. Ordeshook, Associate Professor of Political Science, Carnegie-Mellon University.

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153 **Helvétius and the Roots of the "Closed" Society.** The argument of this essay is that Talmon and Popper are mistaken in their suggestion that speculation about human affairs is governed by an inexorable logic of political consequences: that there exist certain broad perspectives or 'paradigms' that impel men willy-nilly to pathological extremes in their political views, apart from intention or historical circumstance.

I seek to demonstrate that the general perspective which informed the thought of Helvétius—unquestionably one of the most manipulative of thinkers in his conception of politics—was simply the framework of early-modern science, as it was understood in France. It was the same philosophy which served his unequivocally libertarian contemporaries, such as Voltaire and Diderot, as well as their predecessors. Helvétius' political conclusions resulted, not from pathological attitudes or doctrines, but rather from his attempt to resolve a problem engendered within the new science, a fundamental dilemma in French thinking concerning the relationship of the individual to society and the state.

By Blair Campbell, Assistant Professor of Political Science, University of California, Los Angeles.

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169 **Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision Making.** This study of the House Public Works committee examines the relationship between membership goals and the degree of party conflict, identifies conditions of party conflict and cooperation, and links party conflict and cooperation to policies adopted by the House.

Party conflict is the Public Works committee's most striking behavioral characteristic. This party conflict stems from (a) the issues processed by the committee and (b) the partisan program orientations of committee members. Beyond these initial sources of party conflict on Public Works, there is a norm of partisanship adopted by each of the parties on the committee in order to achieve an extra measure of party cohesion. Public Works committee party conflict is, to be sure, often modified by shared interests cutting across party lines. Committee party cooperation stems, however, not from widespread shared interests but, instead, from a fundamental distrust between the parties respecting the allocation of federal largesse. To preclude porkbarreling, Congress has adopted fixed allocation formulas for distributing the boodle on programs likely to involve a majority in each party, thereby constraining the parties on the committee to cooperate on such proposals.

Whether Public Works is united or divided when reporting legislation, the House of Representatives will typically adopt its proposals. This success of the Public Works committee is attributable to a mixture of anticipation and influence.

By JAMES T. MURPHY, Assistant Professor of Government, Wesleyan University.

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187 APSA Presidential Address

Introduction

By LUCIAN W. PYE, Professor of Political Science, Massachusetts Institute of Technology.

Culture and the Comparative Study of Politics, or The Constipated Dialectic. This presidential address delivered at the Association's September 1973 Annual Meeting at New Orleans treats the origins, growth, and consequences of the professional tensions that have developed between proponents of the so-called "area" and "behavioral" approaches to the comparative study of politics. It attributes such tensions to intra- and extramural competition for status and funds, to unrevised and ill-informed stereotypes based on early misapprehensions of actual practice and intent, and to misplaced scholarly zeal found largely on the behavioral side. The author sees the remedies as lying in a broader recognition of the critical and differing influence of cultural variations on the comparative study of politics, in a more widespread and systematic incorporation of sound area as well as behavioral preparation into our graduate training programs, and in a degree of planned internationalization of our basic social science endeavors abroad that is as yet uncommon.

By ROBERT E. WARD, Professor of Political Science and Director of the Center for Research in International Studies, Stanford University.

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202 COMMUNICATIONS

From Peter H. Lemieux, Fred Kort, David Pfotenhauer, Philip R. Stewart, Walter Dean Burnham, Edward R. Tufte

---

214 EDITORIAL COMMENT

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217 BOOK REVIEWS AND ESSAYS

217 **The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left.** The political spectrum has often been viewed as a linear continuum on which the extremes of the right and left occupy the most antithetical positions. The alternative hypothesis is that there are some dimensions on which the extremes resemble each other. This essay examines the theories of international politics and foreign policy espoused by scholars of the radical right and left. Two dozen points of convergence are grouped under five headings: *Understanding history and politics*, *the causes of war*, *the nature of the enemy*, *the conditions of peace*, and *ends and means in politics*. Because the essay is focused on studies of international politics since the outbreak of World War II, considerable attention is devoted to the parallels between rightwing theories of the USSR and Soviet foreign policy, and leftwing explanations of the United States and American foreign policy. The conclusion suggests that both theories are fundamentally flawed in two respects: (1) As employed by their proponents, the theories appear incapable of being falsified; and (2) studies employing them are marred by serious methodological flaws that violate the canons of systematic inquiry.

By OLE R. HOLSTI, Professor of Political Science, University of British Columbia.

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243 **The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science.** Four major new compilations of macropolitical data are compared and evaluated. Each summarizes a large-scale research effort to code or to collect data suitable for theoretically relevant, cross-national comparisons. As a group the new handbooks incorporate many improvements and innovations on earlier handbooks, which concentrated mainly on cross-sectional, aggregate data or simplistically coded judgments about nation-states. About a third of their measures consist of "made" data, derived by coding journalistic and historical sources. All provide some measures for cross-time comparisons; one is devoted exclusively to time-series data. Many of their measures denote properties of internal and international conflict and of international transactions. All but one are painfully self-conscious about problems of reliability and comparability of data. One criticism is the reliance of several of the handbooks on "counts" of conflict events rather than assessment of more theoretically relevant properties of conflict. A second is the paucity of indicators of inequality and, more generally, of measures which give a "view from the bottom" of political systems.

By TED ROBERT GURR, Professor of Political Science, Northwestern University.

253 Political Theory, History of Political Thought and Methodology

V. G. Afanasyev, *The Scientific Management of Society*. Arthur Trueger, p. 253.

F. G. Bailey, ed., *Gifts and Poison: The Politics of Reputation*. Lewis A. Dexter, p. 254.

Alexis de Tocqueville, *Journey to America*. Trans. by George Lawrence. Edited by J. P. Mayer. Aldo Tassi, p. 254.

Edgar S. Dunn, Jr., *Economic and Social Development: A Process of Social Learning*. Roy E. Feldman, p. 255.

Murray Edelman, *Politics as Symbolic Action: Mass Arousal and Quiescence*. Marc Howard Ross, p. 257.

R. Hackforth, trans., *Plato's Phaedo, Plato's Phaedrus, Plato's Philebus* (three volumes). Thomas L. Pangle, p. 258.

Jeanne E. Knutson, *The Human Basis of the Polity: A Psychological Study of Political Men*. Ira Rohter, p. 260.

Gaetano Mosca, *The Ruling Class: Elementi di scienza politica*. Dante Germino, 261.

Matilda White Riley et al., *Aging and Society; Vol. I: An Inventory of Research Findings, Vol. II: Aging and the Professions, and Vol. III: A Sociology of Age Stratification*. Frederick R. Eisele, p. 262.

Patrick Riley, trans. and ed., *The Political Writings of Leibniz*. Richard Kennington, p. 263.

J. M. Roberts, *The Mythology of the Secret Societies*. Nannerl O. Keohane, p. 265.

Paul E. Sigmund, *Natural Law in Political Thought*. Judith N. Shklar, p. 266.

Goldwin Smith et al., *The Professor and the Public: The Role of the Scholar in the Modern World*. Jurgen Herbst, p. 267.

D. Woods Thomas, Harry R. Potter, William L. Miller, and Adrian F. Aveni, eds., *Institution Building: A Model for Applied Social Change*. Seymour B. Sarason, p. 268.

Ernest van den Haag, *Political Violence and Civil Disobedience*. Jeffrie G. Murphy, p. 269.

Eugene Victor Walter, *Terror and Resistance: A Study of Political Violence, with Case Studies of Some Primitive African Communities*. Ali A. Mazrui, p. 271.

272 American Government and Politics

Edna Bonacich and Robert F. Goodman, *Deadlock in School Desegregation: A Case Study of Inglewood, California*. Leslie Lenkowsky, p. 272.

William R. Brock, *The Evolution of American Democracy*. Richard P. McCormick, p. 273.

Richard Buel, Jr., *Securing the Revolution: Ideology in American Politics, 1789–1815*. Richard H. Kohn, p. 274.

Harold W. Chase, *Federal Judges: The Appointing Process*. David Adamany, p. 275.

Randolph C. Downes, *The Rise of Warren Gamaliel Harding, 1865–1920*. Frank A. Burd, p. 277.

Wayne E. Fuller, *The American Mail: Enlarger of the Common Life*. Paul T. David, p. 278.

William C. Havard, ed., *The Changing Politics of the South*. Alexander Heard, p. 278.

Barbara Hinckley, *Stability and Change in Congress*. Randall B. Ripley, p. 280.

Sidney Hyman, *Youth in Politics: Expectations and Realities*. Everett Carll Ladd, Jr., p. 281.

Anne C. Loveland, *Emblem of Liberty: The Image of Lafayette in the American Mind*. Michael McGiffert, p. 282.

Alan Lupo, Frank Colcord, and Edmund P. Fowler, *Rites of Way: The Politics of Transportation in Boston and the U.S. City*. G. Ross Stephens, p. 282.

Pauline Maier, *From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776*. Lance Banning, p. 284.

Richard E. Morgan, *The Supreme Court and Religion*, Jacob W. Landynski, p. 285.

Kenneth J. Pollinger and Annette C. Pollinger, *Community Action and the Poor: Influence vs. Social Control in a New York City Community*. Kenneth R. Greene, p. 286.

Lawrence R. Velvel, *Undeclared War and Civil Disobedience: The American System in Crisis*. W. Taylor Reveley III, p. 287.

John C. Weistart, ed., *Community Economic Development*. Emmett H. Buell, Jr., p. 288.

Who's Who in Government, 1st edition, 1972–1973. Thomas E. Cronin, p. 290.

Frederick M. Wirt, Benjamin Walter, Francine Rabinovitz, and Deborah Hensler, *On the City's Rim: Politics and Policy in Suburbia*. John H. Baker, p. 291.

292 Comparative Government and Politics

Theodore L. Becker, *Comparative Judicial Politics;* and Laura Nader, ed., *Law in Culture and Society*. Herbert Jacob, p. 292.

Jacques Berque, *Egypt: Imperialism and Revolution*. John P. Entelis, p. 293.

Helmut Böhme, ed., *The Foundation of the German Empire: Select Documents*. Trans. by Agatha Ramm. Otto Pflanze, p. 295.

D. G. Boyce, *Englishmen and Irish Troubles: British Public Opinion and the Making of Irish Policy, 1918–1922*. Paul A. Pfretzschner, p. 295.

James Bunyan, *The Origin of Forced Labor in the Soviet State, 1917–1921: Documents and Materials*. Leonard B. Schapiro, p. 296.

Ronald H. Chilcote, *Emerging Nationalism in Portuguese Africa: Documents*. Gerald J. Bender, p. 296.

David C. Cole and Princeton N. Lyman, *Korean Development: The Interplay of Politics and Economics* Gregory Henderson, p. 297.
Vladimir Dedijer, *The Battle Stalin Lost: Memoirs of Yugoslavia, 1948–1953.* Milan J. Reban, p. 298.
Fritz T. Epstein, ed., *The American Bibliography of Russian and East European Studies for 1966.* John A. Armstrong, p. 299.
Max Gallo, *The Night of Long Knives.* Trans. by Lily Emmet; and Harold J. Gordon, Jr., *Hitler and the Beer Hall Putsch.* C. F. Latour, p. 300.
Jerome M. Gilison, *British and Soviet Politics: Legitimacy and Convergence.* A. H. Brown, p. 301.
Mary Ellen Goodman, *The Culture of Childhood: Child's-Eye Views of Society and Culture.* R. W. Connell, p. 302.
Peter M. Gukiina, *Uganda: A Case Study in African Political Development.* James H. Mittelman, p. 303
Dirk Hartung, Reinhard Nuthmann, and Dietrich Winterhager, *Politologen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationen im Beschäftigungssystem.* John Dreijmanis, p. 304
Gisela Hersch, *A Bibliography of German Studies, 1945–1971.* Manfred Henningsen, p. 305
Hajo Holborn, *Republic to Reich: The Making of the Nazi Revolution;* Eberhard Jäckel, *Hitler's Weltanschauung: A Blueprint for Power;* and Percy Ernst Schramm, *Hitler: The Man and the Military Leader.* Gerhard L. Weinberg, p. 306
Chün-tu Hsüeh, ed., *Revolutionary Leaders of Modern China.* Robert E. Bedeski, p. 307
W. J. Hudson, ed., *Australia and Papua New Guinea.* Francis West, p. 308
John O. Iatrides, *Revolt in Athens: The Greek Communist "Second Round" 1944–1945.* Van Coufoudakis, p. 309
Institut d'Études Politiques de Bordeaux, under the direction of Albert Mabileau, *Les Facteurs locaux de la vie politique nationale.* William R. Schonfeld, p. 310
Rounaq Jahan, *Pakistan: Failure in National Integration.* Lawrence Ziring, p. 311
Chalmers E. Johnson, *Conspiracy at Matsukawa.* Kenneth E. Colton, p. 312
Robert R. Kaufman, *The Politics of Land Reform in Chile.* Robert L. Ayres, p. 313
J. A. La Nauze, *The Making of the Australian Constitution.* G. C. Bolton, p. 315
Walter C. Langer, *The Mind of Adolf Hitler;* and Robert Payne, *The Life and Death of Adolf Hitler.* A. James Gregor, p. 316
H. Lever, *The South African Voter.* Pierre L. van den Berghe, p. 318
Anatole G. Mazour, *The Writing of History in the Soviet Union.* George Enteen, p. 319
K. R. Minogue, *Nationalism.* Walker Connor, p. 320
Martin Needler, *Politics and Society in Mexico;* and Daniel Cosío Villegas, *El Sistema Político Mexicano: Las Posibilidades de Cambio.* David Ronfeldt, p. 322
Howard Pack, *Structural Change and Economic Policy in Israel.* Ruth Klinov, p. 324
Robert Pringle, *Rajahs and Rebels: The Ibans of Sarawak under Brooke Rule, 1841–1941.* M. C. Ricklefs, p. 325
J. T. Purcal, *Rice Economy, Employment and Income in Malaysia.* Martin Rudner, p. 326
A. N. R. Robinson, *The Mechanics of Independence: Patterns of Political and Economic Transformation in Trinidad and Tobago.* Wendell Bell, p. 326
Leslie L. Roos, Jr. and Noralou P. Roos, *Managers of Modernization: Organizations and Elites in Turkey (1950–1969).* James W. Land, p. 328
Richard Sisson and Lawrence Shrader, *Legislative Recruitment and Political Integration: Patterns of Political Linkage in an Indian State.* William L. Richter, p. 329
Denis Mack Smith, *Victor Emanuel, Cavour, and the Risorgimento.* Giovanni Bognetti, p. 330
Edward W. Soja, *The Geography of Modernization in Kenya: A Spatial Analysis of Social, Economic, and Political Change.* Jay E. Hakes, p. 331
S. Frederick Starr, *Decentralization and Self-Government in Russia, 1830–1870.* Marc Raeff, p. 332
Surveys and Research Corporation, *Directory of Selected Scientific Institutions in Mainland China.* Susan Beth Rifkin, p. 333
Reginald E. Zelnik, *Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855–1870.* Richard Pipes, p. 333
Marvin Zonis, *The Political Elite of Iran.* Marvin G. Weinbaum, p. 334

335 **International Politics, Law and Organization**
Luis E. Aguilar, *Cuba 1933, Prologue to Revolution.* Manuela Semidei, p. 335
Bennett Boskey and Mason Willrich, eds., *Nuclear Proliferation: Prospects for Control.* Robert L. Rothstein, p. 336
Adda B. Bozeman, *The Future of Law in a Multicultural World.* Wesley L. Gould, p. 338
Silviu Brucan, *The Dissolution of Power: A Sociology of International Relations and Politics.* Robert L. Farlow, p. 339
Joseph I. Coffey, *Strategic Power and National Security.* Jerome H. Kahan, p. 340
Herbert Corkran, Jr., *Patterns of International Cooperation in the Caribbean, 1942–1969.* Thomas Mathews, p. 341
Yehezkel Dror, *Crazy States: A Counterconventional Stratgeic Problem.* Donald L. Hafner, p. 342
John King Fairbank, *The United States and China,* 3rd edition C. P. FitzGerald, p. 344
Richard A. Falk, ed., *The International Law of Civil War.* Oliver J. Lissitzyn, p. 345

- Robert B. Harmon, *The Art and Practice of Diplomacy: A Selected and Annotated Guide*. Elmer Plischke, p. 347
- George S. Harris, *Troubled Alliance: Turkish-American Problems in Historical Perspective, 1945–1971*. Richard H. Pfaff, p. 348
- Ghita Ionescu, ed., *The New Politics of European Integration*. Ernst B. Haas, p. 349
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Thucydides' View of Athenian Imperialism*

CHRISTOPHER BRUELL
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Our topic appears for more than one reason to be an historical or merely historical one, for it concerns the thought of a man of the past who was himself an historian, i.e., who was concerned above all with what belongs to the past. The appearance is deceptive at least to this extent: Thucydides is not properly classified as an historian. He never calls himself an historian, and he claims that the war which he "put together in writing" is of such a size and character that it is able to reveal to him and to those for whom he writes the comprehensive and therewith eternal truth of human or political life. His study of Athenian imperialism as a cause or ingredient of that war serves his great task and to that extent its seriousness. But this means that precisely if our study is to be historical it must not be merely historical. We must try, to begin with, to understand Thucydides as he understood himself; we must take seriously his claim, however extravagant it may appear; we must be open to the possibility that he is correct. We are not averse to such a possibility, despite the austerity or sternness of his work; for that austerity is suffused with gentleness, his exactingness with humanity, and we sense in and through his work the presence of a nobility which is lasting because it is undeceived (cf. Book III, Chapter 83).[1]

This perhaps would be sufficient introduction to our study if the problem of our openness to or ability to learn from Thucydides were solved by our willingness to consider the possibility that he is correct. But, as it is, that willingness itself, insofar as it implies that we are somehow competent to judge of his correctness, is not free from problems. An essential part of such competence would be freedom of mind or openness to the truth. Now, Thucydides does not seem to assume that his readers approach him with this openness, although he clearly writes for those who are capable of it (I, 22). The problem is not merely the unwillingness of the many to toil at the search for truth (I, 20), but rather something which affects even the well-disposed few. For all of us, he seems to feel, are born and bred to certain convictions and to a certain comprehensive outlook which shape our understanding of the great matters that he deals with and which perhaps reflect the truth but remain nevertheless at a certain remove from it. Those convictions, further, answer to concerns more powerful than our concern for truth; our attachment to them defies attempts, by dedication to the truth, to loosen its bonds. The concerns in question, together with the views which answer to them, would block completely our access to the truth they conceal if there were not a link between them and the concern for truth, or a path from the ones to the other, as well as a path between the primary or naive views and the adequate view which Thucydides intends to teach. But this means that his teaching must begin with powerful opinion as opposed to truth and must appeal in the first place to concerns other than the concern for truth. The openness to truth is not a precondition of study so much as it may be the outcome and reward of a certain kind of study.

As more than one commentator has observed,[2] Thucydides makes of his reader a witness to the deeds and speeches he puts down: so penetrating is his narration that the men and nations of which he writes comes to life before us; we hear their speeches and see their deeds. And since the matters with which they are concerned—war and peace, justice and injustice, freedom and empire—retain their place in the economy of human life, we not only hear and see but we are also moved. We are moved by, we respond to his narration in accordance with our fundamental view of these matters, however vague or undefined that view may be. In being moved, we are taking a stand that reflects that view; we cannot read Thucydides without taking a stand and to that extent articulating our fundamental position. It is by the sympathetic fostering of the articulation of our position, it seems, that Thucydides begins to lead us to the considerations in the light of which it can be corrected or abandoned.

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* I am indebted in many points, both general and particular, to the discussion of Thucydides in Leo Strauss, *The City and Man* (Chicago: Rand McNally, 1964).

[1] These references, which will hereafter be indicated by number alone, are standard for any Greek edition of Thucydides and are referred to in most translations.

[2] Cf. the introductions to the Penguin (Thucydides: *History of the Peloponnesian War*, translated with an introduction by Rex Warner [Baltimore: Penguin Books], p. 5) and Loeb (*Thucydides*, translated by Charles Foster Smith [London: William Heinemann Ltd., 1919], p. xvii) translations.

We are confronted then almost at the outset of the tale by the fact of Athenian imperialism. Not long after the Persian war there arose in the Greek alliance which had repulsed the barbarian a division followed before long by intermittent warfare between those who were attached to the Athenians and those who remained under Spartan leadership (I, 18). In time, the Athenians took over the ships of most of the cities allied with them and imposed tribute on them all. As a result, their military establishment at the start of the Peloponnesian war was greater than that of the united alliance at its height (I, 19). We can say then that Athenian imperialism is a condition of the greatness of the war and therewith of its particularly revealing character (I, 1; cf. I, 15). We remain perhaps truer to the proper perspective if we limit ourselves at this point to the assertion brought forward by the Corinthians and the Spartans, and indeed by Thucydides himself (I, 23 and 88), that the imperial greatness of Athens brings on the war.

The two incidents which constituted the proximate provocations to war and furnished the immediate grounds of quarrel had given confirmation to her enemies of the character and scope of the Athenian threat. First, the Athenians accepted a defensive alliance with Corcyra, perhaps the second naval power in Greece (I, 33), which was bound to bring them into conflict with a leading city of the Peloponnese, Corinth. They did so partly because they thought that war with the Peloponnesians was coming in any case, and they wished to wear out the Corcyraeans and Corinthians in mutual combat. At the same time, they were impressed by the location of the island with regard to Italy and Sicily (I, 44). It is not said whether this fact impressed them from an offensive or defensive point of view, but just as subsequent events may be sufficient to settle this question for us, we may expect that the aggressive Athenian policy of the previous fifty years was sufficient to suggest an answer at the time to the anxious minds of the non-Athenian Greeks. At any rate, this alliance led to a clash between Athens and Corinth. After this, anticipating a Corinthian move to detach from them their ally, Potidaea, which was linked by ancestral ties to Corinth, the Athenians demanded of the Potidaeans that they take down their walls, hand over hostages, and expel, to admit no further, their Corinthian magistrates (I, 56). The Potidaeans then revolted from Athens with Corinthian encouragement and help, and in attempting to put down the revolt, Athens again clashed with Corinth.

The sequel to these events was a conference of the Spartan allies in which the question of war with Athens was mooted. It is not surprising that it was Corinth which took the lead in summoning this conference and in urging the injustice of Athenian policy and the necessity of war; but she was supported by other cities, each with its own charge to lay against the Athenians. After these cities had had their say, the Corinthians advanced to conclude the arguments. They directed the greater part of their complaints against Spartan inaction, for the case against the Athenians appeared to be clear enough: the injustice they are committing against Greece is manifest—some are already enslaved by them, others are being plotted against, and they are making great preparations for war (I, 68; cf. 103, 105–106, 119). But the Corinthians also painted a memorable picture of the Athenians as people who would never have peace themselves or permit it to others. (I, 70). The response of the Spartans was to vote that the treaty between Athens and Sparta had been broken and that injustice was being done by the Athenians (I, 87; cf. 79).

There are many reasons to sympathize with democratic Athens in the war to which this vote led, and therefore many temptations to make light of the arguments brought against her. We cannot tell at this stage whether even Thucydides does not share that sympathy. But he lets us hear the Corinthian condemnation of Athens before a single Athenian has spoken, and he takes the trouble to tell us that Greek opinion at the outset of the war was heavily on the side of the Spartans, who were promising to liberate Greece (II, 8; cf. I, 124). The anti-Athenian view seems to be primary for Thucydides; we can perhaps best understand the truth which underlies it if we turn to the Athenians' own accounts of their imperial policy.

At the time of the conference of the Spartan allies, a delegation of Athenians happened to be in Sparta on other business. On their own initiative, they came forward and were allowed by the Spartans to speak. They declined to answer the charges brought against Athens by the various cities (I, 73), but their words amount at least to a statement of the Athenian view of Athenian imperialism. They can say that the allies themselves asked them to assume the leadership after the Spartans withdrew. But they must then explain the transformation of the voluntarily offered hegemony into an empire over unwilling subjects. They say therefore that they were compelled to bring their rule into this form, above all by fear, then by honor as well, later by profit as well (i.e., by desire for honor and desire for profit). Subsequently, they seem to take the necessary further step of extending this "compulsion" to the original act of accepting the leadership: in accepting (and not abandoning) the empire, they were overcome by the greatest things, honor, fear and profit; nor were they the first to do such a thing, but it is forever laid down that the weaker is kept down by the stronger (I, 75, 76). The Athenians would thus appear to fullfill, by the offer of an inadmissible defense, their promise not to defend themselves: for what under certain circumstances might be allowed to fear, could not without the gravest consequences be granted to the desire for honor and profit. Other of their arguments tend to confirm this impression. It may be true that the Spartans, if they had remained in the leadership, would have found themselves under a like compulsion or necessity to become "painful" to the allies and to rule forcefully (I, 76); but does not this "if" strike directly at the Athenian case for "compulsion" to rule? We may grant that it is now hazardous for the Athenians to do the right by letting their subjects go (I, 75 at the end); but if it is former wrongs which produce the hazard, is their failure to face it to be excused? And with regard to their arguments as to their worthiness to rule and their acting with measure or with greater justice, given their power, than they have to (I, 76), might one not reply that moderation in crime is no great claim to merit among the noncriminal? It was perhaps with such considerations in mind that a Spartan leader said of the Athenian speech that it nowhere contained, despite its length, a denial that the Athenians were committing injustice against the Spartan allies and the Peloponnese. (I, 86 at the beginning).

During the war itself, the thought underlying Athenian policy was restated by the Athenian ambassadors at Melos in the course of the negotiation which preceded the Athenian invasion of that island. Their situation (i.e., the weakness of the Melians) allowed the Athenians to speak with even greater frankness than their countrymen had employed on the earlier occasion at Sparta. Accordingly, whereas the earlier speakers had declined to reply to the accusations brought against Athens on the ground that the Spartan assembly did not constitute a court of law (I, 73), the ambassadors at Melos decline to justify the empire or their aggression against Melos in the expectation that the parties to the discussion belong to those who know "that just things in human reckoning are determined on the basis of equal power to compel [literally: 'necessity'] while the superior do what they can and the weak yield" (V, 89). And in the course of a contemptuous dismissal of the Melians' hopeful trust in the favor of the gods—"because, free from sin, we stand against the unjust" (V, 104)—the Athenians go perhaps even further:

> As for good will on the part of the divine, we do not think that we will fall short either; for we don't claim as our right or do anything outside of what humans hold with regard to the divine or wish with regard to themselves; for as we believe on the basis of opinion with regard to the divine and *on the clear basis of unremitting natural* necessity with regard to humanity, where one gets the upper hand, he rules. And neither laying down the law, nor being the first to use it as laid down, but taking it as it is and will be forever when we have left it behind, we use it, knowing that you and others, if you became as powerful as we are, would do [the] same (V, 105).

On the other side, we are able to find that on a subsequent and less favorable occasion an Athenian by the name of Euphemus (otherwise unknown)[3] puts the case for the empire in more moderate terms. Above all, he ascribes its acquisition to fear alone (i.e., he excludes desire for honor and profit), for, as he explains, all are permitted to provide for their own safety without incurring envy (VI, 83; cf. 87 and 82). But in determining the weight to be given this un-Athenian instance of moderation (cf. *Euphemus' own reference to the difficulty of* "moderating" the Athenians, VI, 87), one must not overlook the context. The Athenians are in Sicily near the start of their disastrous attempt to enslave that island; they have already encountered some difficulties (e.g., VI, 71, 74–75); they are seeking to renew an alliance with a people who are among their intended victims. The situation seems to call for euphemism if not for a spokesman called Euphemus.

To all this we might add that even Pericles, who can with some justification be regarded as the most moderate of the great Athenian leaders—consider his advice on the war (I, 144 and II, 65) as well as Thucydides' eulogy more generally (II, 65)—tells the Athenians not long after the outbreak of the war that they hold their empire, by this time, "as a tyranny, which it is *deemed* wrong to take, but is dangerous to let go" (II, 63; my underlining). And he advises them to put up with the hatred in the present which is the lot of all who think fit to rule others, not merely on account of the dangers of letting go but also in light of the advantages which do and may in the future accrue to them from empire, and perhaps above all for the sake of the ever-remembered renown which it brings (II, 64 and 62).

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[3] Arnold Wycombe Gomme. *A Historical Commentary on Thucydides* (Oxford: Clarendon Press, 1945) on III. 41.

We should also take note of the account of the establishment of empire which Thucydides gives in his own name in the form of a digression from his narrative (I, 97) after his account of the conference at Sparta. He confirms the Athenian claim that it was the allies who first asked them to assume the leadership of the alliance. (I, 95, 96, 97) And he adds that the pretext for the Athenian transformation of alliance to empire was provided by revolts on the part of allies who did not wish to fulfill their obligations—strictly enforced by the Athenians—under the league. He explains that the suppression of these revolts was made possible, or at least easy, by the shirking of these same allies from the military exercise and exertions that would have prepared them to defend themselves (I, 99). But he also makes clear that the Athenian response to these coincident provocations and opportunities was the enslavement of the allied cities contrary to "that which had been established" (I, 98). (According to the *scholium* cited by Gomme, *ad. loc.*, this means "contrary to the lawful and proper." Other commentators mentioned by Gomme understand it to mean "contrary to the covenant of the league.") More generally, his account here of Athens in the fifty year period between the wars (Persian and Peloponnesian) is one of almost constant motion, as the Athenians, now in one place, now in another, against allies and nonallies, Greeks and foreigners, sometimes called in by others, sometimes, apparently, uncalled (e.g., I, 112), seek to extend their power and dominion. Nor did a corresponding Spartan activity in those years make less impressive, while it required, the Athenian policy. Thucydides closes this summary as follows:

> [In these years] the Athenians established their empire on a firmer footing and themselves advanced greatly in power. But the Spartans, while they perceived this, did not prevent it, unless to a small extent. They kept quiet for most of the time, being even before this not quick to go to war, unless they were forced, and also prevented to some extent by domestic wars. . . .

Only when the power of the Athenians was clearly exalted and they were already laying hands on the Spartan alliance, did it seem to the Spartans that it was time, if they could, to bring them down (I, 118).

The policy of the Athenians is not only imperialistic, it is admittedly so; their imperialism not only lacks for the most part legitimate pretext, but such pretext is rarely claimed. And if their frankness does not increase their guilt, it at least makes it plainer for all to see. This is the substance of what one may call the Spartan case with regard to the war, but could it not be Thucydides' view as well? If one accepts it one is able to discern a striking and oft-noted unity and indeed beauty in the movement of the work as a whole. For Pericles' funeral speech with its boundless praise of Athens and Athenian greatness, where Pericles does not hesitate to speak, not to say boast, of the eternal monuments of bad and good everywhere left by the Athenians (II, 41), is followed by the plague, by which Athens is brought so low. The Melian dialogue, in which the Athenian ambassadors put forward the claims of empire with the insolence already noted, is followed by the Sicilian expedition in which Athenian imperialism is seen finally to have overreached itself. And the work as a whole is the story of the setting of that Athenian greatness which, at the work's outset, rode so high. But this understanding of the book would call for satisfaction that punishment was indeed meted out where due, whereas the passion with which we are likely for the first time to put it down is rather sadness or melancholy. The reason is not so much that Thucydides does not permit us to see the final defeat of Athens, that the work closes shortly after the Athenians have managed to repair somewhat their political and military affairs, for his account of the terrible sufferings of the Athenians in Sicily would amply or more than amply fill that lack. It is rather that this great instance of the apparent victory of right does not succeed in diverting our attention from the many instances in which it does not appear victorious. The Sicilian narration itself is interrupted by an account of the almost senseless slaughter of the inhabitants of all ages of Mycalessus at the hands of Thracian mercenaries being sent home from Athens (VII, 29); and this may recall to mind the atrocities of the Spartan Alcidas, who slaughtered his prisoners, friend and foe alike (III, 32). We are not likely to have forgotten the Melians, who, however right their cause, were slaughtered by the Athenians, or the Plataeans, who suffered a similar fate at the hands of the Spartans (III, 68), or the willingness of the Spartans to abandon, as the price of peace, cities promised protection by their agent Brasidas (compare V, 18 with the account of Brasidas in Thrace), or the Corcyraean civil war, in which, under the color of party strife, men were killed for private hatreds or because they were owed money by their murderers (III, 81). The party strife itself, passing these bounds, became so savage that father killed son and men were murdered next to the very temples from which they had been dragged; this savagery in turn was surpassed—both in other respects and in the singularity of the revenges exacted—by that of the civil wars which, following this one, moved so to speak the whole of Greece at this time; revenge became more important than not suffering oneself; it was not limited by the just or the good of the city, but only by the pleasure of the avenging parties, each side having made the common things it professed to serve its prize; in the course of events, the citizens in the middle were destroyed by both sides, the clever by those of the meanest capacities, so that the latter, for the most part, were the ones who survived (III, 81–83). Such is the context in which one must understand the comment of Thucydides on the execution of Nicias: of the Greeks of that time, he least of all, in view of his attention to conventional virtue, deserved such a fate (VII, 86). It would appear to be rather the weakness than the strength of right that is Thucydides' theme, the consistent defeat or going down of right or at least the righteous before force, compulsion, necessity. That defeat is not in his view particular to the time of the Peloponnesian war but, as his "archeology" (account of olden times) and also his account of the settlement of Sicily—marked as it is by repeated invasions which drive whole peoples from their homes—indicate, it is true of all times. We must try to indicate somewhat more fully its character or what underlies it. In speaking of the effects of the plague, Thucydides says that, pressed by the greatness of the evil, people abandoned the laments for the dead, the respect due to temples, and the laws regarding burial (II, 51–52). Earlier he had spoken of an extraordinary necessity which forced the Athenians to transgress an oracle and occupy an accursed ground (II, 17). The Athenians, accused by the Thebans of treating sacred ground as profane, allege the necessity that forced them to it (IV, 98). The war which convulsed the Greek cities in both civil and foreign strife proved to be a violent teacher which assimilated the tempers of most men to their harsh circumstances (III, 82). Behind the defeat of the righteous by necessity or force, we discern the defeat of the righteousness of the previously just by a necessity or force whose character however has not yet been sufficiently brought out. In the course of the aforementioned civil wars there occurred such harsh things as "have been and will be always, so long as there is the same nature of human beings" (III, 82). In the course of the Athenian plague, the increase in general lawlessness as men turned openly to the pursuit of pleasure was caused precisely by the release of men from the constraints of fear of gods and law of men (II, 53). Neither release need always await a plague: the latter, for example, could be accomplished by great strength. But what of the Spartans, who, powerful as they were, were not emphatically imperialistic? Toward the end of his work, Thucydides tells us that next to the Spartans, the Chians were the most moderate in prosperity, i.e., refrained most from the use of the power they possessed, (VIII, 24). A little later he tells us that next to the Spartans the Chians possessed the most slaves (VIII, 40). The moderation of the Spartans with regard to empire, which had seemed to refute the contentions of the Athenians who spoke at Sparta, is due to nothing so much as their possessing many slaves, whom they must remain home to watch. On the other hand, the resort to justice on the part of the Melians, the most conspicuous spokesmen for justice in Thucydides, is explained by the fact that they have no other resort, and Thucydides as a rule entrusts the arguments which resort to justice either to the weak or to the insincere (for example, Cleon).

But if Thucydides is willing to grant this much to the argument of the Athenians at Sparta, he is far from condoning the mood of triumph which characterizes its Athenian utterance: if right does indeed bow in this way before necessity, no profit which one might derive from this fact would compensate for the ultimate loss. Thucydides begins the first speech in his book with the word "Right" and the second with the word "Necessary." Is not the tragedy to which that juxtaposition appears to point *the* theme of the work, a theme developed in the somber tones appropriate to it? Thucydides would appear to have replaced the Socratic question, What is justice, by the spectacle of the worldly weakness of the justice which we all know, perhaps foreshadowing in this respect Machiavelli, for whom the weakness of right rather than its character is the theme. But Thucydides has not abandoned the sense of what is lost.

The conclusion reached is exposed at least to this difficulty: one cannot continue to call unjust that which is done by a universal necessity; it belongs to the notion of right that the actions it requires be possible. This seems to be the meaning of the correction made by the

Athenians at Melos on the argument of their predecessors at Sparta. For they either deny that such an action as theirs is unjust, or they dare to suggest that it is just by an eternal law that is both human and divine.[4] It appears that we have not yet properly understood the thought contained in the juxtaposition of the terms right and necessity, for we failed to understand that precisely if that juxtaposition has the meaning previously suggested, the question of the character of right, the question what is right, is necessarily raised by it.

We must then examine more closely than we have as yet done the alleged necessity for the powerful to do wrong. Surely the fact, that those who are able to do wrong do not refrain from doing wrong, does not establish the necessity for them to do wrong. The manner in which the extent and character of such necessity can be established is indicated in the Melian dialogue. The Athenians had forbidden the Melians to speak of justice. They are to confine themselves to what is possible in view of what each side truly intends to do (V, 89). The Melians respond, forced, as they assert, to abandon considerations of justice for those of advantage, that they regard it as useful to the Athenians that the Athenians not destroy the common good, which holds that for him in danger whatever is plausible is just, and which allows him to derive benefit from persuading some one by arguments that fall short of precision: will not the Athenians one day be in more need of this common good than any one, destined, as they are, to be by their punishment an example to others (V, 90)? The Melians thus, contrary to their assertion, introduce the consideration of justice, for do we not mean by justice the common good? They also indicate the limits to their ability to demonstrate that it is to the Athenians' advantage to let them off. And indeed the Athenians, by arguments which there is no need to detail here, have no difficulty in showing them that it is not. But then there is no common good with regard to the Athenians and Melians; the Athenian invasion is dictated by a sound calculation of Athenian interest. Such interest, or the good, is a kind of necessity—it can be overriden, one can make mistakes, but not without its exacting the consequences of them. Nor, as the dialogue also indicates, are there any irremovable barriers standing between the good and our necessary desire to have it or directing our desire for the good away from its object. These then are the ineradicable roots of the "necessity" for those who can to do wrong, although for the reason indicated above, as well as because the Athenians and others cannot be accused of destroying what (in such cases as this at least) does not exist, we no longer speak of the necessity to do wrong.[5]

Because the Melian dialogue is followed eventually by the slaughter of the Melians, one might conclude that there is no more important task than the encouragement of the all too rare "mistakes" referred to or the creation or strengthening of the indicated barriers. But that conclusion would not be entirely correct, and not only because the slaughter is not required by the dialogue. The Athenian thesis is indeed compatible with, and to some extent conducive to, a remarkable gentleness. It goes without saying that it does not excuse the senseless butchery engaged in by Alcidas or the Thracian mercenaries (see Part Two above). But further, it is the Athenians who, whether or not their being more enlightened in this respect than others (e.g., the Spartans) is a spur to their imperialism, are also in their dealings with foreigners more gentle than, in particular, the Spartans. The quality in question and its cause appear most clearly in the thought and speech together with the deed of the singular Thucydidean character, Diodotus. Like his countryman Euphemus, Diodotus is unknown outside the pages of Thucydides;[6] his name means "Gift of Zeus"; and indeed he appears once to play his amazing part and then is gone. His speech abounds in subtlety and we are tempted to say of him—borrowing to some extent from what Nietzsche says of Machiavelli (*Beyond Good and Evil* #28)—that he derives a certain joy from the contrast between the quickness and lightness of his utterance and the gravity of his thought. For what other speaker in Thucydides, in a private capacity and to fellow countrymen, states so fully what we have called the Athenian thesis, making clear its applicability to individuals as well as cities; and what other speaker alerts his audience in advance to his intention to deceive them? We should resist the temptation to say that because of the seriousness of the occasion on which he speaks. The Athenians are debating for the second time the Mytilenian question, having decided the day before to punish the Mytilenians for their revolt by killing the men and enslaving the women and children. Diodotus supports the repeal of that decree, not, as he insists, on the ground of justice, but on that of "good counsel," of what is best for Athens (III, 44). His speech opens with a consideration of the complicated relation between the private good of the speakers who advise the Athenian assembly and that of the city advised: i.e., it opens with a reference to the problematical commonness of the common good of the city (III, 42–43). Its heart is an attack on the death penalty in general on the ground that it is in the nature of all, both privately and publicly, to err (or make mistakes) and that there is no law which can keep them from this (III, 45). It is perhaps true that Diodotus does not go so far as to draw the conclusion ascribed by us to the Athenians at Melos, but this is only because the circumstances prohibit the utterance of that conclusion, not because it does not belong to his thought. The Athenians—i.e., the Athenian *demos*, which did not necessarily share nor always permit itself to be guided by the enlightenment of some of its leaders—had passed their terrible decree against the Mytilenians under the influence of anger; they were indignant that the Mytilenians, who had not been treated badly by them, had nevertheless revolted. Their anger had been fired by two speeches of Cleon ("also in other respects the most violent of the citizens and by a large margin at that time the most trusted by the demos" [III, 36]). Cleon had spoken repeatedly of the "injustice" committed by the Mytilenians and had praised the anger which seeks swift revenge (III, 38 and 37–40 generally). Diodotus could not persuade the Athenians to repeal their decree and thus save the Mytilenians, if he did not first free them to some extent from their anger. He tries to do this by encouraging them to think rather of their interest than of the "wrong" which they suffered; the necessary compulsion of interest or advantage is to retrieve its place from the compulsion of anger and revenge, the compulsion which had rendered the Corcyraean civil war so terrible (Part Two above). Diodotus is able to free the Athenians somewhat from their anger because, in the first place, he never shared it; he does not share it because he does not blame the Mytilenians. Anger is the companion of lack of education and a narrow mind (or of folly) (III, 42). The thought of Diodotus leaves as little room for anger and indignation as that of Socrates.

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[4] The Athenians at Sparta perhaps themselves move in the indicated direction in I. 76: . . . ἀεὶ ϰαθεστῶτος τὸν ἥσσω ὑπὸ τοῦ δυνατωτέϱου ϰατείϱγεσθαι . . . (my underlining). Cf. the discussion of their speech in Part One above.

[5] Given my argument—see the preface—for the necessity of a naive—as opposed, for example, to a scholarly—starting point in the study of Thucydides, it may perhaps be helpful to point out that the differences between my conclusions and those of Jâcqueline de Romilly in *Thucydide et l'impérialisme athénien* (Paris: Budé, 1951, 2nd ed.) on the meaning of the opposition of right and force or compulsion —cf., for example, with Part Three above, de Romilly pp. 256–59, esp. 257, as well as 280–82 and 243–44—are not unrelated to the greater seriousness with which I take, and argue that Thucydides takes, the issue of justice, the issue of the justice of Athenian imperialism, in the first place (cf. de Romily, pp. 89–91).

[6] A. W. Gomme, on III. 41.

Diodotus' action is a typical Athenian action, or an action belonging to Athens at her best, such as we would expect also of Demosthenes and perhaps even of Themistocles. There is a definite hierarchy of men in Thucydides, and the placing of Diodotus among its heroes suggests, as it appears, nothing so much as the unity of enlightenment and gentleness. At the same time, the fact that he shares that eminence with Themistocles, the founder of the Athenian empire, and above all with Alcibiades, and the fact that the thought of Diodotus would not lead necessarily to the abandonment of empire, indicate the limits to that unity. It remains true, however that may be, that the thought ascribed to Diodotus is inseparable from a certain kind of right.

Alcibiades, more than anyone else in Thucydides, represents another side to Athenian greatness, the boundless striving for preeminence and immortal glory, a striving characteristic perhaps of Thucydides himself. It is fitting, then, that he should be the one to point to a connection between this striving, indifferent as it is to anything but its object, and the kind of right of which Diodotus has given evidence. Alcibiades maintains, as he had good reason to discover, that men are envious by nature, with the result that the eminent, while they live, cause pain especially to their like but also to others (VI, 16). Does not this pain obstruct our view of the excellent and therefore of human excellence itself? If this is so, then for this reason if not also others, only one who has striven successfully to be best would be able to assess every human type at its worth and this clear-sighted giving to every man his due—compatible as it is with the aims of practice and essential to that of theory—would be that genuine right of the stronger which is necessary to the lion himself, Thucydides.[7]

[7] An ancient commentator reports the saying of "some" who were surprised that the sober Thucydides went into such detail to explain a curse: "Here the lion laughed." (See the *scholia* collected by C. Hude, at about I. 126.)

Politics and the Career Mobility of Bureaucrats*

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Political scientists have long recognized that federal career civil servants, while prevented by the Hatch Acts from overt partisanship, are enmeshed in politics.[1] They initiate, defend and execute public policy, and thus exercise enormous political discretion.[2] This discretion is particularly evident in the United States where planned competition between the branches of government and a decentralized party system conspire against the development of clear policy direction. Public administrators consequently are thrust directly into a political arena involving interactions among administrative bureaus, interest groups, and legislative subcommittees.[3] Even if constitutional checks and balances did not exist and political parties were more "responsible," modern requirements for professional expertise in the management of public affairs undoubtedly would still force great discretionary power on public bureaucrats.[4]

Career civil servants traditionally have posed problems of control for noncareer political executives faced with implementing a president's policies. Career bureaucrats protected by civil service laws and rights,[5] with years of continuous public service in one policy field, are thought to have a great advantage in the policy process over secretaries, assistant secretaries and bureau chiefs who sometimes lack long experience in government and who, on average, remain in these policy-making positions only for short periods of time.[6]

Thus the careers of higher civil servants have been thought directly linked to the conduct of public affairs. Since the Second Hoover Commission in 1955, wide recognition has been given the political importance of career mobility.[7] In essence, the consensus of scholarly opinion since the time of the Hoover Commission presents two sides. One is an implicit empirical assertion that the American federal bureaucracy was (and is) a highly static structure which has discouraged mobility through its personnel system. A second is an evaluation of immobility in the administrative service which finds that immobility has produced political unrepresentativeness in administration,[8] bureaucratic stagnation,[9] the failure to promote excellence in the public service,[10] and even some of the disabilities of interest group liberalism.[11] The conclusion generally reached is that the public's business is best managed by a mobile rather than an immobile civil service and that civil service reform ought to enhance mobility rather than hinder it.

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[1] Some of the most recent refutations of the politics-administration dichotomy include: Louis Gawthrop, *Bureaucratic Behavior in the Executive Branch* (New York: The Free Press, 1969); Norman Powell *Responsible Public Bureaucracy in the United States* (Boston: Allyn and Bacon, Inc., 1967); Francis E. Rourke, *Bureaucracy, Politics, and Public Policy* (Boston: Little, Brown and Company, 1969). For an excellent discussion of administrative policy making in the American political environment, see: Norton E. Long, *The Polity* (Chicago: Rand McNally & Company, 1962).

[2] Rourke, pp. 50–55.

[3] A classic analysis of this triangular alliance is found in J. Lieper Freeman, *The Political Process: Executive Bureau-Legislative Committee Relations* (New York: Random House, 1955). See also, Harold Seidman, *Politics, Position and Power: The Dynamics of Federal Organization* (New York: Oxford University Press, 1970), chap. 2, "Executive Branch Organization: View from the Congress."

[4] Don K. Price, *The Scientific Estate* (New York: Oxford University Press, 1965).

[5] For a discussion of the protection given U. S. Civil servants, see: David H. Rosenbloom, "Some Political Implications of the Drift toward a Liberation of Federal Employees," *Public Administration Review*, 31 (July/August 1971), 420–426.

[6] Dean E. Mann with Jameson W. Doig, *The Assistant Secretaries: Problems and Processes of Appointment* (Washington, D.C.: The Brookings Institution, 1965), p. 7; David T. Stanley, Dean E. Mann, and Jameson W. Doig, *Men Who Govern: A Biographical Profile of Federal Political Executives* (Washington, D.C.; The Brookings Institution, 1967), chap. 4, "Tenure."

[7] Commission on Organization of the Executive Branch of the Government, Personnel and Civil Service, (Washington, D.C., February, 1965), Recommendation No. 6, and *Task Force Report on Personnel and Civil Service*, (Washington, D.C., February, 1955), chap. 3, "Strengthening Top Management: The Senior Civil Service." For a recent defense of the "Senior Civil Service" see Theodore J. Lowi, *The End of Liberalism* (New York: W. W. Norton and Company, 1969), pp. 299–305.

[8] See Paul P. Van Riper, *History of the United States Civil Service* (Evanston: Row, Peterson, 1958), pp. 549–59, for a discussion of the relationship of mobility to "representative bureaucracy."

[9] The problem of stagnation is discussed in John W. Macy, Jr., "Administrative Careers in the Public Service," in *Administrative Leadership in Government: Selected Papers*, ed. Don L. Bowen and Robert H. Pealy (Ann Arbor: The University of Michigan, Institute of Public Administration, "Papers in Public Administration," No. 32, 1959), pp. 40–49.

[10] Two pieces, in particular, stress the mobility

This paper examines the career patterns of federal higher civil servants[12] by developing a comparative measure of career mobility and linking these career patterns to a theory of political control. The popular view is that more mobility increases the control which presidents and members of their administrations have over civil servants and less mobility decreases that control. Put differently, highly mobile civil servants are more responsive to the wishes of their superiors while less mobile officials are less responsive. An examination of organization theory literature generally supports this view, although the normative conclusion that high mobility is good is subject to qualification.[13]

Mobility and Bureaucratic Control

Anthony Downs develops an argument best summarized as his "Law of Increasing Conservism": "In every bureau, there is an inherent pressure upon the vast majority of officials to become conservers in the long run."[14] In this view, bureaucracies experience life-cycles. At the beginning of the cycle, when the bureau is young, there is a lot of mobility in an expanding organization, promotions are frequent, and young, ambitious bureaucrats compete either to aggrandize their positions and themselves or move on to new opportunity. As bureaucrats and organizations get older, organizational expansion slows, and promotion possibilities shrink. Bureaucrats then concern themselves with maximizing security and convenience. They avoid risk, follow the written procedures to the letter, and resist change. In short, conserving officials become more difficult to control, particularly when innovative policy is called for. Thus immobility becomes, in Downs's view, an indicator of the bureaucratic stagnation:[15]

> The longer any official remains in a given position, the more likely he is to become a conserver. Long tenure in a given job may imply several things: the job-holder has little ability and therefore a low expectation of promotion; he is ineligible for further promotion because of some unchangeable factor; he is "over-due" for a promotion because he has marginal abilities—in which case he does not want to do anything that might "rock the boat"; or he is a fanatic zealot whose radical views have alienated his superiors.

Herbert Kaufman found, in his case study of the U.S. Forest Service, that mobility assisted in breaking down centrifugal forces of administration.[16] Forest rangers who were moved frequently from one area to another were prevented from "going native" (i.e., arbitrarily modifying headquarters policy to satisfy local interests); administrative fiefdoms which might have been established in field offices by immobile officials were thus dis-

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process as a positive contribution to career planning and development: John J. Corson and R. Shale Paul, *Men Near the Top* (Baltimore: The Johns Hopkins University Press, 1966), chapters V-VII; and Paul P. Van Riper. "The Senior Civil Service and the Career System," *Public Administration Review,* 18 (Summer, 1958), 189–200.

[11] Lowi, *End of Liberalism,* pp. 71–72, 304. In this view, government agencies are controlled by the relevant organized interests and not by any centralized authority exerting a national scheme of planning and administration.

[12] "Higher Civil Service" data are taken from: "United States Higher Civil Service," Principal Investigator David T. Stanley, The Brookings Institution, November, 1963, Inter-University Consortium for Political Research. The sample consists of 558 officials (one case was lost in this study) classified on the General Schedule at grades GS-15 to GS-18. A sample of 300 higher foreign affairs officials was randomly drawn from the 1966 *Biographic Register.* The sample consisted of officials who, as of 1966, worked for either Department of State, Agency for International Development, or U.S. Information Agency and who had attained a rank higher than or equal to GS-14, R-3. S-3, CR-3, or O-4. For an earlier discussion of the types of officials in the Biographic Register see: James L. McCamy and Allesandro Corrandini, "The People of the State Department and Foreign Service," *American Political Science Review,* 48 (December 1954), 1067–82. For a general discussion of the higher civil service. see David T. Stanley, *The Higher Civil Service* (Washington, D.C.: The Brookings Institution, 1964). A discussion of the "supergrade" civil servants is found in John J. Corson and R. Shale Paul, *Men Near the Top,* and Michael Cohen, "The Generalist and Organizational Mobility," *Public Administration Review,* 30 (September/October 1970), 544–552.

[13] While it seems reasonable that high immobility would lead to organizational stagnation—an undesirable condition, Oscar Grusky finds a negative correlation between rates of mobility and organizational effectiveness (i.e., the higher the rate of mobility, the lower the effectiveness): "Managerial Succession and Organizational Effectiveness," *The American Journal of Sociology,* 69 (July 1963), 21–31. A series of problems associated with high rates of mobility has also been reported in the British civil service, see: R. G. S. Brown, *The Administrative Process in Britian* (London: Methuen & Co., Ltd., 1970). pp. 53–56.

[14] Anthony Downs, *Inside Bureaucracy* (Boston: Little, Brown and Company, 1967), p. 99 (emphasis in original).

[15] *Ibid.,* p. 98.

[16] Herbert Kaufman, *The Forest Ranger: A Study in Administrative Behavior* (Baltimore: The Johns Hopkins University Press, 1960), pp. 155–156, 215–218.

couraged. Furthermore, the frequent transfer of officials from one office to another produced patterns of interactions which, in turn, yielded norms and values common to the whole service. Deviant thought and action were more easily disciplined and nonconformists were pressured to realign their thinking and behavior or leave the service.[17] Thus mobility enhanced the ability of senior Forest Service officials to control the behavior of their subordinates.

The Measurement of Mobility

Mobility has several dimensions which should be clearly specified. Merely classifying a civil service system as mobile or immobile ignores the various kinds of mobility which can exist. One can speak of *organizational mobility* (e.g., movement from one department, bureau, division, or branch to another), *occupational mobility* (e.g., movement from one job to another, such as a move from budget analyst to personnel specialist), *vertical mobility* (e.g., movement from one rank to another, which has sometimes been called "organizational mobility"[18]), and *geographical* mobility (e.g., movement from headquarters to field offices, or from one region to another). These are the essential kinds of mobility for any set of bureaucrats, public or private, American or foreign.

Furthermore, for any one kind of mobility—organizational, occupational, vertical, or geographical—two dimensions can be used to describe the career mobility of civil servants. One is the *amount,* or frequency, of movement. This can be defined as the turnover, or the number of "moves" a sample of bureaucrats makes during fixed time periods. The second is the *bias* of the movement, defined as the tendency of civil servants to leave or to enter some categories more than others.

Bias is a more difficult dimension to describe than the amount of mobility and it is only conceptualized here; the measurement is discussed elsewhere.[19] One way of approaching the problem is to compare "actual" mobility patterns with a pattern of "expected" or "perfect" mobility—that mobility which would have occurred had there been *no* association between an official's organizational, occupational, status (rank), or geographic category of origin on the one hand and his category of destination on the other.

For instance, if one were studying organizational mobility and observed that the organizational "moves" (either few or many) made by civil servants in a given time period involved equal numbers of transfers between two departments with equal numbers of personnel one would not be surprised; the actual mobility is exactly what would have been expected had the exchanges occurred merely by chance. If, on the other hand, the exchanges were "structured" so that small department A, with ten per cent of the total manpower, sent just as many officials to large department B, with forty per cent of the total manpower, as B sent to A, that would be a surprise. It can be said that more mobility bias exists in the latter than in the former example. If one calls the mobility bias (B) equal to the *absolute* difference between the actual number of "moves" (A) and the expected number of "moves" (E), then $B = |A - E|$, where B varies from zero—the unbiased situation—to higher numbers of "unexpected" moves as the mobility bias increases. As B gets larger, it seems reasonable to suppose that the careers of officials exhibit a high degree of structure. This is taken up again in the conclusion.

Because the main concern of the theoretical literature seems largely to have been with the amount rather than the bias of mobility, this paper focuses on the first dimension. To measure the amount of mobility we seek an index which has four properties: first, that it be simple; second, that it permit generalizations to be made about a universe of civil servants over time based on only a sample of officials, since it is not feasible to collect data for all higher federal civil servants at all times; third, that the index be a scale measuring degrees of mobility which can be correlated with other explanatory variables (e.g., background characteristics descriptive of the varieties of people in the civil service); fourth, that the index permit a comparative analysis of mobility with other samples of officials drawn at different times from other political systems.

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[17] These conclusions agree with research on group behavior. The classic discussion of interaction group theory is found in George C. Homans, *The Human Group* (New York: Harcourt, Brace, 1950); for a collection of research on the pressures toward conformity in groups, see Dorwin Cartwright and Alvin Zander, *Group Dynamics: Research and Theory,* 3rd ed. (New York: Harper & Row, 1968), pp. 139–211.

[18] For instance, see Robert Presthus, *Behavioral Approaches to Public Administration* (University of Alabama Press, 1965), p. 111.

[19] For an application of a transaction flow model to mobility data, see: Eugene B. McGregor, Jr., "Education and Career Mobility among Federal Administrators: Towards the Development of a Comparative Model" (Ph.D. dissertation, Syracuse University, 1969). For a discussion of the logic of a transaction flow model, see: Steven J. Brams, "Transaction Flows in the International System," *American Political Science Review,* 60 (December 1966), 880–898.

The Circulation Index

These conditions are met in the Circulation Index (CI) which is calculated from a matrix of moves any sample of officials makes over a period of time in which categories of origin and destination are distinguished. For the sake of illustration, the initial discussion is limited to organizational mobility, but the index can be applied to occupational, vertical, and geographical movement as well. Two kinds of sampling are used. The first is a sampling of officials in which civil servants are randomly drawn from a pool of administrative elites. The second is a sampling of time (again, for reasons of economy) in which the organization of an official is coded for fixed intervals of time. By arranging these "snapshots" in a series for each official in the sample we can generalize about the amount (and bias) of organizational movement.

For example, let us assume we wish to determine the amount of mobility which occurs among only three line departments—A, B, and C—from 1960 to 1968. Let us further assume that we draw a random sample of only 60 officials as the only feasible method of generalizing about a large universe of officials. If we record the organization of each official in the sample on an "every second year" basis beginning in 1960 (i.e., 1960, 1962, 1964, 1966 and 1968), then we have an economical way to describe the amount of mobility in our administrative system. For the sake of illustration, let us assume that these 60 civil servants began in their original departments A, B, and C in 1960 and arrived by various routes at their destination departments A, B, and C in 1968, and that no one retired during that time. For each of four consecutive pairs of origin-destination years (i.e., 1960 and 1962, 1962 and 1964, 1964 and 1966, and 1966 and 1968) we obtain a matrix of all the "moves" and "nonmoves." By summing the four matrices, a summary matrix of 240 "moves" and "nonmoves" for the eight year period is obtained. A hypothetical summary matrix is shown in Table 1.

Table 1. Hypothetical Summary of Organizational Mobility of Higher Civil Servants: 1960 to 1968 (N=60)

Department	*A*	*B*	*C*	Totals
A	50	10	10	70
B	30	80	20	130
C	6	4	30	40
Totals	86	94	60	240

As one can see from the diagonal cells—i.e., (A, A), (B, B) (C, C)—there was more immobility than mobility during the eight year period. This is reflected in the value of the circulation index (CI) which is calculated as the inverse of the proportion of immobility:

$$CI = 1 - \left[\frac{\sum D_{ii}}{T} \right], \quad \text{where} \quad \sum_{i=1}^{k} D_{ii}$$

is the sum of k diagonal cells (i.e., total number of "nonmoves") and T is the total number of "moves" and "nonmoves" in the symmetrical matrix. CI varies from 0, a case where the sum of the diagonal cells is 100 per cent of the total moves (i.e., no one leaves his original organization for any consecutive pair of years), to a maximum value of 1, an unlikely case where the sum of the diagonal cells is zero (i.e., for any consecutive pair of years no one remains in the same organization). A CI value of .50 indicates an equal amount of mobility and immobility. For Table 1, then, the value of CI is 1 − 160/240 = .33, which reflects the fact that there is considerably more immobility than mobility in this administrative system.

The question occurs, however: What is the meaning of a CI value such as .33? In the absence of measures of the statistical significance for such a number, two guides are handy. One guide is to compare the CI values for more than one set of officials. Indeed, CI is well suited to comparison, for the computation of CI discounts the size of the sample, the numbers of organizational categories used, and the number of pairs of years for which mobility is recorded. Given comparable samples of officials, comparable periods of time, and comparable organizational levels, one has a convenient summary statistic for comparing the amount of mobility in different administrative systems. A second guide is to contrive alternative values for CI and then compare these values with the "real" values obtained from sampling. Both of these approaches are discussed below in an examination of two samples of officials: David T. Stanley's sample of 558 "higher civil servants" and my sample of 300 higher "foreign affairs officials."[20]

In the case of the "higher civil servants" sample, the "every fifth year" technique was employed such that information about an official's organization was recorded every fifth year between 1946 and 1961 (i.e., 1946, 1951, 1956 and 1961) and for 1963 as well. For the sample of "foreign affairs personnel," the "every fourth year" rule of sampling was applied between 1946 and 1966 (i.e., 1946, 1950, 1954, 1958, 1962, and 1966). This, of course, leaves gaps of five years and four years, respectively, in which possible mobility is not coded.

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[20] See footnote 12.

Whether the gaps in the coding are so large that the careers of officials are misrepresented is difficult to tell. Coding large numbers of officials on an "every year" or "every two years" basis is very expensive and time consuming. What is missed when four and five year coding intervals are used is that group of officials who may be loaned to another department or agency for one or two years only to return to their original organizations within a single coding interval. But should this be a widespread mobility pattern among government officials, the present coding system ought to be sufficiently sensitive to discover the practice. Since the sampling of time occurs at regular and fixed intervals, it seems unlikely that there is a great amount of short-term movement which ceases only in the year when organizations are coded. We conclude that the sampling techniques employed here are sufficient to discern the crude patterns for which we are looking.

Table 2 shows the Index of Circulation values for the two sets of administrative elites. As can be seen, foreign affairs officials are more than five times as mobile as higher civil servants, who very rarely move outside a single department or agency. Much of this foreign service mobility is undoubtedly due to the tendency of Foreign Service Officers in the Department of State to be loaned for short tours to other agencies such as AID and USIA after which they return to State for reassignment. In this coding system, the pattern of loaning out officials is regarded as organizational mobility even though there has been no permanent change of organization. What is interesting, though, is that this loan practice is conspicuous by its general absence among higher civil servants.

For the higher civil service it is true that older officials are slightly less mobile than are younger civil servants. This also seems reflected in the finding that there was twice as much mobility in the period 1936 to 1956, when higher civil servants in 1963 were young, than in the period 1946 to 1963, although this earlier period of time was also an era of great expansion and change—caused by attempts to deal with an economic depression and fight a major war—which placed great demands on a limited number of personnel. These findings suggest that as "successful" civil servants rise to their highest levels of power and influence they become more anchored to a single agency or department. Whether or not these officials become more "conserving" as they become older and less mobile cannot be determined from these data.

Such a pattern does not obtain for foreign affairs officials. If anything, older foreign affairs officials are slightly more mobile than younger officials, although, paradoxically, lower-ranking officials are more mobile than higher-ranking officials. However, those foreign affairs officials (there are no comparable data for the higher civil service) who have received extensive high level, mid-career training are found to be nearly half as mobile as those officials who have not received such training. The significance of this finding is worth noting, since mid-career trained officials represent an elite chosen for at least a year of graduate work intended to "broaden" their future managerial capacities. Such programs include: the National War College, the Industrial College,[21] the Senior Seminar in Foreign Policy, and University programs for advanced training in economic, political, and international affairs and "area studies." Inasmuch as these officials are chosen for "grooming" by their sponsoring organizations for high-level work when they return, it is clear that such mid-career programs are investments in immobility rather than investments in a highly mobile executive service.

A second way to indicate the political significance of the Circulation Index is to contrive alternative values which describe the mobility of other political elites. By calculating what the CI value would be if bureaucrats served fixed terms of office, we have a standard against which real mobility can be compared. For instance, if bureaucrats serve four year terms of office (like presidents and two-term representatives) after which they move to a

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[21] Not many officials do, in fact, attend either the National War College or the Industrial College which are run under the *aegis* of the Department of Defense. For a discussion of these two schools, see: John W. Masland and Lawrence I. Radway, *Soldiers and Scholars: Military Education and National Policy* (Princeton University Press, 1957), Part 5, "Senior Military Education."

Table 2. Index of Circulation Values for Organizational Mobility of Federal Administrative Elites

		Higher Civil Service ($N=557$)	Foreign Affairs Officials ($N=300$)
Variables	Samples	1963-Federal System[a]	1966-Federal System[b]
	Total Sample: 1946 to 1963	.037 ($N=557$)	.208 1946 to 1966 ($N=300$)
	Total Sample, 1936 to 1956	.073 ($N=557$)	x
Age	Older Officials (50 years old or older)	.031 ($N=332$)	.213 ($N=131$)
	Younger Officials (49 years old or younger)	.053 ($N=225$)	.205 ($N=169$)
Education	Mid-career Educated Officials	x	.126 ($N=68$)
	Non-mid-career Educated Officials	x	.236 ($N=232$)
	Officials having formal education beyond the bachelor's degree	.044 ($N=252$)	.216 ($N=157$)
	Officials having a bachelor's degree or less	.034 ($N=302$)	.200 ($N=143$)
	Social science and humanities majors	.054 ($N=235$)	.206 ($N=133$)
	Physical and life sciences majors	.034 ($N=222$)	.211 ($N=167$)
Rank	Higher ranking officials in 1963 (GS-16, O-2, S-2, R-2, CR-2 and above)	.053 ($N=228$)	.218 1966 ($N=142$)
	Lower ranking officials in 1963 (GS-15, O-3, S-3, R-3, CR-3 and below)	.059 ($N=263$)	.314 1966 ($N=158$)

[a] The "federal system" for "higher civil servants" consists of eighteen departments and agencies: the Departments of Defense, Agriculture, Commerce, Interior, Justice, Labor, States, Post Office, Treasury and Health, Education and Welfare; Agency for International Development (formerly the Economic Cooperation Administration), Atomic Energy Commission, Bureau of the Budget, Civil Service Commission, Federal Aeronautics Administration, National Science Foundation, U.S. Tariff Commission, and U.S. Information Agency.

[b] The "federal system" for the sample of foreign affairs personnel includes the above departments plus AID, USIA, Foreign Agricultural Service, "military service," and a miscellaneous "other federal" category.

Note: The dichotomous subsamples sometimes total less than the sample size where an unusable code or blank existed in the data decks and the case was excluded from analysis.

job in another organization, the value of CI becomes .25. This value obtains from several ways of calculating CI: (1) if sampling is done every year for n years and only 25 per cent change their offices per year; (2) if the "every fourth year" technique is employed and terms of office are staggered so that only 25 per cent change their organizations each year, or if all officials move together only once in each four year period; (3) if the "every nth year" technique is employed for a period of several years and any of the above conditions obtain or the "average" term in office is four years (i.e., as many officials serve five year terms as three year terms, six year terms as two year terms, etc.), in which case a CI value of .25 is the best *single* approximation of the terms in office.

Similarly, we can compute several values of CI$_n$, in which n denotes the term of office in years, as follows:

$$CI_1 = 1 - 0 = 1$$
$$CI_2 = 1 - .50 = .50$$
$$CI_4 = 1 - .75 = .25$$
$$CI_8 = 1 - .875 = .125$$
$$CI_{16} = 1 - .937 = .063$$

The relationship between CI values and the term of office is shown in Figure 1. The curve slopes downward to the right and approaches a CI value of 0.

The most striking thing about Figure 1 is the finding that a CI of .037 for higher civil servants is nearly equivalent to a twenty-five year term in one organization. Since the sampling period only extends seventeen years, there is some chance that the sample cannot be a basis for generalizing beyond sixteen to seventeen years. But the time period is long enough to indicate that only a rare "successful" higher civil servant makes more than one interdepartmental or interagency move in a lifetime of public service and that this move is most likely to occur early in a man's career.

The political significance in this finding is seen in the mobility of other political elites. In the executive branch, higher civil servants remain within one organization more than twice as long as do re-elected presidents and at least eight times as long as most assistant secretaries.[22] To the extent that the length of time spent in one organization increases the amount of control which can be exerted on the management of an agency, then assistant secretaries, and thus the President, must be at a competitive disadvantage in the struggle to provide policy direction to departments and agencies.[23]

Federal legislators, on the other hand, show much lower turnover rates than do "political" executives. Samuel P. Huntington finds a marked trend in the direction of both representatives and senators being elected more than once to their respective houses and a concomitant increase in the importance of seniority in the Congress.[24] While one can decry the lack of "new blood" in the Congress, it should also be noted that the longer tenure of legislators probably permits a closer, albeit piecemeal, surveillance of the bureaucracy from the congressional committee system than one finds from most political executives.

Conclusions

This discussion has been concerned with the conceptualization, measurement, and political significance of the career mobility of American federal bureaucrats. Data limitations prevent a cross-national comparison of the careers of civil servants. However, conceptualizing career mobility in terms of the amount and bias of various kinds (organizational, occupational, etc.) of mobility offers a typology depicted in Figure 2.

The significance of such a scheme is that it potentially contributes to a comparative analysis of civil service systems, for the type of mobility pattern may really be a manifestation of other patterns of bureaucratic control. Systems characterized by a high amount of structured (i.e., biased) mobility (cell 1,1) among

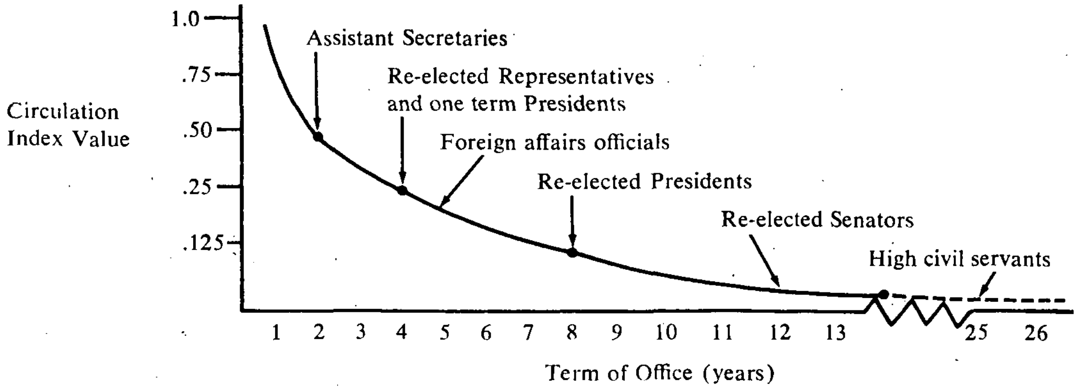

Figure 1. Relationship Between CI Values and Term of Office

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[22] Mann, *Assistant Secretaries,* pp. 227–231.

[23] See footnote 6.

[24] Samuel P. Huntington, "Congressional Responses to the Twentieth Century," in *The Congress and America's Future,* David B. Truman, ed. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1965), pp. 8–11; evidence for a surprising amount of congressional activity in policy making—particularly regulatory policy—can be found in: Theodore J. Lowi, "Four Systems of Policy, Politics, and Choice," *Public Administration Review,* 32 (July/August 1972), 298–310.

Figure 2. Typology of Bureaucratic Elite Career Mobility

	AMOUNT OF MOBILITY	High	Low
BIAS OF MOBILITY	High	High amount of structured mobility 1,1	Low amount of structured mobility 1,2
	Low	High amount of unstructured mobility 2,1	Low amount of unstructured mobility 2,2

the administrative elites are systems that probably have centralized mechanisms for bureaucratic coordination and control which enforce such a pattern. American foreign affairs elites may be characterized by such a pattern. The British Administrative Class, by reputation, would fall into that category with a transdepartmental system of control maintained by the permanent secretaries and concentrated in the Treasury and newly created Civil Service Department. Furthermore there may be a high degree of structure in the movement. For instance, British Treasury officials have long been recognized as an elite within an elite; officials may leave Treasury for assignments in other departments, but only the few best officials in other departments are likely to move to Treasury.[25] Cell 1,1 may be typical of the Indian higher civil services and several continental European services as well, particularly France and Germany, although the French *grands corps* and the German apportionment of its *Beamtenkorps* among the *länder* must surely serve as barriers to career movement.

Cell 2,2, by contrast, shows just the opposite pattern in a low amount of unstructured mobility which seems most in evidence when a civil service system lacks centralized mechanisms for control. Low, unbiased transfers of personnel occur in instances where constituent bureaucratic units are relatively successful in guarding their autonomy; the impermeability of organizational boundaries is testimony to the degree of autonomy which has been won. The American civil service system seems characterized by such a pattern. Some preliminary evidence suggests that the Italian case may be similar as well.[26]

However, observing an unusually low rate of organizational mobility among American federal departments and agencies raises more questions than are answered. At what level of administration does the amount of circulation rise to the point where one can conclude that a centralized and coordinating authority is being exerted? Would this occur among the bureaus within line departments? Or does one have to dig deeper to find stable units of bureaucratic power?

While mobility data are presently lacking, there is some utility in hypothesizing about patterns of bureaucratic power. Several political scientists, for instance, suggest that federal departments are merely "holding companies" for bureaus operating from an autonomy bolstered by congressional authorization and appropriation committees and interest groups.[27] Thus, organizations such as Federal Bureau of Investigation (Justice), Bureau of Indian Affairs (Interior), Social Security Administration (HEW), Federal Aviation Administration (Transportation), Army Corps of Engineers (Defense), Internal Revenue Service (Treasury), Bureau of Labor Statistics (Labor), Bureau of the Census (Commerce), and the Forest Service (Agriculture) really represent major units of power whose political constituencies contribute to bureau independence.

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[25] See, for instance: Samuel H. Beer, *Treasury Control* (New York: Clarendon Press, 1957); Sir Edward Bridges, *The Treasury* (New York: Oxford University Press, 1964). The reforms under which the Civil Service Department was created are discussed in *Report of the Committee on the Civil Service to Parliament,* Lord Fulton, chairman (London: Her Majesty's Stationery Office, 1968). The appointment of Sir William Armstrong as head of the Home Civil Service including the Civil Service Department, is illustrative of the kind of structured mobility we mean, since he formerly held the position of Joint Permanent Secretary to the Treasury. Caution must be exercised in comparing higher American civil servants with senior civil servants in Great Britain whose counterparts in the United States often are "in-and-outers" not contained in either sample of officials examined in this study; see: Richard E. Neustadt, "White House and Whitehall," *The Public Interest,* 2 (Winter 1966), 55–69.

[26] Robert D. Putnam, "The Political Attitudes of Senior Civil Servants in Western Europe: A Preliminary Report," paper delivered at the 1972 Annual Meeting of the American Political Science Association, Washington Hilton Hotel, Washington, D.C., September 5–9.

[27] For a general formulation of these relationships, see Freeman, *Political Process.* Also, see Arthur Maass, *Muddy Waters: The Army Engineers and the Nation's Rivers* (Cambridge, Mass.: Harvard University Press, 1951); Seidman, *Politics, Position and Power,* chapter 2, "Executive Branch Organization: View from the Congress;" Richard F. Fenno, *The Power of the Purse: Appropriations Politics in Congress* (Boston: Little, Brown, 1966), pp. 264–413, 564–615.

There is some evidence that career mobility patterns reinforce this bureau autonomy by restricting civil service careers to a series of coterminous professional groupings and administrative-political boundaries. Whole bureaus are often dominated by one or two professional specialties, some of which are indicated below.[28]

Federal Bureaus:	*Specialty:*
Army Corps of Engineers	Civil Engineers
Forest Service	Foresters
Bureau of Reclamation	Civil Engineers
Geological Survey	Geologists
Office of Education	Educators
Federal Bureau of Investigation	Lawyers and Accountants
Federal Aviation Administration	Air Traffic Controllers
National Bureau of Standards	Natural Scientists
Public Health Services	Medical Doctors
Bureau of Labor Statistics	Economists

The establishment of the political autonomy of bureaus is further reinforced by separate personnel systems managed by the bureaus themselves and not by either their own departments or the Civil Service Commission. While the Civil Service Commission does preside over a general schedule of "classified" federal employees covering most of the civilian federal service, several career corps are to be found at the bureau level. The FBI, the Forest Service, the civilian personnel of the Army Corps of Engineers, the Geological Survey, the National Park Service, and the commissioned corps of the Public Health Service are all examples of such career systems.[29]

In general, federal personnel administration is founded on an open-career, position classification system in which qualified people can be hired for specialized positions at nearly any level of the federal service. Bureaus, such as the ones indicated, are often found to contain closed-career services into which are recruited new members only at a young (usually school-leaving) age. Promotions then occur only within the service, and in some instances (e.g., Forest Service ranger) a special service rank is conferred on the service member which he may carry from job to job without reclassification. Such services also typically restrict irregular (e.g., lateral) entry and relegate the control of entry and promotion to the "senior" bureau members.

One would suppose that high mobility, such as frequent lateral transfers of officials across organizational boundaries, would break down bureau autonomy. Low mobility, on the other hand, provides time for the process of interaction to manifest itself in a bureaucratic "culture" of binding norms and values. For example, in the Forest Service the bureau is the major mobility barrier. Mobility is high *within* the bureau, but there are few lateral transfers *between* the Forest Service and other bureaus. The result of the pattern is clear, as Herbert Kaufman's observations demonstrate:[30]

> . . . forest officers are exposed for substantial periods to the environment of the agency before they are appointed to executive positions, and they remain always under the surveillance of men who have spent practically their whole adult lives in Forest Service employ. They are absorbed into the organization by a kind of gradual social osmosis, during which they, in turn, absorb many of the prevailing values, assumptions, and customary modes of operation.

The conclusion we reach is that the careers of high level civil servants, and maybe the careers of most of the federal service, are probably truncated at the bureau level of administration. Whether this pattern can be regarded as healthy or not seems largely a function of one's political perspective. In the past, presidents have been unhappy with the system and Congress, in its silence, seems to have found some comfort in the presidents' discomfort.

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[28] Most of these are taken from Frederick C. Mosher, *Democracy and the Public Service* (New York: Oxford University Press, 1968), p. 105.

[29] I am indebted to David T. Stanley for pointing out several of these examples to me.

[30] Kaufman, *The Forest Ranger,* pp. 181–182.

Changing the Rules Changes the Game: Party Reform and the 1972 California Delegation to the Democratic National Convention

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During the 1968 Democratic National Convention at Chicago, while the attention of the press and public was focused on the chaos on the floor and in the streets, some institutional tinkering took place which, in retrospect, may well be the most significant action taken by that body. A minority of the Rules Committee of that convention had brought to the floor a series of proposals to "open up" the selection of delegates to future Democratic Conventions.[1] These proposals, designed in part for the purely political purpose of dividing those delegates leaning toward the nomination of then Vice-President Hubert Humphrey, suggested sufficiently archaic delegate selection procedures in some states to provide a brief coalition for reform.[2] Ultimately the full convention did pass a proposal which required that delegates to the 1972 and subsequent conventions be selected under a set of rules quite different from those operative to that point. Specifically, the following mandatory resolution was adopted:

> It is understood that a state Democratic Party, in selecting and certifying delegates to the National Convention, thereby undertakes to assure that such delegates have been selected through a process in which all Democratic voters have had full and timely opportunity to participate . . . [further] the convention shall require that: #1) The unit rules not be used in any states of the delegate selection process; and #2) All feasible efforts have been made to assure that delegates are selected through party primary, convention or committee procedure open to public participation within the calendar year of the National Convention.[3]

This mandate, as interpreted and implemented by the Democratic National Committee's Commission on Delegate Selection and Party Reform, has had a major impact on what Polsby and Wildavsky have called the "strategic environment" of our political landscape.

> All political strategies are worked out within a framework of circumstances which are in part subject to manipulation but to an even greater degree are "given" . . . Some of these circumstances are contingent and relate to the strategies being pursued by other active participants in the election process and to the resources at their command. Other circumstances are more stable and have to do with features of the American political system that have persisted over time.[4]

This article is an effort to assess some of the effects on the strategic environment of the Democratic Party's new rules on the selection of delegates to the National Convention. The events described are limited to the experience of California,[5] but it is assumed that some of the generalizations derived from that case will prove to have a more general applicability.[6] The obvious difficulty with any effort of this type is to distinguish between those effects which are relatively stable and those primarily related to the specific configuration of events that occurred in 1972. If this problem can be surmounted, it should be possible not only to make some general statements about the impact of those changes on our national politics, but also, through an examination of the decision-making processes undergone by candidates and their campaign organizations faced with these new situations, to begin to understand the problems associated with transforming a set of ideas into a working reality.

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[1] These proposals were the product of a study made by a group organized by a liberal Democratic faction, the Commission on the Democratic Selection of Presidential Nominees, Harold Hughes, Chairman. The report of that Commission, *The Democratic Choice* (Washington, D.C., 1968), included a series of recommendations, some of which were later adopted in modified form by the 1968 National Convention.

[2] In Georgia, for example, all delegates were simply appointed by the State Chairman of the party, who in turn was appointed by the governor: *Ibid*, page 75. The fact that most of those involved in the Hughes Commission recommendations had their eye primarily on the 1968 Convention (rather than 1972) has been well documented. Theodore White, *The Making of the President, 1968* (New York: Atheneum, 1969); pp. 273ff; Arthur Herzog, *McCarthy for President* (New York: Viking Press, 1969), pp. 242–7; Lewis Chester, Godfrey Hodgson, and Bruce Page, *An American Melodrama,* (New York: Viking Press, 1969), pp. 551–7. At the Convention, after the Challenges to delegations chosen under the old rules had been safely put away for 1968, the recommendations described supra passed almost as an afterthought: 1972 was a long time off.

[3] Commission on Party Structure and Delegate Selection, *Mandate for Reform* (Washington, D.C.: Democratic National Committee, 1970), pp. 14–15.

[4] Nelson W. Polsby and Aaron B. Wildavsky, *Presidential Elections,* 3rd ed. (New York: Scribners, 1971), p. 7.

[5] Material for this article is based on a large number of private conversations, personal observations, and formal interviews gathered during a period extending from May of 1971 through November, 1972. During this period I spoke on matters connected with the subject of this article to more than 100 of the relevant actors: members of the California Commission on Party Reform, campaign staff members of the McGovern, Lindsay, Muskie, McCarthy, and Humphrey campaigns, state legislators, party officials, party financial contributors and members of the eventual winning slate of delegates as well as participatnts in the delegate selection meetings of the various campaigns. An effort was made to cover a broad spectrum of views, and to speak with participants from every part of the

New Rules

In 1972 California sent 271 delegates to the Democatic National Convention pledged to the winner of the June 6th primary. The sheer number of delegates, more than 17 per cent of those needed to nominate, along with the winner-take-all feature of the California primary, made it a strategically significant state for any serious candidate. The procedure for entering this primary remained unchanged from past years. Each candidate was required to file with the Secretary of State a slate of delegates pledged to the candidate and distributed by number equally among the state's 43 congressional districts. Then, within a specified period of time, each candidate had to file a minimum of 14,693 signatures of registered voters pledged to the support of his slate of delegates. Those slates which received the required number of valid signatures were then placed on the June ballot.[7] The candidate whose slate won the highest number of votes in that election would receive the right to send all 271 of California's delegates to the National Convention.[8]

The changes in party rules had no direct impact on the procedures described above. Rather the changes were reflected in the procedures for choosing the various slates of delegates themselves. Prior to 1972, delegates had been selected by an executive committee chosen by the candidate himself. The practice had been for this committee to ask a handful of their most prominent supporters in each district to recommend the delegates from that district.[9] This practice traditionally resulted in delegations representative of the elite corps of the Democratic party: legislators, top party officials, and major contributors. In almost every case the criterion for choosing delegates was simply to select that individual who had shown through his past campaign record or other means that he would provide the greatest aid to the campaign in his district.[10] Those responsible for selecting the delegates saw their role as primarily that of putting together a campaign

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state and from every ideological position within the party. The interviews themselves were unstructured and openended.

Access to the private opinions of many of these actors was facilitated by my own role in the events of 1972. Through August of that year I served as assistant to the manager of the campaign for Senator George McGovern in the 50 counties of Northern California. In this role I had responsibilities which extended from the organization of the McGovern delegate selection meetings through the National Convention itself. Most of what is described herein happened in my presence. Much of the material on which this article is based stems from a day-by-day record which I kept of the events and conversations that took place during the campaign.

As a participant-observer I was able to gather information about the motives and deeds of some actors not ordinarily available to researchers. At the same time, because the relevant actors gave much of the information contained here to me in my role as participant, ethical problems would be raised were I to reveal specific sources. Many actors do not wish to be publicly associated with statements they are willing to make privately. Others have made clear their unwillingness to be publicly associated with some of the implications and conclusions of this article.

[6] In their specific consequences, the new rules varied greatly in impact from state to state. In states where delegates were chosen through a convention system (Iowa, for example), the mandate for open participation made possible a situation where a candidate with substantial elite support but a small popular base could win a large share of the delegation. The new rules directly affected the question of who the nominee was to be in states with this type of system. In California, a closed primary state, the effects were less direct; their influence dealt primarily with the ability of the party to garner campaign support for the party's nominee. We have, as yet, no study which would allow us to gauge the full national impact which the rules had on the question of actually determining the party's nominee.

[7] White voters received copies of the various slates with their sample ballots in the mail, at the polls the ballot simply says vote for "delegates pledged to. . . ."

[8] The winner-take-all provisions of the California primary came under strong attack at the 1972 Convention. While the victorious slate was seated at that convention, a rule was adopted that would eliminate this provision in subsequent conventions.

[9] Eugene C. Lee, "The California Democratic Delegation of 1960" in *Case Studies In American Government,* ed. E. A. Bock (Englewood Cliffs, N.J.: Prentice-Hall, 1962), p. 143.

[10] In 1964, when Edmund Brown, who was then governor, led a united party slate in support of Lyndon Johnson's nomination, virtually every delegate chosen was either a contributor, fund-raiser,

team. Political criteria were virtually the only relevant ones involved in their decisions.

As one consequence of this method of selection, most women, minority group members, and young people were simply eliminated from serious consideration at the outset of the process. The vast majority of party officials, campaign chairmen, major contributors, and elected representatives were white, male, and more than 30 years old. In many cases to place a young person on a delegation might involve the loss of support from a prominent legislator or contributor who felt himself bypassed. There were few campaign committees willing to risk this type of loss for a symbolic gesture. It was against this system of selection and the types of delegates chosen under this system that the new rules were primarily directed.

In the Spring of 1971 the California Commission on Delegate Selection met to recommend changes in delegate selection procedures that would bring the state into compliance with the guidelines laid down by the National Reform Commission. The new rules adopted by the state party required that each candidate's campaign committee appoint a representative to a statewide steering committee from every congressional district. This person would be given the responsibility of locating and publicizing a meeting place for the delegate selection caucus for his candidate in that district. On February 12th, the day set by the new rules for the selection of delegates,[11] the organization of every candidate would hold a public meeting open to every registered Democrat. The meetings would be held at the same time, but in a different location with the congressional district for each candidate. At that meeting any person could be nominated to fill one of the delegate slots alloted to that district (each district had between six and nine delegate slots, the number depending on whether or not the nominee of the party had carried that district in the 1968 general election). Each caucus would be permitted to nominate up to twice the number of alloted delegates and alternates for that district. Then, within 10 days, the steering committee representatives chosen by the cam-

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elected official, party official, campaign chairman, or (with a handful of remarkable exceptions, if female), the wife of a member of one of these categories. Perhaps a more striking example of this phenomenon is to be found in the insurgent California Delegation of 1968. In that year California Attorney General Thomas Lynch had filed a favorite-son delegation pledged to the renomination of Lyndon Johnson. Most of California's elected officials, labor leaders, etc. had already pledged to serve on this slate when Robert Kennedy announced for the presidency on March 13th—only 20 days before a slate had to be filed in the state. It might be reasonable to expect then, that the Kennedy delegation would have been somewhat less traditional in its composition. Yet,

95 per cent were over 30
80 per cent were male
89 per cent were white

—13 per cent were congressmen, state legislators, or the wives of such officials
—28 per cent were identifiable major contributors to the California Democratic party, or the wife of a major contributor
—27 per cent were identified by the campaign press release as party officials, local government officials, campaign officials, or long time party activists
—6 per cent were labor union officials

In all, 130 of the 174 delegates, or 75 per cent of the total delegation, fell into these four categories. Of the 44 delegates who did not fall into these groups, four were celebrities, eight were college or university professors, twelve were students (and three of these were state officials of the Young Democrats), and twenty were identified only as "housewife." Excluding those men identified by a public, party or campaign office (and their wives), 33 per cent of the delegates were identified as being either attorneys or businessmen.

[11] One of the most important albeit indirect consequences of the new rules involved the early date required by those rules for the district meetings. The simple mechanics of verifying the approximately 15,000 signatures which each campaign was required by law to file with their slate of delegates (more than 1,200,000 such signatures were filed by the various campaigns in 1972) is a laborious process requiring many weeks. The process of verifying, printing, and distributing sample ballots to all of California's ten million voters requires another 30 days. These two mechanical deadlines, embodied in law, meant that the final date for securing and submitting the 15,000 signatures in 1972 was March 23. In order to provide each campaign organization with an equal and ample period in which to circulate their petitions, no candidate was allowed to collect signatures prior to February 22nd, the final date for submitting the names of the delegates selected for each candidate. Since the delegate selection meetings could not be held on a weekday, this meant setting their date on the weekend either of February 12th or the 19th. The latter was chosen by the legislature to provide the time necessary for printing the petitions with the name of each delegate.

It is interesting to note that under these new deadlines, Robert Kennedy would not have qualified as a candidate in 1968, and George Wallace was unable to qualify in 1972. Under the old deadline (prior to the caucus procedure for delegate selection), which was April 6th, George McGovern would have been a quite different candidate than he was on February 12th. (Muskie had by then suffered a close call in New Hampshire and two defeats, Florida and Wisconsin); Lindsay was out of the race—as was McCarthy. The California campaign was, by then, shaping into a McGovern-Humphrey race. None of these events were easily forseen two months earlier.

paign committee would meet to choose 88 per cent of the statewide slate which ultimately would run in the June primary pledged to their candidate.[12]

The final slate of delegates and alternates chosen by the statewide steering committee had to be in compliance with the new guidelines that were designed to increase the numbers of some groups heretofore underrepresented in the Democratic National Convention.[13] While no specific numbers were laid down, it was obvious that black and brown Democrats would have to be represented in numbers no less than their proportion in the general population, that people 18–30 would have to be represented in numbers at least close to their strength in the state as a whole, and that the number of women would have to be dramatically increased over that of past years.[14]

Old Players and the New Rules

In any assessment of the impact of the new delegate selection rules on the Democratic Party, it is important to remember that the implementation of those rules remained the responsibility of the party's elite. The party in California was reformed from above, not from below.[15] Therefore it becomes important to try to understand how the leaders of the party perceived what was expected under the new rules, what they hoped to achieve, and how they set about trying to achieve those goals. Members of the California Reform Commission began their task with agreement on the premise that any slate of delegates had to be viewed first and foremost as a set of political resources. They saw in the new rules an opportunity to provide candidates with the ability to make a more effective use of the resources available to them.

The traditional method of selecting delegates described earlier had a series of important benefits, some of which will be described at a later point. The members of the California Reform Commission, however, were less concerned with such benefits than they were with the liabilities which had grown up around that system over the years. They saw in the changes of the rules a possible means of removing some of those liabilities. Specifically, they hoped to restore some of the discretion that had been chipped away from the candidate's executive committee by traditional practices.

Major contributors, for example, had originally been rewarded with a place on a candidate's delegation in return for their financial help in the primary campaign. But over the years, more and more of these contributors had come to view this reward as a right rather than as an earned privilege. More and more of them declined to contribute substantial amounts until the general election—and then only if they had received their reward in advance in the form of a convention seat.[16] Similar situations prevailed in the case of elected officials. They expected a place on the delegation of the candidate they endorsed. While often the support of such an official meant little more than the use of his name, to refuse him his position could mean, because of his ties with local party activists, difficulties in mounting an effective campaign effort in his district.[17]

The traditional system had evolved to the point where a great number of those selected as delegates felt that it was their right to be so selected. They felt little gratitude or obligation to the campaign organization. Moreover, because there were always more contributors and legislators than available positions on a delegation, many people who viewed those positions as their "right" had to be bypassed. Some of them were bound to become righteously resentful. Further, those people who had put in the long hard hours at the local headquarters simply had to resign themselves to the fact that there was virtually no chance of their being elevated to the status of delegate. The only real decision left to the campaign's slatemaking committee was that of determining whom they could least afford to offend.[18]

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[12] The remaining 12 per cent were to be selected after the primary by the winning slate.

[13] The Democratic party's Commission on Party Structure and Delegate Selection reached the issue of group representation by a simple interpretation of their mandate on participation: "The Commission believes that discrimination on the grounds of age or sex . . . (or) race, creed, color, or national origin . . . is inconsistent with full and meaningful opportunity to participate in the delegate selection process," *Mandate for Reform*, pp. 39–40. To remedy this wrong, the Commission required that state parties take "affirmative steps to encourage representation . . . of young people . . . minority groups . . . and women in reasonable relationship to their presence in the population of the State." *Ibid.*, p. 40.

[14] While the Commission specifically noted that its guidelines were "not to be accomplished by the mandatory imposition of quotas," the only alternative appeared to be "voluntary" imposition of quotas by the various campaign committees, *Ibid.*, 40, fn. 2.

[15] This was, in fact, the case with the Party nationally as well. The convention which initially adopted these rules, it should be remembered, was not one dominated by reformers. Although the implementing commissions themselves were chaired by liberal reform Democrats (Senator George McGovern, Rep. Donald Fraser), the recommendations of their commissions had to be accepted by both the Democratic National Committee under the leadership of Lawrence O'Brian and by the various state party organizations.

[16] As an example, during the period in late 1971 when Senator Muskie still appeared certain to win the Democratic nomination, several of the largest contributors in Northern California met in San Francisco and agreed to withhold any substantial donations until after the Convention in July. All felt confident that a convention seat was theirs for the asking regardless of this pact. As one man put it, "Muskie knows he'll have to come to us eventually—no matter what we haven't done to that point."

[17] Every campaign activist interviewed who had been involved in choosing delegates in past years agreed that with four or five major exceptions, legislators were more often selected to avoid negative sanctions than to make any positive gain in resources for the campaign. As one put it, "Four legislative endorsements equals one printed press release, and that's the sum total of the benefits they get you."

The participants of the Reform Commission in California—many of whom had been involved in the slatemaking efforts of previous years—hoped that the combination of changes (the de facto quota requirements and the mandate to slatemakers that consideration must be given to nominees selected at district meetings) would restore some of the campaign advantages lost under the old system. It may appear odd that changes designed to limit the ability of the campaign's slatemaking committee to select whomever they wished were viewed by precisely those decision makers as the path toward greater flexibility. But as we have seen, the abstract freedom to choose under the old system meant in reality being forced to choose from a limited set of players. The new rules appeared to restrict the ability of the slatemakers to select exclusively from among that set.

Under these rules the slatemakers would operate in a situation similar to what Schelling has described as a "coercive deficiency." They would no longer have the authority necessary to comply with the demands placed on them by the traditional players. As Schelling notes, in such a situation the burden of avoiding mutual danger (in this case, loss of resources essential to the campaign) shifts to the other party. In our specific situation, this would mean that legislators or contributors could be refused a place on a delegation in a way which would make it less legitimate for them to hinder the campaign by withholding their resources.[19] Thus by limiting the ability of the slatemakers to choose from the traditional group of politicos, the new rules were designed to give each candidate new opportunities to reward persons outside those more traditional categories.

The major expectation of the Reform Commission and of those involved in the actual slatemaking operation in 1972 and in the past was that the new rules would promote some of the lower echelon campaign people to positions on a delegation. The quota requirements were thought to be especially useful in this regard because the majority of those bypassed in previous years were women. In addition, the demands by liberal activists for greater representation on delegations was viewed as legitimate by most of these campaign professionals—a judgment that was motivated both by a concern for equity and by some hard pragmatic thinking. One of the primary purposes of those involved in the implementation of the new rules in California (as elsewhere) was to rekindle the enthusiasm of the party's liberal wing. The quotas and the caucus system seemed like a small price to pay at the time if the benefits included a party united behind its nominee in the fall.[20]

Thus by the day of the delegate selection meetings, virtually all the important participants in every campaign were agreed that their delegations would look quite different in 1972. Few, however expected that this difference might be counterproductive in their campaigns, because several safeguards were included in this process of reform. For example, the slatemaking committee that actually made the final selection of delegates was not required by the new rules to pick exclusively (or even at all) from the list of persons nominated in district meetings.[21] Under the new rules, any actions by those district meetings were advisory in nature. Further, the quotas imposed by the new rules were thought to be neither rigid nor dramatically high: the relevant groups had to be represented only in "reasonable relationship to their numbers in the general population." In addition, considerable overlap in the various categories was both allowable and expected: many of the young would also be black and female, and so on. The new rules were not expected to eliminate the domination of white males over 30, but rather to chip away at that domination.

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[18] Frank Sorauf, "Party and Patronage" in *Politics and Social Life*, ed. Nelson W. Polsby (Boston: Houghton-Mifflin, 1963) p. 452 describes this other face of patronage convincingly.

[19] Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960), p. 37.

[20] Why did so many of the Democratic Party's elite accept changes which effectively limited their power to control outcomes at the National Convention? One possible explanation is the widely held belief that Senator Edmund Muskie would be the Party's standard bearer in 1972. Muskie's early lead had the effect of focusing the attention of party elites primarily on the problem of unifying the party behind his candidacy against President Nixon in November. Party regulars, concerned with winning that election, were persuaded to accept changes providing delegate positions to the liberal-reform element in order to gain the support of that group in the Fall. The prevailing assumption was that the Convention itself would be more of a showcase than a substantive decision-making body. The participation by more women, young people, and minority groups would aid in the cause of party unity, hence victory. Most party leaders, in short, were looking back to the problems of the 1968 campaign (liberal defections from Humphrey) when they made their decisions on implementation of the new rules. They assumed that the problem in 1972 would again be one of uniting liberals around the candidacy of a "regular" Democrat, Muskie.

Finally, the new rules were seen to provide several advantages in the simple maneuvering room which they gave to the slatemakers because these persons were under no legal requirement to accept the recommendations of local district meetings (a fact that was not widely understood). The slatemakers fully expected to be able to use the general confusion about the new rules to turn aside a great number of the traditional requests for delegate slots. Those who continued to press for such slots could be told that it was politically necessary for them to stand for election at their own district meeting. If they still persisted in pressing for a slot after having been rejected by their local caucus, the slatemaker could either plead helplessness in the face of "the people's decision" or could override the decision of the caucus and place the person on the delegation in any case. If the latter course was taken, the slatemakers would appear in the role of benefactors. Under the old rules such appointments had become so traditional that exceptions required justification. But under the new rules it could reasonably be expected that any contributor appointed in such a fashion could legitimately be asked to go beyond his normal effort that year.

Clearly also, the need for more people from categories previously unrepresented on delegations was felt to provide the slatemakers with the opportunity to alter the status of a slot as a political reward. If all or most traditional political figures are not on a delegation, then being on a delegation is not necessarily a matter of distinction. Some argued this fact would help to minimize the number of competitive situations in which gaining the aid of one person by giving him a spot on the delegation meant losing the support of two others who felt slighted (and who might be individually less important but jointly more important to the campaign). With luck it was felt that traditional political figures could be convinced that they had lost little of value under the new rules, while those in the newly eligible categories could be convinced they had received a great honor.

Setting the Stage

While the foregoing discussion is a fair representation of the views of the great majority of California's campaign professionals, the decisions made by many of those same professionals at the time of the delegate selection meetings in 1972 produced outcomes quite different from those expected a year or so earlier. In order to understand what took place at those meetings we will have to try to understand the political situation of each campaign organization at the time the meetings were scheduled. For the way each campaign staff dealt with the changes in the delegate selection process was largely due to the strategic situation of the campaign staff at that time.

Decision making in a political campaign must be directed toward solving immediate needs and problems, partly because of the scarcity of resources. The number of emergencies that arise during the course of a long campaign are immense and often unforeseen. Limits on time and money make it impractical to plan alternative courses of action to meet circumstances which may never occur. Further, presidential primaries involve a number of different coalitions, each felt by the participants to be less than the minimum coalition needed to conduct a general election campaign against the other party.[22] One of the jobs of a successful primary campaign staff thus involves assimilating the supporters of the losers into the camp of the winners. Ordinarily this means sharing decision-making responsibilities with those newly enlisted. This in turn implies that any long-range planning must be subject to constant review and change by the new participants if the sharing of authority is to be real.[23] Finally, it is self-evident that a campaign which begins with little support will be forced to subordinate any long-range planning to the almost daily struggle for mere survival.[24]

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[21] Under the new rules, the slate-making group for each campaign was to be composed of one person nominated by the campaign's executive committee from every congressional district. This person, who normally served as chairman of his own district meeting, was expected to be primarily responsive to the desires and direction of the campaign staff who had selected him. When we refer to the slate-makers, our reference is to the campaign staff who were actually expected to do this job, not to this larger group who simply ratified the final decision.

[22] As with many of the attitudes and sentiments of the campaign professionals, there is no clear factual basis for this feeling. We have little evidence to support contentions that elite disaffections produce mass disaffections—or, indeed, that campaign organizations have any substantial impact on the final decision of the voter. From a purely mechanical viewpoint it could be argued that a primary campaign with enough money and manpower has no real need to incorporate the elite structure of its opposition into a broader general election coalition. But the point is that whether the conviction that a larger coalition is necessary is correct or not, it does remain a firm belief guiding the behavior of the actors in California.

Thus, to understand what happened, it becomes important to understand the strategic situation of the various candidates in January, 1972—as that situation was perceived by the participants themselves. Such an understanding does not involve us in an analysis of the campaign strategies per se, for as we have seen, strategies tend to bloom late (if at all) in presidential primaries. Rather we must look to the relevant "scenarios" to see each campaign as it saw itself at the time. Political scenarios are chains of logical statements which purport to demonstrate the path to victory. They begin with a factual statement of the candidate's present situation. From this premise follows a series of conditional statements whose probability of coming true is directly dependent on the factual satisfaction of the previous conditions. Scenarios serve two major functions. One is to provide plausibility to a campaign, an argument that victory is possible even if unlikely. Although a surprisingly large number of the traditional political activists in California have expressed a willingness to support long-shot candidates, few have been willing to support a candidate they felt had no chance at all. A good scenario allows a campaigner to make a pragmatic argument for supporting even these long-shot candidates. In addition to being the equivalent of a series of predictions, a scenario becomes a set of campaign imperatives as well. The conditional statements amount to a series of goals for the campaign staff, the attainment of each making the next possible and so on. As such the scenario of a campaign serves as a rough substitute for strategy, helping to set priorities, allocate resources, and so on.[25]

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[23] The process of integrating losers into winning campaigns is an intricate and ritualized phenomenon. Below the level of major public officials (whose endorsement is a pro-forma obligation), it often takes the form of an elaborate courtship governed by a set of unwritten but complex rules. Winners are expected to humble themselves, to extol the virtues of their previous opponents, to credit luck rather than virtue for their success. Losers praise the efforts of the winners, usually observing that they must need no additional help, that they would simply disrupt a winning team should they come abroad. Winners are expected to counter this objection by pointing out their own weaknesses and noting the indispensability of the loser's aid, and so on. This formal behavior serves a number of important functions. It helps to defuse the now unproductive emotions which may have existed between two former competitors; it helps by appealing to those values that long time activists hold in common, stressing similarities rather than differences; or, as one politico put it, "If I shot all my enemies today, I'd have no friends tomorrow." Finally, it helps prevent primaries from becoming zero-sum games, an important consideration if our major parties are to remain the legitimators of the final two choices for public office.

This ritual courtship may or may not extend to the point of involving the loser in an active way in the winning coalition. After propriety has been served by the necessary asking and refusing, a point is reached where it becomes obvious that either the winner is sincere in his plea for help or the loser is firm in his refusal. If the latter is the case the reason given is usually a simple plea of exhaustion, a legitimate excuse. Under the code, it is now not legitimate for the loser to grumble. If the winner is sincere and the loser is interested in becoming involved, then the problem becomes one of finding or creating a role in the campaign structure commensurate with the new recruit's status in politics. This often results in a number of delicate situations requiring sensitivity and diplomacy. A new recruit offered responsibilities which are perceived to be incommensurate with his or her status may take offense; a legitimate reason for opposing the winner may be created. On the other hand, to provide a role of great authority for a former loser may result in the offending of some of those who have been with the winner since the beginning. To share decision-making authority in a campaign, it should be remembered, means taking authority away from those who already have it. The point is that in almost every campaign, strategic decisions are subject to constant review and revision by new personnel as they become involved. Long-range planning thus appears to most of the participants as obviously unproductive.

[24] To the major participants in a campaign the pace is constant and frantic. Authority in a campaign tends to be fluid rather than rigid, based on persuasion rather than command, and enforced rather through a sense of legitimacy than through sanction. Because most activists are involved in a campaign at least as much for the pleasure they derive from the activity as for a concern with the hope of eventual gain should victory be theirs, compliance with campaign decisions is best obtained primarily through discussion. The smallest decision may involve a campaign staff person in hours of such discussion. But to neglect this form of "involvement" often means making the campaign less enjoyable to the participant, thus diminishing one of the prime motives for his or her participation in the first place. Further, the need for money is infinite and the supply is always less than needed. Endless hours must be spent on fund-raising. Events must be planned. Contributors have a sometimes annoying wish to be talked with on matters other than money. "Leaders" of every group, no matter how small, must be courted courteously. All such activities take time—leaving precious little time for reflective thought. Moreover, the best minds of a campaign will usually be put to work on the most serious problem—which are always the most immediate problems. Persons concerned with long-term strategy tend to be taken less seriously by campaign professionals.

A "New" Politics?

To focus our discussion, we begin with an examination of the scenario of Senator George McGovern, the eventual winner of the California primary. McGovern's staff was initially in complete agreement with the general consensus of campaign professionals described earlier: that the new rules could be used to produce a more effective campaign.[26] As we shall discover, however, the McGovern situation as perceived by his campaign people appeared to dictate a series of decisions which severely altered those expectations.

To most observers, George McGovern began 1972 in the weakest position of any major contender. His supporters and opponents alike agreed that his one chance in California's winner-take-all primary would be to become the only identified liberal candidate in a field that included Muskie, Wallace, Humphrey, and Jackson. In those circumstances, the minority of liberal Democrats in the state might provide McGovern with enough votes for a plurality victory. But such an outcome appeared unlikely at the beginning of the year. McGovern was faced with several liberal campaigns, each of which seemed to have some initial advantage: McCarthy began with greater strength in the polls; Lindsay had glamor, money, and a good press in California; and Chisolm appeared to be attracting support among both black and active women's groups. If the results in the early primaries proved to be inconclusive, then there was the strong possibility that all the liberal candidates would actively campaign in the California race. If that proved to be the case, it was widely assumed that Muskie would be an easy winner. But if a monopoly of the support and resources available to liberal candidates in California could be hoarded prior to that time, then it might be possible to pre-empt the state from the other liberals. This pre-emptive strategy, like all other strategies, would be useless if McGovern were out of the race after the Florida primary. But if both McGovern and Lindsay had shown enough strength to survive in the early going, then California's group of liberal activists would be forced to choose between them. The presumption of the Mc-

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[25] As examples, here are the semi-official scenarios of the campaign staffs of Chisolm, McCarthy, and Lindsay at the beginning of 1972.

Chisolm: By commiting herself early to running for President, Chisolm hoped to have placed the burden of black disunity on those who would oppose her. Once established as *the* black candidate, she would have become the rallying point for the disaffected and those wishing to put liberal pressure on the probable nominee, Ed Muskie. If Muskie had eliminated the other candidates in the early going, then Chisolm might reasonably have expected a respectable vote total in the California primary. This in turn would have increased her ability to bargain at the National Convention.

McCarthy: Assumed that Muskie would eliminate the nascent McGovern effort in the New Hampshire primary, and that some combination of Humphrey, Jackson, and Muskie would eliminate Lindsay in Florida. With both of the major contenders for the liberal position out of the race, McCarthy would then have met Muskie alone in the one-on-one race in Illinois. There, in the full glare of national publicity, McCarthy would have pinned the Daley label on Muskie (to make it more difficult for the Senator to obtain liberal support in later primaries), and come close enough to establish himself as a man with substantial support in even this unlikely state. If these conditions had been met and McCarthy had come close in Illinois, then he should have been able to win in friendly Wisconsin as the only liberal in the field. With two months to organize in California and New York, he should have then been able to consolidate this support and prevail against a divided field. With the Convention divided between Humphrey and Muskie, McCarthy should then have been able to obtain a bloc of votes large enough to be the balance of power, exact concessions on the platform, and so on.

Lindsay: As a latecomer to the Democratic party and to the presidential race, Lindsay was simply too new a Democrat for many California activists. But against this liability Lindsay was able to throw his appearance, style, and image—important in a state where glamor politics has many adherents. Many politicos have found it possible to support a liberal candidate if he has the good looks to offset this presumed liability. Emphasizing his press and television appeal, Lindsay's backers could plausibly foresee a victory in the California primary. Lindsay's scenario was simple. He would ignore New Hampshire where his unfavorable New York image would hurt him, and where he and McGovern would be competing for the presumably small liberal and/or anti-Muskie vote. New Hampshire should have eliminated McGovern, leaving no identifiable "liberal" in the Florida primary. There against a conservative field, Lindsay would parlay his liberal stance and charismatic image into a plurality victory. On the base of this victory he would move on to Wisconsin, to another victory, to increased momentum, and so on.

[26] Although McGovern's efforts have been widely identified as nonregular or anti-regular Democrat, those attitudes were not shared by the top decision makers on his California staff. McGovern's northern California manager, for example, had served in that same capacity for the party's last gubernatorial nominee, Jesse Unruh. His state chairperson, Mrs. Elizabeth R. Gatov, California's National Committee-woman between 1956 and 1964, had served as chairman of many state campaigns over the past 15 years.

Govern group was that the campaign with the initial strength would get stronger as liberals rallied around the liberal candidate who appeared to have the best chance. If all went well, the scenario continued, the other liberal candidates would have to choose between entering the state (thereby dividing liberals and helping the centrist Muskie) or staying out, hoping that McGovern would win and that his liberal delegates would ultimately turn to one of them during a deadlocked convention.

This scenario suggested an early organizational effort in California. McGovern's strong liberal record, his interim candidacy in 1968, and his long opposition to American policies in Vietnam provided him with a small edge over the other liberal candidates—at least until such time as one or another of them demonstrated substantially greater appeal in the polls.[27] In addition, McGovern was aided by seeming at the time to be the possible holding candidate for Senator Edward Kennedy; both because many politicos viewed his campaign as hopeless and because many former Kennedy campaign people were associated with his effort. This "stalking-horse" image made it possible for McGovern's people to attract the support of some traditional activists who hoped for a convention deadlock and an eventual Kennedy candidacy. An early organizational effort also seemed wise for purely defensive reasons. Senator Muskie's position at that point in time was strong enough to give credibility to the argument that his was the only real campaign of the year. It was hoped that, with the increased visibility that accompanies early organization, some rallying points could be established that would prevent a general stampede to Muskie.[28]

Pursuing this strategy of early organization, the McGovern campaign staff had established by the beginning of 1972 at least the rudiments of an organization in every county in California. Their plan was to take the most effective advantage of the new rules from the perspective of four relatively distinct goals:[29] (1) to obtain the largest attendance at their district meetings, thus obtaining greater publicity; (2) to have the most "democratic" delegation, in the sense that more of their delegates would actually be chosen from among those nominated in the district meetings than those of any other campaign; (3) to have the most "representative" delegation, in the sense that it would not only meet, but surpass, the quota recommendations set down by the State Commission on Delegate Selection; and, (4) to put together the best delegation possible from the standpoint of political effectiveness, the individuals best able to help Senator McGovern win the primary in California.

At the time these goals were formulated, none was felt to be incompatible with any other. The fact that the local committees had already been established and (in most cases) were functioning reasonably well supported the belief that enough publicity and personal contact could be generated to insure that large turnout which the state campaign staff had assured the press was an important indicator of "grass-root" strength.[30] In addition, it was assumed that the individuals involved in the local organizations would be the largest and most cohesive voting bloc in any given caucus. If this crucial assumption proved to be correct, then all of the other goals laid out by the state campaign were easily reachable.

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[27] McGovern's abortive effort in 1968 had a greater impact among California activists than has been generally realized. The peak of that effort came in the debate between McGovern, Humphrey, and McCarthy before the 1968 California delegation. McGovern impressed a good many of that group, ultimately gaining more than a third of his total strength from the California delegation. (See Chester, Hodgson, and Page, *An American Melodrama.* pp. 560–562 for an adequate description of this event.) The good will garnered at that convention provided McGovern with another small advantage over Lindsay.

[28] Senator Edmund Muskie was obviously in the best position of any candidate at the beginning of the year; the leader in all polls, both state and national; acceptable to (if not the first choice of) all wings of the party; felt by most to have a good chance against President Nixon in November—all things seemed to be going well for Muskie. However as a person, Muskie was not the first choice of most party activists, and certainly far from their sentimental favorite. If other candidates began to show strength, it was at least conceivable that Muskie could be stopped. Liberals who were with him because they desired a 'winner' would have preferred a winning liberal; moderates might have preferred a rejuvenated Hubert Humphrey. Still others could hope that should Muskie fail, Ted Kennedy might offer himself at the convention. Aware of these weaknesses, the Muskie campaign staff was also involved in a pre-emptive campaign strategy in California. As the only campaign group other than McGovern's to organize prior to the delegate selection meetings (although six months later than McGovern) the Muskie group felt that the early decision forced on activists by the date of the delegate selection caucuses would mean that potential Kennedy backers, pragmatic liberals, and sentimental Humphrey supporters would all have to choose between the possible and the probable just at that point when Muskie appeared the strongest.

[29] Goals 1–3 were specifically directed toward the achievement of the ends dictated by McGovern's initial scenario: to pre-empt the liberal campaign position in California. Goal number four is the traditional goal of all campaign professionals involved in putting together a state delegation.

[30] One of the traditional avenues to favorable publicity traveled by candidates with little support in public opinion polls is to argue that support at an early point in a campaign should be measured qualitatively. The "grass roots" argument used by the McGovern staff was in this hoary tradition. Support for front-running Muskie, it was argued, was broad but without depth. Support for McGovern was limited

Members of the state campaign had been in contact with the executive committee of each local area in an effort to work out with them the rough outlines of a slate for each caucus. Districts which included large, politically cohesive black communities were told that blacks should be overrepresented on their slate of nominees (since they would be underrepresented in rural areas with small black populations). Local committees were asked to work for the election of several large contributors who lived in the district. In most cases, the names of specific individuals who would be supported by the local committee at the caucus were worked out well ahead of time. It was assumed that the result of this organizational activity would be that the local committee members would dominate the district meetings and, under the direction of the state campaign, "democratically" (by force of numbers) elect those individuals who met both the criteria of utility to the campaign and the quotas set down by the new rules.

The District Meetings

What actually took place at the district meetings was quite different from what had been planned. The efforts of the local committees had produced the large crowds required by the publicity strategy. But the other elements of that strategy—to produce by elections nominees that met both the McGovern Commission quotas and the politically qualified delegates needed to aid the campaign in California—was a failure.

There were several reasons for this failure. Of these reasons perhaps the most important was the impact of the appearance at the district meetings of many white liberal activists who had not been involved in the campaign effort up to that time. These activists had a quite different appraisal of the functions and purposes of the new rules, and this difference provoked a clash of viewpoints that drastically affected the ultimate outcomes at the district meetings. The campaign professionals, it should be recalled, saw the new rules primarily as a more effective means to the traditional goal of any political campaign: victory at the polls. The new categories which the rules mandated would allow the professionals more freedom to choose the best campaign personnel. The remedies which the new rules provided to placate liberals (both in terms of the quotas and the new opportunities for participation) were felt to be an effective means of preventing the defections of those activists in 1972. Little more was expected.[31]

The liberal activists, however, were far less concerned with reform as a means to electoral victory. Suspicious of politics as an avenue of change, they had come not so much in order to participate in decisions believed by professionals to be in the campaign's interest as to judge the intent about application of the new rules. Only if they were satisfied that they were not being tricked into participation, being used to trick others into believing that something had changed, would they offer their conditional support. These activists focused on the fact that the convention would determine the nominee and platform of a major party. They were less interested in symbolic representation than in making it possible for a liberal candidate or a liberal platform to gain the legitimation that the Democratic Party's nomination was assumed to provide.

Partly as a consequence of this view, many of these activists expected persons selected as delegates to behave as "substantive" representatives, people who represented ideological perspectives or distinctive interest groups in the sense that they would "act for" those groups.[32]

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but intense. Over the long haul, they argued, the latter would prove victorious. While the press personnel were understandably skeptical about the conclusions of this self-serving argument, they were willing to use attendance at the delegate selection meetings as a rough indicator of where each campaign stood in terms of "active" support. That is, they were willing to consider the numbers that showed up at the district meetings as a newsworthy event. The McGovern people, having helped to establish this criterion, and having made claims about the quality of their supporters. were therefore required by the rules of political journalism either to have the largest crowds or to be labeled the real losers of the day.

The fact that political reality in electoral campaigns is to a large extent the product of a consensus brought about by the interactions of politicians and journalists has never been fully described, but see Polsby and Wildavsky, *Presidential Elections,* p. 134 and Nelson W. Polsby,"Primaries, Polls, and Populist Ideology," *Wall Street Journal,* May 22, 1972.

[31] It is worthy of note that none of the campaign professionals interviewed believed that the new rules would have any substantial direct or indirect effects on the voting populace. The concern of all was with defections among the cadre of activists who provide so much of the manpower in a campaign. The problem was perceived to be one of elite disaffection; the solution was to provide symbolic representation to the disaffected elite. That such actions might have an effect on the mass base of the party was not considered seriously by the California professionals.

[32] The distinctions between "symbolic," "descrip-

In contrast to the campaign professionals who saw delegates as basically campaign personnel, the assembled activists viewed them as moral ambassadors, the equivalent of public officials. For them, political skills and resources thus became a minor consideration.[33] The primary questions they asked were, "What do they believe" and "Which group do they represent" For the activists saw themselves in terms of groups to be represented by quotas of delegates of this type. Rather than acting primarily out of responsibility to the candidate or his campaign, most of this group saw themselves in a bargaining situation: they would support the candidate in return for representation on his delegation.

In virtually every district meeting this attitude led to a demand for a recess during which those assembled could break up into several subgroups of women, blacks, chicanos, youth, and so on. Each of these subgroups then met as a "caucus," formed a slate of two or three persons, and began a period of negotiation with other subgroups for reciprocal support. It is worthwhile noting that this procedure was encouraged not only by the representative goals of the Reform Commission but also by the organizational ideology of the liberal activists themselves. That is, the quota system established by the new rules appeared to legitimate the existing trend among liberal activists to claim representation for subgroups with theoretically distinct interests. Originally this argument provided a method for racial minority groups with low rates of active participation to legitimate their claim for greater representation in a group than the number of their activists would allow. The legitimacy of the subgroup's interest, the argument goes, is a question not to be determined by the number of people present and participating who share that interest.[34] The success of this argument as a tactic—quite apart from its validity—has led to its adoption by other numerical minorities. The force of this argument, we should note, is that interests rather than people should be represented. This in turn implies both that those sharing the interest are best suited to determine who should speak for them and that it is not necessarily the case that a person who falls into a subgroup category will adequately represent the "interest" of that subgroup.[35]

The planned strategy of the campaign staff, we should recall, dictated a cohesive voting bloc which would cut across ethnic, sex, and age lines, and which would be committed in advance to a politically as well as ethnically balanced slate. This plan failed for several reasons. First, most activists not previously involved in the campaign offered only conditional support. Evidence of planned slatemaking, although in accord with the letter of the new rules, was widely perceived as being of the species "old politics": a violation in spirit if not in fact. In those district meetings where the local campaign group made an effort to push through the prearranged slates, the non-campaign people quickly and unanimously closed ranks against this threat to popular sovereignty. In every case this coalition of opposition was so strong and so vehement that the local committee was forced to choose between two equally unpalatable choices: (1) to push ahead anyway, either losing all, or, if successful, running a strong risk of alienating a great deal of the potential support for its candidate within their district;[36] (2) to seek accommodation. The local campaign people, the most pragmatic of those at the district meetings, quickly settled on the second course.

Further, to pursue the original strategy would have meant openly arguing against the legitimacy of selection by subgroups. This proved morally disturbing as well as politically difficult. Not only had subgroup representation acquired new legitimacy because of the imposed quotas, but also the notion of vote-trading among the various groups to produce a consensus seemed distinctly "democratic." The slates that resulted from these meetings had been arrived at only after a period of hours of rather intense politicking. Nerves had been frayed, tempers had flared as commitments were made and broken—to most of those assembled it was serious, hard work, not

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tive," and "substantive" representation are, of course, Hanna Pitkin's in her *Concept of Representation* (Berkeley: University of California Press, 1968).

[33] In fact, they often became a liability. The anti-politician attitude was so marked at several of the meetings that persons running for delegate nominations were hooted from the podium for listing such traditional qualifications as campaign experience—most especially in cases where that experience was gained in the service of a moderate candidate. In one congressional district caucus a local officeholder nominated three men in the mistaken assumption that his political prominence would rebound to the benefit of his nominees; all three finished last in the balloting.

[34] A widely believed implication of this argument is that to reject it is tantamount to rejecting any concern for the interest of the subgroup.

[35] On the distinction between descriptive or symbolic representation and substantive representation, cf., Pitkin, chap. 9.

[36] Given the state of their campaign at that time (far back in the polls, with four opponents from the liberal wing of the party alone), the threat of losing support of any kind was immensely powerful to the local McGovern campaign people.

fun. Yet when the bargaining and voting were complete, each district had produced a slate which met not only the quota requirements but also a consensus satisfactory to most of the interests involved at the meeting. Representative goals had been achieved through democratic means.

But this strategy of subgroup representation produced consequences which left the state campaign staff far from satisfied. For one thing, many individuals with resources useful to the campaign, but without a "constituency" among the assembled activists at the congressional district caucuses, failed to achieve the necessary votes.[37] The fact that the votes necessary for election by the assembly were obtainable in many cases only with a subgroup nomination means—in the case of women in particular—that many who fell into a quota category but were not perceived as "representing" that category's interest were eliminated. Many women who had been most active in the campaign, for example, were not ordinarily among the high finishers in the vote totals. Rather the various women's caucuses tended to nominate women whose reputation and concern were focused primarily on the issue of women's rights.[38]

Additionally, the state campaign organization had asked the local committees in certain districts to make an effort at over-representation of some of the ethnic groups that formed a relatively large proportion of the population of that district. Rather than add one black delegate from each district (many of which had black populations considerably below 1 per cent), the campaign's theory was to select more black delegates from those areas that provided much of the black political leadership in the state. A district with a black congressman, assemblyman, mayor, and the vice-chairman of the state's Democratic party might have population figures which called for only two black delegates. Yet it was obviously more sensible to the campaign staff to choose more than required here in order to remove the pressure from those districts where the number of black residents was small or where the leadership was supporting some other candidate.

While this aspect of the strategy was successful, what the state staff had not anticipated was the possibility that districts with virtually no minority population would also nominate minority delegates. Yet in caucus after caucus that was precisely what happened. In the state's largest district meeting, for example, there were two persons of Spanish-speaking origin present out of a total of more than 900 activists. Those two individuals formed a "Chicano Caucus," ran as representatives of that "group," and were among the top seven vote-geters.

When the state campaign staff assembled late that evening to analyze the results of the delegate selection meetings, they were faced with a series of problems that offered no easy solutions. The procedure of subgroup nomination followed in almost every caucus had effectively isolated as a residual group the white males over 30 years of age. This group, although in some cases the largest single group at any caucus,[39] was not provided for in the new quotas. Further, these people did not constitute a group in the sense of being bound together by any shared "interest." As a result, they did not constitute a voting bloc at any meeting. Those over-30 white males who were interested in running for delegate either made an unsuccessful effort to pursue traditional buttonholing strategies or in some cases simply decided not to run.[40] Of the 105 delegates elected in district meetings in Northern California alone, only 8 per cent were in the over-30, white male category. The campaign staff had previously appointed ten white males as chairmen of some of the meetings and had made it clear that three additional men from this category would be appointed regardless of the caucus outcomes. But even with these appointments, the statewide composition of the slate that resulted from the delegate selection meetings was unbalanced, by any standard of "reasonable relationship to numbers in the general population":[41]

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[37] The campaign's state finance chairman, a man who was to leave his business and family for weeks on end to devote his time to fundraising efforts, was nominated at a northern California district caucus for a delegate position by a local Assemblyman's staff assistant and lobbyed for by the campaign's Bay Area coordinator. He ran sixteenth (out of 19 running), but was the highest vote-getter of the white males over 30.

[38] Although several women had been appointed campaign county chairpersons, they found this to be of little help in obtaining a delegate slot: all were by-passed in the voting at their meetings.

[39] One meeting had no less than seven subgroup caucuses. The fact that it was the "interest" of each category that claimed representation (rather than simply the category) made it difficult to combine the categories—although the rules did allow a certain amount of overlap of this kind. Of the final slate of the Northern California delegates, for example, only five of the blacks were women, and only one of those was under 30 years of age.

[40] Two courageous state assemblymen, both of whom risked political advancement to support McGovern, both of whom wished very much to be delegates, finally decided not to run at the meeting when it became clear they would very possibly be defeated in their own districts.

51 per cent were women
40 per cent were under 30 years of age
21 per cent were black
20 per cent were Chicano, Indian, and Asian
19 per cent were white, male, and over 30.

The state campaign people, we should recall, were not forced by the new rules to accept these essentially advisory caucus results.[42] Nor was that their initial inclination. Again, their hope had been that the new rules would provide them the freedom to select delegates who would come as close as possible—given the early date of the meetings—to involving the effective political leadership in each district. The McGovern district meetings were themselves unrepresentative of that leadership for the obvious reason that McGovern's appeal at that time was generally restricted to Democrats from the liberal side of the spectrum. Yet even the leaders of those liberal campaign people, labor organizers, and contributors who were committed to McGovern were generally bypassed by those at the district meetings. In fact, to agree to the results of the meetings would have meant accepting as delegates a large number of people who had never been involved in the McGovern campaign effort prior to that period.

To overrule the selections made in the district meetings would have created a different set of difficulties. The activists present at those meetings viewed themselves as constituting a sovereign body.[43] They looked with disfavor on the notion that a few staff people could appoint persons not in attendance or without sufficient support at the meetings. Many communicated these sentiments vehemently to the campaign staff.[44] The state campaign was thus put in such a position that not to accept the results of the meetings would have meant the subversion of "democracy." Worse still, it would have meant bypassing the democratic procedures of the caucuses in order to lower the number of women, young people, and minority representatives on the delegation.

As so often happens in political campaigns, the decision was made according to the rule which solves the most immediate needs and poses the fewest immediate problems. The immediate need dictated by the McGovern scenario was to establish the senator's campaign as the foremost liberal effort in the field. To violate the decisions of the district meetings in order to lower the number of minorities on the slate, the staff felt, would not have aided that cause. In addition, it was felt that those who had been excluded by the caucus process were those who, because of their own experience and attitudes, would be the most willing to accept the decision without withdrawing their support.[45] Thus the final McGovern slate, later the California delegation to the Democratic National Convention, remained basically that chosen by the "people" in the meetings established by the new rules.[46]

Evaluation

The foregoing description should make it obvious that the outcomes of the new delegate selection process were quite different from those expected. But did those outcomes follow from the changes in the rules, or did they grow out of the campaign's unique circumstances? There is no definitive answer to that question, but certain conclusions do appear clear. First, both the manner in which the delegates are chosen and the categories which must be represented under the new rules make it much more difficult to select delegates on the traditional basis of campaign criteria. In the past a delegate's ideological proclivities mattered very little; all that was asked was that he commit himself to the candidate's cause. Merit (at least in theory) was the criterion: persons were chosen because it was felt they were best able to help the campaign. These essentially technical standards are much less likely to be utilized in the district meetings established by the new rules, in part because most attendees share neither a belief in the value of the old standards nor the knowledge to apply them. Moreover, in the struggle for votes which dominates democratically organized meetings, the technical standards compete at a disadvantage with the more personal characteristics of the candidates. And, of course, the new criteria for increased representation would make the application of the old standards more difficult even if the caucus attendees were so inclined.

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[41] California's minority population (black, brown, Indian, Asian) comprises about 18 per cent of the total population of the state; women constitute about 52 per cent. One of the major questions at the time of the caucuses was whether to represent according to general population figures, according to Democratic registration figures, or according to Democratic loyalty. The final decision appears to have been to use that standard which best served each minority group.

[42] The McGovern commission rules required only that all Democratic voters have the "full and timely opportunity to participate" in the delegate selection process.

[43] Several of the meetings adopted resolutions expressing this point. Not only did they demand that the final selections be made from the nominees of the district meetings, but—in cases where the number of nominees exceeded the number of delegates alloted to that district—that they be chosen in the order of the votes they received. Thus a district alloted six delegates might find 20 candidates. The top 12 names would be submitted to the state staff, but the caucus resolved that the top 6 must be selected. For most activists the only legitimate reason for deviation was to *increase* the number of minority representatives on the slate.

[44] These sentiments were usually accompanied by threatened sanctions involving not only the withdrawal of support, but also of positive action to harm the campaign's chances in California. In one district two of the eight delegates were not chosen from among the caucus nominees. The result was a virtual civil war within the campaign in that district, including a series of bitter accusations and recriminations in the local press.

[45] This expectation was only partially realized. The leaders of two of San Francisco's most powerful labor unions chose to regard the decision as a personal choice on the part of McGovern to slight organized labor in general and themselves in particular.

[46] A press release issued by the campaign headquarters shortly after the meetings stated that: "First, and we feel most important, 94 per cent of the McGovern delegation was chosen from among those who were nominated at the delegate selection caucuses last Saturday. . . . . As supporters of the man who headed the McGovern Commission on Party Reform, we have long felt a special obligation to practice, as well as advocate, real citizen involvement. We are proud of this delegation because, in every possible way, it fulfills our special obligation" (February 16, 1972).

Second, the new rules produce a situation in which the delegates selected have a much less central relationship to the state and local campaign effort. In part this is because most delegates were chosen by other than campaign criteria. But it is also because the obligation of those chosen was to the people at their district meetings rather than to officials of the campaign list itself. Authority in a political campaign has traditionally been distributed in a hierarchical pattern. The candidates choose national leaders who choose state leaders who choose local leaders. Each group is responsible to those who appointed them.[47] The new system of delegate selection breaks this pattern. Delegates are chosen by persons outside the campaign hierarchy and therefore are under no obligation to defer to the decisions of the campaign leadership or, indeed, to those of the candidate himself.

The establishment of quotas exacerbates the two previous consequences. The use of categories tends to legitimate the representation of "interests," which often produces criteria for delegation selection at odds with the traditional standards. Representatives of an "interest" chosen in caucus by others who share that interest, will be under no obligation to defer to the campaign leadership if they determine that exigencies require action to which the delegate is opposed. Finally, of course, the quotas make it more difficult to use political criteria in choosing delegates because most of those chosen under such standards are white, male, and over 30.[48]

These general consequences proved to be particularly troublesome to McGovern's efforts after his California primary victory to broaden his base among the elite structure of the Democratic Party. In the past, the winner of California's primary balanced his delegation to the National Convention by adding representatives of the losing slates.[49] This process often involved asking original members of the winner's delegation to resign. McGovern's delegates, however, were not selected by traditional political criteria and were less amenable to arguments which called for traditional political gestures. Because of the time and manner in which they were chosen, they were generally less willing to engage in practices which appeared to thwart the purposes of the new rules. Some were not inclined to trust anyone who had not been with McGovern in the beginning; others were opposed in principle to resignations and additions because they involved less representation for the categories that were victorious in February. Many were upset at the idea of rewarding those who had so recently been their enemies.[50] For all these reasons—as well as the simple fact that each delegate wanted to be present at the triumph of his candidate—there were no resignations by McGovern delegates. In addition, however, of the 33 delegates chosen by the winning slate after the primary election,[51] only 6 had been openly committed to another candidate. The rest had either remained neutral or joined the McGovern effort after the date of the district meetings.

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[47] While there are a number of practical reasons for structuring a campaign this way, the committment of many of the political activists interviewed went far beyond questions of efficiency in their defense of the existing system. One person, active for over 15 years, put it this way:

> If everyone were able to "do their own thing," if we voted on campaign decisions such as whether to endorse this or that local candidate or issue or what the candidate's position on issues should be—if we did that, then the candidate would not really be in charge of his own campaign. That could not only be bad for the candidate, but it's bad for the country. One of the few really good indicators we have of how a man will govern is to ask how good a campaign he has. Is he able to attract loyal, competent men? Is he able to keep them working as a team when they disagree with him or with each other? How accessible will he be if he wins? What is his sense of constituency? What is his sense of politics? All of us make guesses about these things, but a campaign provides the only hard evidense one way or another. If campaigns were run from the bottom up rather than the top down, you would just never know.

Many of the activists involved in McGovern's effort accepted neither the traditional manner of campaign structuring nor its justification. They would argue, rather, that the strength of the campaign lay in its mass base of activists. That strength would presumably be lost if members of the mass did not have an equal say in policy decisions.

[48] While the losers under this system can be described in terms of political categories (officials of the party and campaign, office-holders, contributors, etc.), it is also worth noting that a high proportion of the men in some of those categories were Jewish. Traditionally represented in numbers greater than their proportion of the population on Democratic delegations because of their long-term committments of time and money to Democratic politics, Jewish males over 30 were virtually excluded from participation in 1972. While no discriminatory motives were involved in this exclusion, the fact of being placed in an excluded category still rankled among many of those who, by the traditional standards had earned a place on the delegation but still, even though members of an ethnic minority group, did not appear to qualify under the new rules.

[49] To the losers, this "balancing" process is more

The campaign strategists' inability to make any significant gesture to the representatives of the losers had several adverse consequences. Organized labor, for example, had virtually no representation on the final McGovern delegation.[52] Only 15 per cent of California's incumbent Democratic officeholders were included on the delegation—and half of those had been chosen on the day of the district meetings. As a result, the resources of money, manpower, and public respectability ordinarily available to nominees from these groups came to McGovern late, only in part, and with little enthusiasm.[53]

A final consequence of the new rules, damaging to the McGovern effort, but with possible long-term effects as well, involved the striking change in the public face of the Democratic delegation in 1972. Our National Conventions have served for more than 150 years as the arbitrators of that decision most crucial for our politics: Who is to be a legitimate and therefore a "real" candidate for the presidency?? The nature and source of this authority to legitimate is something which we understand only poorly, but surely it has stemmed in part from the fact that in the past delegates were themselves persons of authority in their own locales—authority garnered by their ability to win elections and to elect others, the basic source of all legitimate authority in a democracy. The absence of most of these persons from the delegation in 1972 had consequences which are difficult to assess. What is clear is that many of the party's elite were unhappy with the final delegation—and felt less of an

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than symbolic. If the winners are vindictive, losers in a primary fight stand to lose more (in terms of office, status, and authority) if their own party wins the general election than they would if the other party won. Because of this fact the losers require some assurances that, by enthusiastic participation in the general election, they may atone for their early sins and earn a place in the division of offices. The first opportunity for such assurance is the balancing of the delegation. The gesture is expected. Not to make the gesture is, to some, the equivalent of a hurled gauntlet. Cf. also note 22.

[50] This attitude was of course, precisely the stance feared by the losers. It is perhaps best exemplified by the treatment given California's junior U.S. Senator, John Tunney, by the delegation when they met after the primary to fill out the open slots on their slate.

McGovern's campaign staff in California had recommended adding Tunney, an early Muskie backer, to the delegation. This recommendation was accepted by McGovern's national staff and, in a phone call played over a loudspeaker, the national campaign director, Gary Hart, told the assembled delegates that McGovern's personal desires were that Tunney be placed on the delegation.

Those delegates who had been politically active in the past had almost unanimously supported Rep. George Brown over Tunney in the bitter 1970 Senatorial primary. They responded to McGovern's recommendation noisily and negatively. A period of debate followed during which a vote count by McGovern's staff showed that Tunney would lose. To stem this tide, George Brown himself was brought to the hall to endorse Tunney's selection. The final speech for Tunney, itself symbolic of the campaign's difficulties, was delivered by Assemblyman Willie L. Brown, Jr., one of the leading black politicians in the state and later chairman of the California delegation. The flamboyant Brown proceeded to castigate Tunney in the most vitriolic manner for fully 10 minutes—each insult receiving louder cheers. Brown concluded by saying: "John Tunney is a devil. But the book says that we have to give the devil his due. What the devil is due on this delegation is one seat—and who better to sit on His Seat than John Tunney." This appeal struck a responsive chord with a majority of the delegation and, by a close vote, they accepted Tunney. There remains some doubt as to how appreciative Senator Tunney was about the gesture.

[51] The law required 88 per cent of the delegation to be chosen by the process described in this article. The remaining 12 per cent were to be selected after the primary by a vote of the 238 delegates chosen on the first date.

[52] Because many of the women and black delegates on the Humphrey slate came from the ranks of organized labor, the challenge to the McGovern slate at the convention took on the characteristics of a battle for labor's right to many union officials. This only further complicated McGovern's difficulties later on.

[53] For example, the Nixon Committee was able to charge as late as October 15th that only three of California's 43 Democratic Assemblymen were openly campaigning for McGovern.

obligation to the nominee as a result.[54] What remains to be discovered is the impact which this new face of the Democratic party will ultimately have on its voting affiliates. The categories prescribed by the new rules were intended to provide at least symbolic representation for several blocs of traditionally (or hopefully) Democratic voters. But in 1972, those rules and the dynamics of politics combined to produce a delegation which did not represent in either a symbolic or descriptive fashion the majority of those who have supported Democratic nominees in the past.[55] If this fact proves to have the delegitimating effect which many in the party's elite structure fear, then the effect of the new rules may be to narrow rather than widen the party's ability to attract votes.

Conclusion

The purpose of this article has been to suggest that the new rules governing the selection of delegates to the Democratic National Convention have produced a series of major changes in the strategic environment within which presidential candidates compete for the Democratic nomination. An examination of the effect of these changes as they applied in the case of the California delegation to the National Convention provides support for the conclusion that the new rules did not aid, but in fact hindered the efforts of the eventual winner to mount an effective electoral campaign. While the major impact of these changes was related to the unique constellation of political events of 1972, the changes themselves could in all likelihood remain operative for the foreseeable future. Future candidates and the Democratic party as a whole will have to adapt as best they can to the new environment produced by reform.

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[54] It is ironic that McGovern suffered more under the new rules than did any other candidate. Those rules freeze the public face of a campaign into the image it had when the delegates were chosen. McGovern, whose early image was as a candidate of a narrow section of the party had both personally and through his staff made many efforts to broaden his support within the party in California. But the inability of his staff to back up its verbal assurances with actual seats on the delegation had left many uneasy. Watching the convention on television, this uneasiness hardened into conviction for many traditional politicians. As one horrified official put it, "It looks more like the cast of 'Hair' than a Democratic delegation."

The Press, for the most part, treated the delegates chosen under the new rules exactly as they had treated delegates in past years: as official spokesmen for the campaign. Often this resulted in conflicting signals. Shortly after the convention, one of McGovern's staff members, having just left an exhausting three-hour meeting designed to persuade an important contributor that he was "wanted," turned on his car radio to hear one of the delegates using the contributor as a prime example of the "old hacks and fat-cats that we beat this year."

[55] Robert Axelrod, "Where the Votes Come From," *American Political Science Review*, 66 (March, 1972), 11–20.

Comment on "Changing the Rules Changes the Game"

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Since the bright summer and dark fall of 1972, Democrats have been disputing whether the McGovern-Fraser commission's reforms were a triumph for justice and democracy or a recipe for electoral disaster. Party-watching political scientists, however, must surely agree that they provide us a veritable mother lode of research material. The dispute reminds us forcefully that however unimportant party rules and processes may seem to some academics, most of the people who operate in our political system's inner circles believe the stakes of intraparty fights are well worth fighting for and recognize party rules as a major factor in determining their outcomes. The reforms demonstrate that the parties' rules and structures are not fixed forever beyond the power of human volition, and thus teach us that henceforth we must be more sensitive to the possibilities of change and more concerned with its probable consequences.

As a former commission member who freely accepts one twenty-eighth of whatever praise or blame our reforms deserve, I have read with fascination Mr. Cavala's account of California's delegate slatemaking rules and practices in 1972. I recall that California concerned us more than any other state: our Guideline C-6 was written and adopted mainly to open up its closed slate-making processes; our Guideline B-6 was an effort to persuade California and a few other states to repeal the winner-take-all provisions of their presidential primary laws; and most of us hailed as a great triumph the new slate-making procedures worked out through long and often painful negotiations between the California party and the national commission's chairmen and staff.

As a political scientist who has come to believe that impact studies offer the most promising way our discipline can make a scholarly contribution to the formation and evaluation of public policies, I welcome Cavala's focus on the interpretation, application, and consequences of California's new slatemaking rules. I regret that his data consist so largely of his own undocumented participant-observer's impressions, but perhaps that is the best one can do so soon after the fact. I should add that his observations are quite consistent with those I have seen in newspaper accounts and heard from other participants, including those who worked for candidates other than Cavala's.

Accepting his account as essentially accurate, then, what does it tell us about the reforms' impact? Cavala's central conclusion is that the new rules were intended to serve new purposes and that the efforts of the candidate organizations to bend them to the old purposes failed miserably. In 1968 and before, he tells us, the typical presidential candidate organization in California put together its delegate slate for the primary so as to broaden its appeal and thus improve its candidate's chances of winning. After the primary the winning organization sought to increase the party's chances of carrying the state in November by adding prominent party leaders and contributors who had supported losing candidates or no candidate in the primary. The main object of the old closed procedures was thus to maximize what might be called "combat effectiveness," first for the primary and then for the general election. There was no nonsense about "open access": if the slatemakers thought you would help them win, you were in; if not, you were out.

Cavala relates how the leaders of the candidate organizations in 1972 (including McGovern's, in which Cavala held a high position) hoped to serve these same purposes under the new rules and even take advantage of them to jettison some deadwood. But the rules routed them. Guideline C-6's requirements for open access resulted in district caucuses dominated (he does not say "packed") by people with little or no past service to the party or feelings of obligation to its leaders. Consequently, he reports, they were less likely than the slatemakers of earlier years to pick delegates for the purpose of enhancing their candidate's chances of winning the primary, and even less likely to step aside after the primary to make room for party notables. And the injunction in Guidelines A-1 and A-2 that the *state* delegation should include women, young people, and members of minority groups "in reasonable relationship to their presence in the population of the state" became a prime criterion at each *district* caucus as well. Moreover, Cavala reports that most of the people attending the district caucuses regarded themselves first as representatives of and responsible to particular demographic groups, second as supporters of particular candidates, and only third as Democrats.

He concludes that "the rules and the dynamics of politics combined to produce a delegation which did not represent in either a symbolic or descriptive fashion *the majority of those who have supported Democratic nominees in the past*" (italics added). Quite correct; but neither the national nor the state reform commissions intended that it should. The prime objective of both was not to make the party more combat-ready for November, but rather to ensure a more *representative* California delegation and national convention. More representative of whom? Not necessarily of all elements of the New Deal coalition, but rather of certain demographic groups—women, youth, minority groups—the commissions believed had been discriminated against by the traditional overstocking of conventions with middle-aged white males. Certainly not the representation of party notables or regulars or contributors, but rather of people who in 1972 were active on behalf of particular presidential aspirants. In Polsby's and Wildavsky's categories, the reforms were intended to maximize the representation of "purists," not of "professionals."

If this is a correct assessment of the commissions' intentions, then Cavala's account shows convincingly that their reforms worked splendidly and defied all efforts by the professionals, including those in McGovern's organization, to make them serve the old purposes. On the other hand, if the party's prime objective should again become a delegation and a convention that maximizes the party's chances of carrying California and the nation, then perhaps Cavala is right in concluding that "future candidates and the Democratic party as a whole will have to adapt as best they can to the new environment produced by reform."

But there is another possibility. If those Democrats whose main organizational goal is combat effectiveness for defeating Republicans can muster the necessary support, they can change the environment. For if the events since 1969 prove anything they prove that a party's strategic environment is not wholly a given to which it must adjust as best it can; it is in part a set of rules, procedures, and understandings it can change significantly. Changing the rules does indeed change the game; but there is no reason to suppose that the changes of 1969–72 are the last we will ever see. Indeed, the successor Mikulski commission has already voted to drop the demographic quotas and include more party notables. So the 1976 rules should differ significantly from those of both 1968 and 1972; how much the game will change remains to be seen.

Social Mobilization and the Russification of Soviet Nationalities*

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The Soviet Union is a multinational country. Russians, the largest nationality, comprise only 53 per cent of the total population; in all, approximately 93 nationalities are officially recognized as indigenous to the USSR. Yet despite the increasing attention that Western scholars have devoted in recent years to the study of ethnic conflict in other countries, the Soviet Union has received comparatively little attention. Although there have been a few case studies of particular Soviet nationality groups, scholars have made little use of the more sophisticated conceptual and methodological tools from the general discipline of political science to study Soviet ethnic relations.[1] The purpose of this paper is to examine certain major determinants of ethnic identity change in the Soviet Union. At the same time, by employing systematic quantitative data, I hope not only to explore this substantive issue but to demonstrate the utility of relatively advanced conceptual and statistical tools in the study of Soviet ethnic processes.

Western scholars have tended to emphasize certain features of Soviet nationalities policy: the largely formalistic nature of the political autonomy granted the nationalities; the severe restraints on literary, scholarly, and artistic expression by nationalities and the invariable "Sovietization" of national groups according to a single model of economic, social, and political organization. Moreover, Soviet doctrine envisions that the federal structure and national differences will eventually disappear.[2] But one should not exaggerate the extent to which central policies of social, economic, and cultural change have weakened the bases of ethnic identity in the USSR. In addition to the policies that tend to inhibit the expression of distinctive national goals or values, the student of Soviet ethnic relations must be aware of a number of long-term, more or less enduring factors that *support* the persistence of ethnic patterns. Tentatively, one may classify such supportive factors into two not mutually exclusive categories.

On the one hand one may identify factors deriving from the Party's *policies,* such as the delimitation of administrative boundaries on linguistic or ethnic bases; the standardization and development of national literary languages, and the expansion of scholarly and artistic production in these languages; the creation of a vast network of native-language schools for some sixty nationalities; and the use of ethnic labels in official dealings with Soviet citizens (such as the labeling of persons by nationality on standard identification papers).[3]

On the other hand are those underlying *cul-*

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[1] Three important exceptions are the studies by John A. Armstrong, "The Ethnic Scene in the Soviet Union: The View of the Dictatorship," in *Ethnic Minorities in the Soviet Union,* ed. Erich Goldhagen (New York: Praeger, 1968), pp. 3–49; Vernon V. Aspaturian, "The Non-Russian Nationalities," in *Prospects for Soviet Society,* ed. Allen Kassof (New York: Praeger, 1968), pp. 143–198; and V. Stanley Vardys, "Communism and Nationalities: Soviet Nation Building," Unpublished paper presented to the 65th Annual Meeting of the American Political Science Association, New York, September 1-6, 1969.

[2] For reviews of official doctrine and policy, in addition to the works cited above see: Richard Pipes, *The Formation of the Soviet Union: Communism and Nationalism, 1917–1923,* rev. ed. (New York: Atheneum, 1968); Robert Conquest, *Soviet Nationalities Policy in Practice* (New York: Praeger, 1967); Grey Hodnett, "The Debate over Soviet Federalism," *Soviet Studies,* 18 (April, 1967), 458–481; and Teresa Rakowska-Harmstone, "The Dilemma of Nationalism in the Soviet Union," in *The Soviet Union under Brezhnev and Kosygin: The Transition Years,* John W. Strong, ed. (New York: Van Nostrand, 1971), pp. 115–134.

[3] For a review of Soviet language policy, see Conquest, *Soviet Nationalities Policy.* For a discussion of educational policies with respect to the nationalities, see Jaan Pennar, Ivan I. Bakalo, and George Z. F. Bereday, *Modernization and Diversity in Soviet Education: with Special Reference to Nationality Groups* (New York: Praeger, 1971); Yaroslav Bilinsky, "Education of the Non-Russian Peoples in the USSR, 1917–1967: An Essay," *Slavic Review,* 27 (September, 1968), 411–437; and Harry Lipset, "The Status of National Minority Languages in Soviet Education," *Soviet Studies,* 19 (October, 1967), 181–189.

tural or demographic conditions that may support the maintenance of ethnic identities, such as the degree of territorial dispersion, urban-rural dispersion, and occupational specialization of nationality group members; traditional religion (the vestiges of which still have a good deal of influence on interethnic contacts); and the degree of affinity (linguistic, cultural, and historical) among ethnic groups.

This paper will test a limited number of hypotheses relating to certain major demographic and cultural determinants of ethnic identity change in the USSR. The paper will not investigate the specifically *political* factors affecting ethnic identity maintenance or change, nor will it attempt to show the relationship between official policies and the demographic or cultural changes that may be occurring among the nationalities. Nevertheless, one should keep in mind that the political context within which the demographic and cultural factors may be operating does not exclusively favor the Russification of the minorities. In Vernon Aspaturian's terms, the Soviet regime has fostered the "Sovietization" of all the nationalities; the spread of Russian culture and language has tended to "Russianize" the native areas; but the regime has not generally promoted *Russification*—the psychological transference of persons from a non-Russian to a Russian identity—though this has occurred to some extent as a by-product of Sovietization and Russianization.[4]

National Identity: Concept and Measurement

I shall use the term "national" or "ethnic identity" to mean an individual's emotional attachment to certain core symbols of his ethnic group.[5] Two distinct aspects of this approach should be stressed. First, national identity is a personal and subjective rather than a group characteristic; second, it encompasses a limited set of beliefs that are defined a priori as "national" or "ethnic."

Following Shibutani and Kwan, one may define an *ethnic group* as "those who conceive of themselves as being alike by virtue of their common ancestry, real or fictitious, and who are so regarded by others."[6] The present analysis assumes that an individual maintains his *ethnic identity* when he remains emotionally attached to the group, even though the ethnic group may no longer preserve distinctive cultural forms. Thus, ethnic identity is not a group characteristic but an individual one.[7] Although the retention of traditional or characteristic language, religion, dress style, or other practices might help a group to sustain its cohesiveness, an individual may retain his ethnic identity even while forsaking such practices. Nevertheless, the present group member *may* associate such elements symbolically with the group; his observance of them may support or demonstrate his feelings of attachment to the group. In other words, such features could form part of what Daniel Glaser has termed as individual's *ethnic ideology:* all ideas and images that individuals ascribe to their own and to other ethnic groups.[8] But an adherence to one's traditional ethnic identity need not imply a commitment to all (or even very many) of the traditional or contemporary norms and values of one's ethnic group.

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[4] Aspaturian writes: "Sovietization is . . . the process of modernization and industrialization within the Marxist-Leninist norms of social, economic and political behavior. Russianization is defined as the process of internationalizing Russian language and culture within the Soviet Union. . . . Finally, Russification . . . is defined as the process whereby non-Russians are transformed objectively and psychologically into Russians, and is more an individual process than a collective one." See "The Non-Russian Nationalities," pp. 159–160.

[5] I shall use the terms "ethnic identity" and "national identity" interchangeably. Although there is a more specialized Russian terminology referring to ethnic groups according to their stage of historical development, Russian writers usually refer to the Russian people and to the other ethnic groups *generically* as either "nationalities" (*natsional'nosti*) or "peoples" (*narody*).

It may be of some interest to add that the Soviet Union and the Soviet people are never referred to collectively as a nation or nationality. One may speak of the Soviet government (*pravitel'stvo*), state (*gosudarstvo*), or country (*strana*). Similarly, one may refer to the Soviet people (*narod*) or peoples (*narody*). But never to the Soviet "nation" (*natsiia*), a term which is reserved for some thirty Soviet ethnic groups. For an interesting and critical discussion of Russian ethnic terminology, see V. I. Kozlov. *Dinamika chislennosti narodov: Metodologiia issledovaniia i osnovnye faktory* (Moskva: "Nauka," 1969), pp. 20 and 55–69.

[6] Tamotsu Shibutani and Kian M. Kwan, *Ethnic Stratification: A Comparative Approach* (New York: Macmillan, 1965), p. 47.

[7] Note that two common usages of the term "national identity" are avoided here. First, one often finds the term employed synonymously with "national integration," especially in discussions of a nation's or new polity's "search for identity." By this is variously meant a quest for a set of common values (transcending, perhaps, more "parochial" loyalties) or for agreement on the norms governing political action. While one might wish to label such a consensus on the ends and forms of the political community "national identity," such a usage tends to confuse the concepts of "state" and "nation" and seems to imply that the achievement of "national integration" (or of a viable political system) is *by definition* inimical to the preservation of narrower "ethnic" loyalties. For a discussion of the tendency to confuse the concepts of nation and state, see Walker Connor, "Nation-Building or Nation-Destroying?" *World Politics,* 24 (April, 1972), 332–336.

A second approach equates the term "national identity" with consensus on the set of beliefs expressed in answer to the following questions: What sort of people are we? What makes me a (Canadian)? How am I different from other people? This approach more or less equates national identity with a collective self-description of a people. Such self-description seems to be identical to what Daniel Glaser has called "ethnic ideology" (to be discussed further below). See, for example, such uses of the term "national identity" in S. N. Eisenstadt, "Israeli Identity: Problems in the Development of a Collective Identity of an Ideological Society," *The Annals of the American Academy of Political and Social Science,* 370 (March, 1967), 116–123; and Mildred A. Schwartz, *Public Opinion and Canadian Identity* (Berkeley: University of California Press, 1967).

This emphasis on the personal and subjective dimension of ethnicity is supported by recent scholarly work on other countries that shows ethnic attachments (ethnic identities) to be much more durable than most other traditional or customary behavioral patterns. Students of American ethnic groups have increasingly acknowledged that many ethnic group members seem at once to be almost completely acculturated into the American "melting pot" and to maintain an identification with their historic group. It is significant that despite the failure of most of his daily experiences to reinforce traditional or culturally distinctive patterns of behavior, the individual still maintains an identity with his historic group that is salient for at least some of his activities.[9]

Recent studies of multi-ethnic developing countries have also revealed evidence of the durability and flexibility of ethnic attachments in changing social settings. Reviewing the impact of ethnic differences in West African countries, for example, Immanuel Wallerstein states,

> Thus we see that ethnic groups are defined in terms that are not necessarily traditional but are rather a function of the urban social setting. By ethnicity we mean the feeling of loyalty to this *new ethnic group of the towns.*[10]

Indeed, ethnic associations or unions often arise in urban contexts as a response to new threats or to opportunities for political or economic gain, and do not necessarily represent regression to an earlier "tribal" lifeway.[11]

If we may apply the conclusions of these studies to the Soviet case, we shall be able to speak of the persistence of ethnic identities without having to measure directly the maintenance of a broad set of traditional behaviors or beliefs. If the endurance or strengthening of ethnic identity does not necessarily involve the adherence to traditional cultural patterns (although it may involve the invocation of or attachment to certain traditional group symbols), it does invariably involve a subjective sense of belonging to the ethnic group.[12]

Operationally, the maintenance of ethnic identity is measured by examining aggregate data showing the degree of attachment to two symbols of the ethnic group: the group name and its historic language. These two foci of national identity were selected for two reasons: They are the only available indicators that allow systematic comparisons of several Soviet ethnic groups; and they are usually taken by Soviet scholars as key indicators of ethnic attachment.

Like the Western scholars cited above, several Soviet scholars have come to recognize national or ethnic consciousness (self-identification) as a self-standing and extremely persistent indicator of nationality. One leading Soviet ethnographer recently wrote:

> Having achieved a definite stage of development, ethnic self-consciousness, like other ideological forms, can acquire a certain independent existence. It can in particular be preserved even when there is a territorial and economic break-off of separate groups of people from the basic ethnic nucleus and when there is a loss of their native language. It can grow under the influence of ideas, even when its material preconditions, such as a terri-

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[8] Daniel Glaser, "Dynamics of Ethnic Identification," *American Sociological Review,* 23 (February, 1958), 32.

[9] See especially Erich Rosenthal, "Acculturation Without Assimilation? The Jewish Community of Chicago, Illinois," *American Journal of Sociology,* 66 (November, 1960), 275–288; Milton M. Gordon, *Assimilation in American Life: The Role of Race, Religion, and National Origins* (New York: Oxford University Press, 1964); Nathan Glazer and Daniel Moynihan, *Beyond the Melting Pot: The Negroes, Puerto Ricans, Jews, Italians, and Irish of New York City* (Cambridge, Mass.: MIT Press, 1963); Vladimir C. Nahirny and Joshua A. Fishman, "American Immigrant Groups: Ethnic Identification and the Problem of Generations," *The Sociological Review,* 13 (November, 1965), 311–326; and Michael Parenti, "Ethnic Politics and the Persistence of Ethnic Identification," *American Political Science Review,* 61 (September, 1967), 717–726.

[10] Immanuel Wallerstein, "Ethnicity and National Integration in West Africa," *Cahiers d'études africaines,* 3 (October, 1960), 133.

[11] Abner Cohen, *Custom and Politics in Urban Africa: A Study of Hausa Migrants in Yoruba Towns* (Berkeley: University of California Press, 1969); and Audrey C. Smock, *Ibo Politics: The Role of Ethnic Unions in Eastern Nigeria* (Cambridge, Mass.; Harvard University Press, 1971).

[12] For a similar argument, see Paula G. Rubel, "Ethnic Identity Among the Soviet Nationalities," in *Soviet Nationalities Problems,* ed. Edward Allworth (New York: Columbia University Press, 1971), pp. 217–218 and 222–223.

torial or linguistic community, are destroyed. Having achieved a definite stage of development, it is capable of reciprocal influence on the factors that gave rise to it.[13]

Along with awareness of belonging to a group or attachment to the group name, native language is often taken by Soviet scholars to be one of the most stable of ethnic indicators. To be sure, it is anticipated that the "internationality language of communication" of the USSR, the Russian language, will ultimately be adopted as a native tongue by all Soviet nationalities. But this process is not expected to be completed until communism is achieved on a worldwide scale.[14]

Soviet theoreticians are acutely aware of language as a focus of national identity and of nationalist resistance to official policies. One linguist has even asserted that "national feeling, ethnic consciousness, is largely based on the native language. This is understandable, since in just this language man hears the first words of his mother."[15] While most Soviet scholars usually maintain that national self-consciousness is distinct from and more enduring than native language, the two indicators of national identity are acknowledged to be closely connected. One recent detailed study of linguistic practices in Soviet Latvia reported that

> Linguistic assimilation leads not only to a change of native language but also to serious changes in national self-consciousness. National language and national self-consciousness are closely related ethnic determinants. Change of the native language, although not necessarily signifying restructuring of national self-consciousness, still testifies to deep ethnic changes, to the development of assimilative processes. An incongruence of linguistic and ethnic affiliation inevitably leads to a change of national self-consciousness.[16]

Following the precedent established by Soviet scholars, we shall treat the nationality label and native language as two basic indicators of national identity in the Soviet Union. To the extent, therefore, that the two indicators coincide in the same individuals, we shall say that national identities are being preserved. To the extent that they diverge, we shall say that ethnic identities are changing or weakening. Furthermore, we shall assume that when the "national language" is not nominated as one's "native language" in the census, the individual is at least partially assimilated by the group whose language he names.

Before proceeding to the development and testing of specific hypotheses concerning ethnic identity change, two limitations of the present measure of identity should be noted: (1) it registers only linguistic change; and (2) it does not reflect *intensity* of identity very well.

First, if individuals change *both* their national self-designation and native language, their ethnic identity also changes (by definition). Unfortunately, our data do not record such simultaneous or complete changes. Our measures of attachment to the national group and the native language are based on Soviet census materials in which such affiliations are designated solely by the respondent to the census (except for children, whose nationality and language are declared by their parents). We can assess how consistently persons identifying with a given nationality also identify with the corresponding national language at a given point in time; but we cannot measure previous simultaneous changes in both indicators. For example, a person of Ukrainian parentage who adopts Russian as his native language and prefers to call himself a Russian will be registered in the census records as a Russian speaking the Russian language. His Ukrainian ancestry would thus be "hidden" by the census figures. We therefore undoubtedly underestimate the extent of identity change of Soviet nationality groups. However, for reasons given below, actual changes in self-designated nationality are likely to be strongly constrained (though not eliminated) by Soviet administrative practices; so the error of measurement owing to this problem may not be excessive. In any case, at present we cannot compensate for this source of error.[17]

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[13] Kozlov, *Dinamika chislennosti narodov*, p. 48. M. S. Dzhunusov also emphasizes the importance of ethnic self-consciousness, although he imbues the term with somewhat broader meaning than does Kozlov. While Kozlov tends to restrict the usage to "attachment to the group," Dzhunusov interprets ethnic consciousness more or less as ethnic ideology. See "Teoriia i praktika razvitiia sotsialisticheskikh natsional'nykh otnoshenii," *Voprosy filosofii*, 1967, No. 9, pp. 26–36. See also I. S. Gurvich, "Nekotorye problemy etnicheskogo razvitiia narodov SSSR," *Sovetskaia etnografiia*, 1967, No. 5, pp. 62–77.

[14] In the short run, however, different "fates" are envisioned for the languages of the numerically small peoples than for the languages of the larger ones. Languages of Union Republic nationalities are considered to be destined for further functional development as their users advance socially and economically. Languages of nationalities of Autonomous Republics or lesser status national regions, on the contrary, are finding their further functional development limited. Many spheres of activity there (science, technology, government, education) are increasingly being serviced by the Russian language as the "internationality language." On this see especially Iu. D. Desheriev and I. F. Protchenko, *Razvitie iazykov narodov SSSR v sovetskuiu epokhu* (Moskva: "Prosveshchenie," 1968), pp. 121–125; and T. Iu. Burmistrova, *Teoriia sotsialisticheskoi natsii* (Leningrad: Izdatel'stvo Leningradskogo universiteta, 1970), pp. 46 and 65. Desheriev and his colleagues have developed the model of functional development of Soviet languages in a number of works. The volume cited here presents the model in its fullest form.

[15] M. I. Isaev, "Natsiia i iazyk," *Voprosy istorii*, 1968, No. 2, p. 106.

[16] A. I. Kholmogorov, *Internatsional'nye cherty sovetskikh natsii (Na materiialakh konkretno-sotsiologicheskikh issledovanii v Pribaltike)* (Moskva: "Mysl'," 1970), p. 141. For similar arguments or assumptions, see M. N. Guboglo, "Vzaimodeistvie iazykov i mezhnatsional'nye otnosheniia v sovetskom obshchestve," *Istoriia SSSR*, 1970, No. 6, p. 24; V. I. Kozlov, *Dinamika chislennosti narodov*, p. 26; and T. A. Bertagaev, Iu. D. Desheriev (supervisor), M. I. Isaev et al., "Rol' russkogo iazyka v razvitii slovarnogo sostava iazykov narodov SSSR," in *Voprosy terminologii (Materiialy vsesoiuznogo terminologicheskogo soveshchaniia)* (Moskva: "Nauka," 1961), p. 34.

In combination with nationality, language will be the only indicator of national identity employed here. Other symbols of national identity (e.g., attachment to the native territory, to historical myths or figures, or to the religious community) might be employed were alternative data available. But in any event, native language is usually treated by Soviet scholars as a core symbol of ethnic identity.[18]

A second limitation in the measure of ethnic identity is that in speaking of an identification with a group or with group symbols we cannot determine how salient such identifications are to the individual, or in what spheres of his social activity his national identity might be manifested. That he is given a chance to name his native language (*rodnoi iazyk*) and his nationality (*natsional'nost'*) in the census means that he makes these choices regardless of whether or not it is very important to him that he thinks of himself as an Uzbek, a Tatar, or whatever title he may give. In fact, we do not assume that the maintenance of a specific ethnic identity necessarily precludes an individual's simultaneous attachment to other political or social units, as, for example, the Soviet state or people, "Muslim peoples," or "Slavic-speaking peoples." In specific circumstances, in fact, a variety of alternative attachments might gain saliency. But if one retains one's traditional national identity one does have fewer affinities with the larger community or with other groups than one might have had. And this difference from others, along with the awareness of being different, is probably reflected in certain social activities in which the person engages.

But if we cannot actually determine the saliency of the individual's national identity relative to his other attachments, we can at least assume that he is aware that he is "different" from other groups in the USSR—and that as a rule it is not just the census questionnaires that stimulate such awareness. In a period of extensive population movement, mass education, and mass communications, the non-Russian is continually exposed both to his own and to other languages and peoples. Furthermore, his nationality is registered in his work record, the personnel files of employers, his internal passport, and other official papers. In this sense, moreover, his nationality is "fixed for life." On receiving his passport at age 16, when he declares his official nationality, he declares it until his death. It cannot be changed. It is also important to recognize that the child's choice of nationality on the passport is not completely independent: it must coincide with the nationality of at least *one* of his parents. Only the child of a mixed marriage can therefore officially assimilate.[19] The permanence of one's

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[17] Rough approximations might be made of losses due to complete identity change in recent years on the basis of a comparison of actual 1970 census totals with estimates based on 1959 census figures and fertility and mortality rates for the intervening years. Such estimates would be very crude, however, because there are no published figures on mortality and fertility rates for specific *nationalities* but only rates for *regions* (provinces and republics), which are usually ethnically mixed. Even more severe problems are presented if one wishes to project the extent of assimilation for earlier years, because of a similar lack of fertility rates, compounded by the severe disruptions due to population and border shifts and war losses between the censuses of 1926 and 1959.

[18] When examining language identity preservation, this analysis ignores changes that may be taking place in the languages themselves. It may be misleading to speak of "preservation" of a national language when the language itself may be only recently standardized or created as a self-standing language. Moreover, many spheres of non-Russian languages are heavily infused with Russian loan words. It has been estimated, for example, that in many non-Russian languages, 70 to 80 percent of the new technical-scientific, socio-political, educational, and other specialized terms consist of borrowings from the Russian language or through it. (Bertagaev et al., "Rol' russkogo iazyka," p. 14.) Nevertheless, Soviet linguists do not envision *fusion* of Russian and non-Russian languages but rather eventual *displacement* of national languages by a single internationality language. It is therefore appropriate to treat identification with a particular language and its literary heritage as a meaningful indicator distinguishing speakers of one language from those of another.

[19] For a description of the contents of the passport and for an explanation of how the entry on nationality is determined, see P. I. Kudriavtsev, Chief ed., *Iuridicheskii slovar'*, 2nd ed. (Moskva: Gosudarstvennoe izdatel'stvo iuridicheskoi literatury, 1956), II, 102–103.

It is worth noting that recent studies of the choice of nationality by children of ethnically mixed parents do not reveal an overwhelming attraction to the Russian nationality. Indeed, some evidence points to a process of *reverse* assimilation of Russians by the local nationalities. For example, L. N. Terent'eva official nationality probably serves to reinforce and reduce the likelihood of change in one's subjective national identification later in life. Moreover, the fixed quality of the official nationality probably constrains the respondent's choice of nationality in the census, since it is possible that the person who fails to name his official nationality as his subjective nationality will believe he is open to sanctions or criticism.[20]

Despite certain limitations in presently available measures of ethnic identity in the USSR, these measures do have an important virtue: they have been used in three Soviet censuses (1926, 1959, and 1970). Although the data base for the present analysis is almost exclusively limited to the 1959 census, the use of the present measures will facilitate subsequent longitudinal comparisons within and across groups. Indeed, it is especially important to develop the tools now for future analyses of Soviet census materials, because the publication of the 1970 census results in the next year will offer significant opportunities for analysis of longitudinal trends in identity change.[21]

Social Mobilization and Russification: Some Hypotheses

Karl W. Deutsch has linked the process of language change to that of "social mobilization," "the process in which major clusters of old social, economic and psychological commitments are eroded or broken and people become available for new patterns of socialization and behavior."[22] The weakening of traditional family and village solidarities; the exposure to mass communications and modern technology; and the development of new residential patterns, habits, and expectations, would all seem to prepare the ground for identity change. As Deutsch observes, a person can assimilate without being mobilized, but assimilation is likely to be accelerated if social mobilization erodes traditional attachments.[23]

One might therefore expect resocialization of non-Russians into modern life-styles to be accompanied in many cases by adoption of what would be perceived as the language that affords the greatest possibilities for upward social mobility: Russian. The high level of official legitimation accorded the learning of Russian and the practical advantages of speaking it are more likely to impress the mobilized individual than the unmobilized one, since the former will have higher career expectations than the latter and will be less influenced by either traditional ties to the local community or traditional animosities to Russians.[24] We may formalize the above argument into a hypothesis:

Hypothesis 1: Urban non-Russians will be substantially more Russified than rural non-Russians.[25]

Nevertheless, there is likely to be variation from one nationality to the next in the level of Russification of their mobilized populations. A major factor that may account for such differences is the level and type of contact with Russians that the mobilized native actually experiences. Clearly the incentives to learn Russian will be fewer if the individual can expect

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showed that in the capital cities of the Baltic republics, children of mixed Russian-native parents selected the *local* nationality in a majority of cases—though the frequency also depended on whether the father or mother was Russian, since children tend to favor the father's nationality. (See L. N. Terent'eva, "Opredelenie svoei natsional'noi prinadlezhnosti podrostkami v natsional'no-smeshannykh sem'iakh," *Sovetskaia etnografiia,* 1969, No. 3, pp. 20–30.) Similar patterns have also been noted in the Tatar ASSR and the North Caucasus. (See O. I. Shkaratan, "Etnosotsial'naia struktura gorodskogo naseleniia Tatarskoi ASSR," *Sovetskaia etnografiia,* 1970, No. 3, p. 15; and G. A. Sergeeva and Ia. S. Smirnova, "K voprosu o natsional'nom samosoznanii gorodskoi molodezhi," *Sovetskaia etnografiia,* 1971, No. 4, pp. 86–92.) Such reverse assimilation is probably largely confined to the native republics and provinces, however.

[20] For a discussion of the validity and reliability of the census questions on nationality and language, see S. I. Bruk and V. I. Kozlov, "Etnograficheskaia nauka i perepis' naseleniia 1970 goda," *Sovetskaia etnografiia,* 1967, No. 6, pp. 3–20.

[21] For a discussion of the contents of the 1970 census, see I. Zinchenko, "Voprosy natsional'nosti i iazyka pri perepisi naseleniia SSSR," *Vestnik statistiki,* 1969, No. 11, pp. 53–58.

[22] Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review,* 60 (September, 1961), 494.

[23] See Karl W. Deutsch, *Nationalism and Social Communication: An Inquiry into the Foundations of Nationality,* 2nd ed. (Cambridge, Mass.: MIT Press, 1966), pp. 162–163. On this point see also V. I. Kozlov, *Dinamika chislennosti narodov,* pp. 332–334; and John J. Gumperz, "Language and Communication," *The Annals of the American Academy of Political and Social Science,* 373 (September, 1967), 228.

[24] For discussions of the motivational and aspirational levels of Soviet urban and rural residents (though not specifically of the non-Russian nationalities) see Ellen Mickiewicz, "Uses and Strategies in Data Analysis of the Soviet Union: Cleavages in Industrialized Society," in *Handbook of Soviet Social Science Data,* ed. Ellen Mickiewicz (New York: Free Press, 1973), pp. 4–16; Murray Yanowitch and Norton T. Dodge, "The Social Evaluation of Occupations in the Soviet Union," *Slavic Review,* 28 (December, 1969), 619–643; and M. N. Rutkevich and F. P. Filippov, *Sotsial'nye peremeshcheniia* (Moskva: "Mysl'," 1970), pp. 237–238.

[25] Once again it should be emphasized that when measuring Russification this analysis measures only *linguistic* Russification, not complete loss of original national identity.

to work in a great variety of urban occupations without having to know Russian. The practical need to know Russian is far smaller in certain cities (such as some of the major cities of the Transcaucasus) than in others (such as the industrial centers of the Ukraine). Moreover, if the native has little opportunity to interact extensively with Russians, the possibility of his learning to speak their language fluently will be greatly reduced. We may, then, state a second hypothesis as follows:

Hypothesis 2: The level of Russification of urban non-Russians will be directly related to their level of contact with Russians.

We do not wish to imply that Russians must comprise a numerical majority (or that there is a certain required threshold in exposure to Russians) in order for assimilation to occur. Rather the variety of incentives to Russify associated with upward social mobility, in combination with the *possibility* of Russification afforded by the presence of substantial (but not necessarily predominant) numbers of Russians, should encourage Russification of a large number of natives.[26]

The same logic would apply as well to *rural* populations. That is, *if* there is any Russification of rural non-Russians it is more likely to occur among those who have a high level of contact with Russians than among those with little direct contact with Russians. But ruralites should not be as susceptible to Russification as urbanites, because the former will have lower career aspirations and greater attachment to traditional values than the latter. Therefore, one might generalize Hypothesis 2 as follows:

Hypothesis 2a: The level of Russification of both urban and rural non-Russians will be directly related to their level of contact with Russians; however, a given level of exposure to Russians will have a far greater Russification impact on urban than on rural residents.

At present we cannot directly assess the effect on linguistic Russification of interpersonal contacts between Russians and natives. Although actual Russification (as distinguished from the acquisition of Russian as a second language) requires substantial day-to-day contact with Russians and the Russian language, we are unable to measure such contact directly. The published census materials do not break down the ethnic composition of either the rural populations by town or farm, nor of urban residents by city or borough. Nothing analogous to the census-tract reporting unit of the United States census has been utilized in published Soviet census materials. So we cannot look for probable ethnic interaction on the basis of residential proximity. Nor do we have adequate information about interaction at the workplace: no occupational breakdowns by ethnic group are reported in the 1959 census. Instead we must rely on a more indirect measure of contact—namely, the ethnic composition of the entire urban or rural population of a given national republic or province. Although exposure to Russians as measured by the "Russian percentage of the (urban or rural) population of the national area" does not measure exposure or interethnic contact very precisely, it does at least give us a rough estimate of the probability of or opportunity for such contact.[27]

In this paper we shall examine the impact of urbanization and "exposure to Russians" only on those non-Russians residing inside their official national territories. Although residence outside the native areas, especially in the Russian SFSR, has a strong Russification effect on all nationalities, examination of the effect of geographical mobility is beyond the scope of the present analysis. All of the hypotheses and major conclusions stated here, however, can be applied as well to "migrants," though the magnitudes of the relationships do of course differ for "migrants" and "insiders."

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[26] Although this study will continue to hypothesize that social mobilization encourages Russification, Karl Deutsch has observed that as a rule the rate of mobilization outruns that of assimilation. Rapid mobilization tends to destroy the unity of multinational states because with increasing education and exposure to mass communications in their native tongues, members of each nationality become more aware of their differences from other groups and of their affinities with persons of kindred language or heritage. Thus the effect of social mobilization may be to exacerbate ethnic conflict rather than to lessen it. See Deutsch, *Nationalism and Social Communication*, p. 126. On this same point see also Joshua A. Fishman, "Language Maintenance and Language Shift as a Field of Inquiry," *Linguistics*, No. 9 (November, 1964), 52–53. For a similar argument applied to the Soviet case, see Richard Pipes, "The Forces of Nationalism," *Problems of Communism*, 13 (Jan.-Feb., 1964), 1–6.

Furthermore. one cannot rule out the possibility that both assimilation and strengthening of national identities may occur simultaneously among different segments of the mobilized population. One Soviet geographer has noted the dual role of cities as centers of both ethnic rapprochement (*sblizhenie*) and ethnic consolidation. (V. V. Pokshishevskii, "Etnicheskie protsessy v gorodakh SSSR i nekotorye problemy ikh izucheniia," *Sovetskaia etnografiia*, 1969, No. 5, pp. 4–5.) Moreover, resistance to assimilation may in fact be a *reaction* by certain social groups, such as the national intelligentsias or political leaders, to assimilatory pressures or to evidence of assimilation. We should remain aware, therefore, that mobilization processes may retard as well as hasten the rate of assimilation, and that the aggregate measures of mobilization and assimilation employed here are not fully sensitive to these complex social processes.

[27] On some additional drawbacks to such a measure, see Joshua A. Fishman, "Sociolinguistic Perspective on the Study of Bilingualism," *Linguistics*, No. 39 (May, 1968), 31–34.

In addition to the variables "urbanization" and "exposure to Russians," we shall examine the impact of two other factors that should mediate between those variables and Russification: sex and traditional religion. First, we would expect males to be more susceptible to Russification than females are for a number of reasons: (a) men more frequently come into contact with Russians at the workplace, since a larger proportion of men than of women is employed outside the home; (b) men have had more formal education than women and hence have had greater exposure to Russian in schools; and (c) most men serve in the armed forces, where they are exposed to Russian (as the language of command) and Russians. Therefore, we may state a third hypothesis as follows:

Hypothesis 3: Male non-Russians will be more Russified than females, both among urban and rural residents.

Contrary to this hypothesis, however, one might expect the rapidly increasing education and labor-force participation of Soviet women to hold the male-female gap in Russification to a minimum.

The final factor that we shall consider is that of religious heritage. Soviet nationality groups are not equally susceptible to Russification, partly because of differential urbanization levels and exposure to Russians, but also because many groups probably retain some elements of an ethnic ideology that brands Russification undesirable. As they seek to explain the differential birth rates of Soviet nationality groups, Soviet researchers have come increasingly to concede that certain traditional beliefs relating to family or domestic life, including some religiously based ones and those encompassed by the general term *byt* (day-to-day customary activity), are especially tenacious. In explaining the far higher birth rates of Soviet (formerly) Muslim groups compared to those of Christians, these scholars note the Muslim traditions of early marriage, high valuation of childbearing, and dislike of birth control methods. These traditions are still important even among urban populations (though birth rates are much higher in rural than in urban populations).[28]

If it is reasonable to conclude that traditional customs help maintain birth-rate differentials, it seems equally plausible that Muslims might to a degree preserve traditional prohibitions against close interpersonal contacts with non-Muslims. What little evidence there is concerning intermarriage rates between Muslims and non-Muslims in the USSR suggests that these are still quite low, especially compared with intermarriage rates between Russians and most Orthodox groups.[29] Although nowadays Russification is not the same as Christianization, a remnant of historically founded opposition to the Russian may nonetheless survive in mass beliefs that have since lost their strictly religious connotations.[30] Such beliefs will be referred to here as a "Muslim ethnic ideology."

To understand how a Muslim ethnic ideology might affect Russification, it will be useful to cite a conclusion of the sociolinguist Joshua A. Fishman, who states that

> We have little systematic information concerning the circumstances under which language consciousness, language knowledge and language-related groupness perceptions do or do not enter into reference group behavior in contact situations. As

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[28] See, for example, G. A. Bondarskaia, "Rol' etnicheskogo faktora v formirovanii territoriial'nykh razlichii rozhdaemosti," in *Voprosy demografii (Issledovaniia, problemy, metody)*, ed. A. G. Volkov et al. (Moskva: "Statistika," 1970), p. 171; M. K. Karakhanov, "Dinamika narodonaseleniia srednei azii," pp. 23–4; and M. P. Melamed, "Vosproizvodstvo naseleniia v Turkmenskoi SSR i vliianie na nego faktory," pp 95–8, both in *Problemy narodonaseleniia*, M. K. Karakhanov, chief ed. (Moskva: Izdatel'stvo Moskovskogo universiteta, 1970). On the preservation of other traditional customs relating to family life, see also Richard Pipes, "Assimilation and the Muslims: A Case Study," in *Soviet Society: A Book of Readings*, ed. Alex Inkeles and Kent Geiger (Boston: Houghton Mifflin, 1961), pp. 593–595.

[29] It is difficult to find truly comparable data for several groups. Figures for intermarriage between Tatars and Russians in Kazan and figures for marriage between Ukrainians and Russians in Kharkov reveal that the former rate is far lower than the latter despite similar overall proportions of Russians and natives in the two cities. See (on Kazan) K. Kh. Khanazarov, "Ob odnom aspekte dal'neishego sblizheniia natsii v SSSR," *Obshchestvennye nauki v Uzbekistane*, 1968, No. 6, pp. 23–26; and (on Kharkov) M. V. Kurman and V. Lebedinskii, *Naselenie bol'shogo sotsialisticheskogo goroda* (Moskva: "Statistika." 1968), p. 126.

[30] Of the Muslim intellectual in the Soviet Union today, Bennigsen and Lemercier-Quelquejay have written: "He may be indifferent to religious dogma and practice, but he remains attached to the whole body of customs and traditions which make up the special character of the Muslim way of life." This continuing attachment to the community of believers is symbolized by the universally practiced rite of circumcision; moreover, intermarriage is strongly frowned upon. See Alexandre Bennigsen and Chantal Lemercier-Quelquejay, *Islam in the Soviet Union*, trans. Geoffrey E. Wheeler and Hubert Evans (New York: Praeger, 1967), p. 213.

a result it is difficult to say at this time whether or when language maintenance and language shift are ideologically mediated as distinguished from their more obvious situational and instrumental determinants.[31]

In the hypotheses presented earlier, we proposed to examine (in Fishman's terms) certain "situational and instrumental determinants" (urbanization and exposure to Russians) of language shift. But among the Muslim groups in particular one might expect a "Muslim ethnic ideology" to function as an "ideological mediator" between the situational factors and Russification. Therefore, we would hypothesize the following:

Hypothesis 4: Muslims will be far less susceptible to Russification than non-Muslims. In particular, whatever the level of exposure to Russians, both urban and rural Muslims will be less likely to become Russified than non-Muslims will be.

Statistical Procedures

The data base for the present analysis is the 1959 USSR census report.[32] All non-Russian nationalities with an official national territory of Autonomous Oblast or higher status are included, with the addition of the Komi-Permiak National Okrug. This provides an N for most purposes of 46 nationalities. It should be noted that a few relatively small groups share a national territory with others, so that the total number of national *areas* in the analysis is 38.[33] Since this study examines the Russification only of that portion of each nationality residing within its official national territory, excluded from the statistical analysis are such "non-native" groups as the Jews, Germans, Greeks, and Poles, as well as the "native" Crimean Tatars—all groups which do not now have a recognized national homeland within the USSR.[34]

The data are drawn directly from tables in the 1959 census report which list figures on native language for each nationality residing in a given region. The populations of each nationality are further subdivided according to two variables: urban-rural and male-female. For each table category, the actual percentage of the given nationality that claims Russian or the traditional national language as native is reported. Thus we do not have to contend with the problem of making inferences only from the "marginals" for the entire population on the urban-rural, male-female, and language variables.

In order to arrange these data so that they could be subjected to sophisticated statistical techniques, a somewhat unusual coding procedure has been followed. The effective unit of analysis is not the nationality but rather the *table cell.* Each of the four table cells defined by the dichotomous urban-rural and male-female variables is treated as a separate case or observation. Since 46 nationalities are examined, a total of $4 \times 46 = 184$ observations comprises the entire data to be analyzed.

The coding procedure for the independent variables "urban-rural" and "male-female" is as follows. First, the two variables are coded according to the usual dummy variable procedures: the quality "urban" is assigned a value of 1; "rural," a value of 0. "Male" is coded 1; "female," 0. Then each table cell (observation) is scored on the two variables. For example, the urban-male table cell is assigned a value of 1 on the urban-rural variable and a value of 1 on the male-female variable; while the urban-female cell is assigned a value of 1 on the urban-rural variable and a value of 0 on the male-female variable. A similar procedure is followed in assigning values to each of the four observations for every nationality.[35]

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[31] Joshua A. Fishman, "Language Maintenance and Language Shift," 62.

[32] Tsentral'noe statisticheskoe upravlenie, *Itogi vsesoiuznoi perepisi naseleniia 1959 goda,* 16 vols. (Moskva: "Gosstatizdat," 1962–3). There is one volume for the USSR as a whole and one for each Union Republic.

[33] Specifically, in the North Caucasus one finds the bi-national Chechen-Ingush ASSR, Kabardino-Balkar ASSR, and Karachaevo-Cherkess AO, as well as the multi-ethnic Dagestani ASSR, for which six nationalities have been included in the analysis: Avarians, Darginians, Laks, Lezghians, Kumyks and Nogais.

[34] The Germans and Crimean Tatars did have national territories until the Second World War, at which time the territories were abolished and the groups deported to Siberia and Central Asia. The Jews nominally have their own Jewish Autonomous Oblast in the Far East as a cultural homeland, but the population of this province, which was formed in the late 1920's (and formally designated the Jewish AO in 1934) has never been dominated numerically by Jews and can hardly be considered a homeland of Soviet Jews. For a description of the founding of the Jewish AO and its population makeup, see Solomon Schwarz, "Birobidzhan: An Experiment in Jewish Colonization," in *Russian Jewry: 1917–1967,* ed. Gregor Aronson et al. (New York: Thomas Yoseloff, 1969), pp. 342–395.

By excluding groups having no official homeland one is in effect controlling for a significant variable in the Soviet context: the amount of official recognition granted the groups, which is associated with the granting of native-language schools, publications, and representation within the state and Party hierarchies. Simultaneously one is also focusing attention on population groups that are still relatively compactly settled within a traditional homeland, a factor that also hinders Russification.

[35] Moreover, each nationality is treated as if it

Conventional methods of coding aggregate data are employed in assigning values on the *dependent* variable (linguistic Russification). Each observation is assigned a value on the dependent variable equal to the "per cent linguistically Russified" of the given population subgroup for the given nationality. For example, if 7.5 per cent of the urban males of a given nationality were linguistically Russified, the value of the dependent variable for the urban-male observation would be 7.5 per cent. Appropriate values are thus assigned to the urban-male, urban-female, rural-male, and rural-female observations for each nationality.

The result of these procedures is a data set consisting of 184 observations on each of three variables: linguistic Russification, urban-rural, and male-female. In addition, other relevant variables have been coded for each population subgroup, e.g., "exposure to Russians," traditional religion, population size, and so forth.

The primary statistical technique used here is regression analysis, specifically ordinary least-squares regression techniques, including dummy-variable techniques in which some of the independent variables are dichotomous (dummies) and are assigned a value of 1 or 0.[36] Unless stated otherwise, all regression coefficients reported here are unstandardized.[37]

Analysis

Urbanization and Russification. The first hypothesis states that urban non-Russians will be substantially more Russified that rural non-Russians. In examining the evidence for or against this proposition we must first recall that the *theoretical* variable we are measuring by the urban-rural indicator is social mobilization. We are assuming that urban residence and rural residence are proxy variables for "mobilized" and "unmobilized." In viewing the rural population, however, we must recognize that our proxy is imperfect because some rural residents will have originated in urban areas or will have had a substantial amount of education, high exposure to the mass media, and so forth. Conversely, some urban residents will be recent migrants to the city or will have had little education. Nevertheless, the available data should give an approximation of the basic impact of mobilization on Russification.[38]

Preliminary evidence does tend to support the first hypothesis: on the average, only 1.2 per cent of rural non-Russians are linguistically Russified, as opposed to 7.9 per cent of the urban non-Russians.[39] Given the expectation that at least some rural inhabitants will not be native to rural areas (perhaps they will be working in skilled or administrative positions) and that many ruralites will have received a good deal of schooling, this figure of 1.2 per cent is indeed low. Further investigation shows that of the 46 nationalities, 32 of the rural sub-groups are less than 1 per cent Russified; 8 fall between 1 and 2 per cent; 1 is between 2 and 3 per cent; and only 5 are greater than 3 per cent Russified (see Table 1). The five cases exceeding 3 per cent Russified are the Karelians (10.4%), Khakasy (7.8%), Altais (6.6%), Komi Permiaks (6.1%), and Udmurts (3.2%). All of these groups have in common very high proportions of Russians in the rural populations of their official national areas, and an adherence to the Russian Orthodox religion dating from their conversion during the Tsarist era. Nonetheless, only the Altais and Khakasy, small Turkic-language peoples inhabiting Western and Eastern Siberia respectively, showed much evidence of linguistic

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were equal in size and importance to others, and each subcategory of the population is treated as if it, too, were equal in size with others defined by the independent variables. The groups are not weighted according to population size because of the inordinate population sizes of a few groups such as the Ukrainians (37 million in 1959), Belorussians (8 million), and Uzbeks (6 million), which together account for 60 per cent of the non-Russian population. The proliferation of small nationalities in the analysis might have been avoided if one wanted to focus on the larger or politically more important groups. However, since this is the first systematic statistical analysis of these data, and since the census data were coded as part of a larger project examining various aspects of ethnic change in the Soviet Union, it was thought desirable to include all ethnic groups for which substantial statistical data could be gathered in both census and non-census sources. This meant taking all groups whose official national areas have Autonomous Oblast status or higher, as well as all major groups officially recognized as native to these regions.

[36] For a description of the use of dummy variables, see Jacob Cohen, "Multiple Regression as a General Data-Analytical System," *Psychological Bulletin,* 70 (1968), 426–443; and Potluri Rao and Roger L. Miller, *Applied Econometrics* (Belmont, Cal.: Wadsworth, 1971), pp. 88–99.

[37] On the justification for using unstandardized rather than standardized coefficients see Hubert M. Blalock, Jr., "Causal Inferences, Closed Populations, and Measures of Association," *American Political Science Review,* 61 (March, 1967), 130–136.

[38] There are substantial data available pertaining to levels of education and the distribution of professional manpower among the nationalities, but these data are not directly cross-tabulated with language usage, and employment of them could lead to faulty "ecological inferences."

[39] All "averages" given in this paper are computed on the basis of group-level data. In addition, unless stated otherwise, all such figures will be based on the total *N* of 46 nationalities.

Table 1. Linguistic Russification of Non-Russian Groups Inhabiting their Official National Areas: 1959

(Percentage Russified)

	Rural	Urban	Total
UNION REPUBLIC NATIONALITIES			
East Europe			
Ukrainians	1.3	15.3	6.5
Belorussians	1.4	22.4	6.8
Moldavians	0.5	8.6	1.3
Baltic			
Estonians	0.2	1.2	0.7
Latvians	1.0	2.0	1.5
Lithuanians	0.0	0.3	0.1
Transcaucasus			
Georgians	0.1	1.1	0.4
Armenians	0.1	1.3	0.7
Azerbaidzhanis	0.1	2.1	0.8
Central Asia			
Kazakhs	0.4	1.8	0.8
Uzbeks	0.1	1.4	0.3
Kirgiz	0.1	1.2	0.2
Turkmenians	0.0	1.9	0.8
Tadzhiks	0.1	1.7	0.4
ASSR AND AO NATIONALITIES (in RSFSR unless indicated)			
Northwest			
Karelians	10.4	38.0	19.0
Komi (Zyrians)	2.9	15.6	6.2
Volga-Viatka			
Tatars	0.4	2.9	1.1
Bashkirs	0.5	4.4	1.0
Chuvash	0.7	15.6	2.5
Mordvinians	1.5	21.6	2.7
Mari	1.2	15.5	2.2
Udmurts	3.2	22.7	6.8
Komi Permiaks	6.1	21.9	7.6
North Caucasus			
Kalmyks	1.6	2.5	1.8
Chechens	0.1	1.9	0.3
Ingush	0.2	2.8	0.5
Kabardians	0.3	3.8	0.7
Balkars	0.5	3.2	0.9
Karachai	0.3	2.2	0.4
Adyghe	0.6	4.9	1.0
Cherkess	0.4	5.6	0.7
N. Ossetians	0.6	5.0	2.0
Avarians	0.1	3.2	0.4
Lezghians	0.2	7.2	1.0
Darginians	0.1	2.8	0.5
Kumyks	0.2	2.0	0.8
Laks	0.2	4.9	1.4
Nogai	0.5	1.9	0.6
Siberia			
Tuvinians	0.4	4.1	0.7
Altais	6.6	35.0	8.4
Khakasy	7.8	21.9	9.8
Yakuts	0.7	7.3	1.8
Buriats[a]	1.5	12.0	3.2
Others			
Karakalpaks (Uzbek SSR)	0.0	0.5	0.1
Abkhazians (Georgian SSR)	0.5	6.5	2.4
S. Ossetians (Georgian SSR)	0.1	1.0	0.0
MEAN LEVEL FOR ALL GROUPS:	1.2	7.9	2.3

Source: Tsentral'noe statisticheskoe upravlenie, *Itogi vsesoiuznoi perepisi naseleniia 1959 goda*, 16 vols. (Moskva: "Gosstatizdat," 1962–3).

[a] Figures for the Buriats are based only on the Buriat ASSR.

Russification before the Soviet era.[40] While these cases disprove the assertion that urbanization is necessary for Russification, they are atypical. If these instances show that it is possible for unmobilized populations to become Russified, the other 41 cases demonstrate that such rural Russification is highly exceptional.

Urbanization does as a rule appear to be required for Russification to take place. Moreover, in all groups the urban populations are more Russified than the rural, although the level of Russification of urban populations sometimes barely exceeds the rural. If we take the rural Russification level (1.2 per cent) as a base point, we may state that the mean "effect" of urban residence is to raise the level of Russification by 6.7 percentage points. As a reference point for later comparison, this "urban effect" may be represented by the following dummy-variable regression equation:

$$\hat{Y} = .94 + 6.68X_1 + .60X_2$$

$$\text{(std. err.)} \quad (.86) \quad (1.00) \quad (1.00), \qquad \text{(Eq. 1)}$$

$$\bar{R}^2 = .190$$

where

$\hat{Y}$ = Estimated mean percentage of non-Russians who claimed Russian as their native tongue in 1959,

X_1 = Urban-rural (1 if urban, 0 if rural),

X_2 = Male-female (1 if male, 0 if female).

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[40] According to the 1926 census report, 16.9 per cent of the rural Altais were Russified, while 7.9 per cent of the Khakass ruralites were Russified. These figures are calculated from the 1926 census report, *Vsesoiuznaia perepis' naseleniia 1926 goda*, 56 vols. (Moskva: Ts. S.U., 1928–1933). For figures extracted or derived from the 1926 and 1959 census reports page numbers will not be cited because it would have been far too cumbersome to record the pages from which figures (or bases of figures) for each nationality were derived.

Especially if we keep in view the probability of a time lag between initial settlement in the cities and Russification, the data show that urbanization has a substantial impact on the Russification of non-Russians. Such a time lag might be understood in two ways. First, the adult newly arriving in an urban setting will have an ethnic identity that is not very susceptible to change in later years. Only his children, or younger persons in general (in the 1959) census), are likely to show much evidence of Russification. Quite probably an intermediate stage of bilingualism will be traversed.[41] Second, younger persons will be more highly exposed to Russian through schools and more motivated to learn Russian to increase their job opportunities than will their less educated parents who are already established in careers. Either interpretation is supported by the data in Table 2. Among older persons, Russification is only a few percentage points greater in urban than in rural areas. Among younger persons, however, there is a substantial difference. The data do support a time-lag hypothesis. In fact, however, because we have no data on length of residency in urban areas of younger and older persons, this explanation is only tentative. Even so, recognition of the time lag does keep us from concluding that low overall levels of Russification among the non-Russian urban populations necessarily indicate the ineffectiveness of urbanization.

Table 2. Percentage of Non-Russians Who Claim Their National Language as Their Native Language, by Age: 1959[a]

Age	Residence		Difference (Rural minus Urban)
	Rural	Urban	
0–9	96.6	84.7	11.9
10–19	96.1	85.1	11.0
20–24	96.2	90.0	6.2
25–29	96.8	91.6	5.2
30–34	96.7	91.6	5.1
35–39	96.6	91.1	5.5
40–44	96.7	91.1	5.6
45–49	97.0	92.1	4.9
50–54	97.2	92.7	4.5
55–59	97.3	93.3	4.0
60–69	97.4	94.1	3.3
70+	97.5	95.0	2.5

[a] N = 35. The figures are not the "percentage Russified" because available age-specific data only record the percentage claiming the *national* language as native. The table entries are an unweighted mean of the 35 groups for whom age-specific data are available in the 1959 census volumes for the *SSSR*, pp. 211–225, and *RSFSR*, pp. 388–409. Furthermore, the figures pertain to the "areas of principal settlement" of each national group rather than to either the official native republic or the entire USSR.

Exposure to Russians and Russification. The second hypothesis suggested that the amount of contact with Russians might help explain the levels of Russification experienced by non-Russian groups. As an initial effort to assess the impact of "exposure to Russians" on the Russification of non-Russians, one may fit an additive regression model of the following form:

$$\hat{Y} = -1.57 + 3.42X_1 + .60X_2 + .11X_3$$
$$\quad\quad (.94) \quad (1.13) \quad (.93) \quad (.02)$$
$$\bar{R}^2 = .292 \quad \text{(Eq. 2)}$$

where

$\hat{Y}$ = Estimated percentage of non-Russians linguistically Russified in 1959,
X_1 = Urban-rural, X_2 = Male-female,
X_3 = Exposure to Russians: Percentage of the total population of the (urban or rural) region who are Russians.

Comparing the $\bar{R}^2$ of this model with that in Equation 1, we observe an increment of .102, i.e., a 10 percentage point increase, in the proportion of variance accounted for.[42] However, the goal here is not to exhaust the explanation of variance in the dependent variable but rather to examine the impact of particular hypothesized factors.

From Equation 2 we can observe that an increase of one percentage point in Russian exposure is associated with an increment of .11 percentage points in the linguistic Russification of the native population. Furthermore, once

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[41] Uriel Weinreich, *Languages in Contact: Findings and Problems* (The Hague: Mouton & Co., 1953), p. 94. I have not undertaken a detailed examination of generational differences in Russification because of certain imperfections in the reported data. All age-specific figures on language usage pertain to the somewhat loosely defined "areas of primary settlement" of the ethnic groups. As a rule, these areas are congruent with neither the official national areas nor the entire USSR. As a result, the age-specific figures tend to confound the effects of geographical mobility with other factors, and it is impossible to employ the age-specific figures in conjunction with the "exposure to Russians" variable (except for a few groups for whom the recorded "area of primary settlement" happens to coincide with the official national area).

[42] Since the addition of any new variable to a regression model is likely to increase the amount of variance accounted for (the R^2), the statistic $\bar{R}^2$ is generally more appropriate than R^2 for assessing the incremental change in the variance accounted for by the addition of a new variable to an equation. $\bar{R}^2$ is derived from R^2 but includes a correction or adjustment to R^2 depending upon the number of cases

"exposure" is controlled, there is still a residual effect of urban residence amounting to 3.4 percentage points. Thus the urban-rural differences in levels of Russification cannot be accounted for solely by differences in levels of exposure to Russians but involve separate contributions by both mobilization and exposure. Because the two independent variables are intercorrelated, however, it is impossible to specify precisely how much of the variance or increment in Russification should be attributed to each variable.[43] But one can specify the joint or interaction effect of the two variables.

Hypothesis 2a suggests that the "exposure effect" should be far greater among urban than among rural non-Russians, because urbanites are more susceptible to Russification than are ruralites. In statistical terms, this urban-rural difference in Russification may be understood as resulting from an interaction between exposure and urban-rural residence. Accordingly, to examine the possible urban-rural differences, separate regressions of Russification onto "Russian exposure" were computed for urban and rural populations. These are shown in Equations 3a and b.[44]

Urban

$$\hat{Y} = -.05 + \underset{(2.39)}{}\ \underset{(1.82)}{.79X_1} + \underset{(.04)}{.15X_2} \qquad \bar{R}^2 = .119 \qquad \text{(Eq. 3a)}$$

Rural

$$\hat{Y} = -.40 + \underset{(.35)}{}\ \underset{(.39)}{.42X_1} + \underset{(.01),}{.06X_2} \qquad \bar{R}^2 = .316 \qquad \text{(Eq. 3b)}$$

where

$\hat{Y}$ = Estimated percentage of non-Russians linguistically Russified in 1959,
X_1 = Male-female, X_2 = Exposure to Russians.

Two features of these equations merit attention. First of all, as hypothesized, the impact of exposure to Russians is much greater on urban than on rural non-Russians. An increase of 10 percentage points in the Russian proportion of the population is associated with a 1.5 percentage point rise in linguistic Russification of urban non-Russians but with only a 0.6 percentage point rise among rural residents.[45]

Second, the predicted level of Russification when "exposure to Russians" is zero also approximates zero. This result tends to validate the assumption that only where there is direct exposure to Russians do natives become linguistically Russified. Properly speaking, of course, one should not extrapolate the relationships in Equations 3a and b beyond the range of values actually represented in the independent variables. For example, in only two urban populations do Russians comprise less than 10 per cent: Armenia (4.5 per cent) and South Ossetia (7.2 per cent). To keep from making erroneous extrapolations, it is important to keep in mind the range and distribution of the "Russian percentages" of the urban and rural populations. These are provided in Tables 3 and 4.

Table 3. Russians as a Percentage of the Urban and Rural Populations of the Non-Russian Areas: 1959, Summary Statistics

	Mean	Median	Minimum	Maximum.
Urban	51.7	52.4	4.5	87.5
Rural	22.5	18.9	0.9	67.8

Table 4. Russians as a Percentage of the Urban and Rural Populations of the Non-Russian Areas: 1959, Distributions[a]

	Russian percentage of the population								
	0–9	10–19	20–29	30–39	40–49	50–59	60–69	70–79	80–89
Urban	2	4	2	8	1	5	4	7	5
Rural	13	7	4	5	3	2	4	0	0

[a] Cell entries are the number of cases falling into the given range.

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and variables in the equation. In effect, the adjustment penalizes the researcher who is working with a small N or a large number of regressors (relative to the N). On the derivation and justification for $\bar{R}^2$, see Rao and Miller, *Applied Econometrics*, pp. 20–21.

[43] See Robert A. Gordon, "Issues in Multiple Regression," *American Journal of Sociology*, 73 (March, 1968), 592–616.

[44] This "interaction model" might be constructed by creating a new interaction term equal to the product of "exposure" (X_3) and "urban-rural" (X_1) and adding it to Equation 2. It is also appropriate, however, to split the sample and to run separate regression analyses on the relevant population subgroups (here, urban and rural). See Rao and Miller, *Applied Econometrics*, pp. 148–150; and Emanuel Melichar, "Least-Squares Analysis of Economic Survey Data," American Statistical Association, Proceedings of the Business and Economic Statistics Section, 1965, p. 379.

[45] The product-moment correlation between Russian exposure and linguistic Russification is .37 for the urban and .57 for the rural groups.

In summarizing the above analysis, we may conclude that exposure to Russians has a significant impact on the Russification of non-Russians in urban areas but relatively little effect on rural non-Russians.[46] The hypothesis of greater receptivity to Russification among urbanites thus appears to be supported. When one considers that the average Russian percentage of the rural populations of the national areas is about 22, while the average Russian percentage of the urban populations is 52, one can project that Russification of the urban non-Russian is accelerated both by his higher exposure to Russians and by his greater susceptibility to Russification.

The assertion that Russian exposure has less of an impact in rural than in urban areas does not mean simply that rural areas have smaller proportions of Russians in them than do urban areas but that for a *given* level of exposure to Russians, ruralites are less likely to become Russified than urbanites. The actual magnitude of this difference may be estimated (from Equations 3a and b), for example, for the hypothetical case in which 50 per cent of the urban and 50 per cent of the rural population are Russians. In this instance, about 7.5 per cent of the urban natives would be linguistically Russified, while about 3 per cent of the rural natives would be Russified.

The test of the effect of exposure to Russians on the Russification process thus underlines the importance that social mobilization has in that process. It would be inappropriate, however, to attempt to assess the *relative* contributions of "urbanization" and "exposure" to Russification. Exposure to Russians does appear to be necessary if Russification is to take place among both rural and urban residents. Yet exposure alone has but a minimal influence on the Russification of non-mobilized natives. In other words, neither factor is very effective without the other. Nevertheless, the cases of a few nationalities demonstrate that over a long stretch of time ruralites *can* be assimilated. That there is a moderate correlation between exposure to Russians and Russification of rural natives would also support this conclusion.

Table 5. Mean Levels of Linguistic Russification of Male and Female Non-Russians, 1959[a]

(Percentage Russified)

	Residence	
	Urban	Rural
Males	8.3	1.4
Females	7.5	1.0
Difference	0.8	0.4

[a] $N=46$. Cell entries are the unweighted mean level of Russification of the populations of each nationality falling into the given category.

Sex Differences in Russification. The evidence for the third hypothesis generally confirms the expectation that men will be more highly Russified than women (see Table 5). On the average, urban men are .8 percentage points more Russified than urban women, while rural men are .4 percentage points more Russified than rural women. Given the small size of the average male-female differences, we shall not devote much attention to this phenomenon. In retrospect, it is probably erroneous to expect much difference between males and females in native language, especially since the participation of Soviet women in the labor force is fairly high and since the modal marital pattern is ethnic homogeneity in the family.

The male-female variable has been left in the regression equations presented thus far for two reasons. First, although one could simply combine or ignore the male-female categories for purposes of analysis, the sex variable is theoretically important. Second, as long as leaving the variable in the equations does not distort the magnitudes of the relationships between other independent variables and the dependent variable, no harm is done. In short, the male-female variable qualifies as what Rao and Miller have labeled a "superfluous variable" which may properly be included in the analysis.[47]

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[46] Note that the lower impact of Russian exposure on Russification in rural areas is gauged by the relative sizes of the *unstandardized* regression coefficients, not the standardized coefficients. Indeed, the correlation between Russian exposure and Russification is higher in the rural than in the urban areas. Consequently, Russian exposure accounts for a greater proportion of the variance in Russification of rural populations (32.4 per cent) than of urban populations (13.7 per cent).

[47] Rao and Miller, *Applied Econometrics*, pp. 35–38. For a similar argument see also Ronald J. Wonnacott and Thomas H. Wonnacott, *Econometrics* (New York: Wiley, 1970), pp. 65–66. We should add that the contribution of the male-female variable to the over-all R^2 is extremely small. Nevertheless, the differences highly consistently support the hypothesis: among rural populations, men are more Russified than women in 36 of the 46 cases, while in the other 10 cases the male-female differences are negligible (less than one-tenth of a percentage point); among urban populations, men are more Russified than women in 34 of the 46 cases, while (contrary to expectations)

Religious Heritage and Russification. Earlier, it was proposed that a "Muslim ethnic ideology" may mediate between certain dynamic factors affecting Russification and the actual emergence of Russification. Muslims as a group tend to resist Russification. Although one cannot necessarily identify an ideology especially receptive to Russification among non-Muslims taken as a whole, one would expect their sharing of a religious heritage with Russians (this would apply particularly to Orthodox groups) to reduce the likelihood of an inherited or enduring opposition to Russification (other things being equal).[48] Therefore, in statistical terms the Russification impact of both mobilization and exposure to Russians should be much smaller for Muslims than for non-Muslims. Indeed at first glance the data do tend to support this hypothesis: on the average, 3.6 per cent of the urban Muslims are Russified, compared with a figure of 12.6 per cent for the urban non-Muslims; the corresponding figures for the rural populations are 0.3 per cent for Muslims and 2.3 per cent for non-Muslims.

One might surmise that these initial figures reflect not the differential receptivity of Muslim and non-Muslim populations to Russification but rather differences in actual contact with Russians. In fact, however, the correlation between exposure to Russians and traditional religion is almost nil. On the average, there is virtually no difference in current levels of exposure for Muslims and non-Muslims in urban areas. On the other hand, in rural areas there is some correspondence, in the expected direction, between exposure and traditional religion. But even this relationship is weak.[49] In brief, differences in Russification between Muslims and non-Muslims cannot be accounted for in terms of differential levels of exposure to Russians.

Evidently, the Muslim/non-Muslim differences in Russification levels occur *despite* similar levels of exposure to Russians. This evidence supports the hypothesis that Muslims and non-Muslims are not equally susceptible to Russification—even when exposed to the same "conditions" defined by level of mobilization and exposure to Russians. As a further test of this hypothesis (Hypothesis 4), we may examine the residual difference between the level of Russification predicted from Equation 3a and the level actually displayed by each nationality; these residuals when cross-tabulated with religion will show how the inclusion of a "religion variable" in the model might affect the explanation of Russification. While the religion variable could simply be introduced into the model and its effects tested directly without going through this intermediate step, a look at the residuals should give a better feel for the statistical procedures employed here.

Table 6 presents the residuals for the urban residents only. The general relationship between the residuals and traditional religion is summarized in Table 7, which indicates the number of groups falling above or below the estimated levels of Russification—regardless of how far they may deviate from the estimate. That traditional religion has a strong impact on Russification is clear: 22 out of 23 Muslim nationalities are *less* Russified than predicted on the basis of exposure to Russians; while 14 out of 17 Orthodox groups are *more* Russified than predicted. Of the remaining six cases, the three Buddhist groups fall below the predicted levels, as do the three non-Orthodox but Christian Baltic nationalities.

In summary, Muslims as a group do not appear to be very susceptible to Russification, while except for the Georgians, North Ossetians, and Yakuts, the Orthodox groups appear to be much more susceptible. Whether one should attribute the scores of the Baltic nations to "religion" or rather to their relatively recent annexation to the USSR and strong traditions of resistance to Russification cannot be determined from these data; undoubtedly various factors are working simultaneously.[50] The case

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women are more Russified than men in 9 cases, and in the 3 remaining cases the differences are negligible. These totals are subject to a certain amount of rounding error in computing of percentages.

[48] The special susceptibility of (former) Orthodox groups to Russification (upon becoming mobilized) has been noted by Armstrong, "The Ethnic Scene," p. 16.

[49] The product-moment correlation between "Muslim-non-Muslim" and "exposure to Russians" is .06 for urban residents and —.21 for rural.

[50] John A. Armstrong has labeled the Baltic nations, along with the Georgians, Moldavians, Finns and Poles, as "state nations": "nationalities with strong traditions of national identity, including distinctive languages, well-developed cultures, and distinctive historical traditions" (Armstrong, "The Ethnic Scene," p. 21). Thus, the Balts are likely to maintain an ethnic ideology strongly distinguishing themselves from Russians.

In fact, a more general case may be made that groups with Union Republic status should be better able than groups of lesser status to preserve their ethnic identities because of (a) their relatively greater access to native-language schools and other cultural facilities; (b) their (usually) relatively rich and long-established literary traditions and histories of independence from Russian rule; (c) their relatively large population and territorial size, their location on the periphery of the USSR, and their social and economic diversity, which give them more of a claim

Table 6. Comparison of Actual Levels of Linguistic Russification with Estimates Based on "Exposure to Russians": 1959[a]

Urban Populations Only

	(1) Russian per cent of population	(2) Actual per cent of natives Russified	(3) Expected per cent Russified	(4) Difference: actual minus expected (2–3)	(5) Dominant traditional religion
UNION REPUBLIC GROUPS					
East Europe					
Ukrainians	29.9	15.3	4.7	10.6	Orthodox
Belorussians	19.4	22.4	3.2	19.2	Orthodox
Moldavians	30.4	8.6	4.8	3.8	Orthodox
Baltic					
Latvians	34.5	2.0	5.4	−3.4	Lutheran
Lithuanians	17.0	0.3	2.8	−2.5	Catholic
Estonians	30.8	1.2	4.9	−3.7	Lutheran
Transcaucasus					
Georgians	19.0	1.1	3.1	−2.0	Orthodox
Armenians	4.5	1.3	1.0	.3	Near to Orthodox
Azerbaidzhanis	24.9	2.1	4.1	−2.0	Islamic
Central Asia					
Kazakhs	57.6	1.8	8.8	−7.0	Islamic
Uzbeks	33.4	1.4	5.2	−3.8	Islamic
Kirgiz	51.8	1.2	7.9	−6.7	Islamic
Turkmenians	35.5	1.9	5.6	−3.7	Islamic
Tadzhiks	35.4	1.7	5.5	−3.8	Islamic
ASSR AND AO GROUPS					
Northwest					
Karelians	72.6	38.0	11.0	27.0	Orthodox
Komi	59.2	15.6	9.0	6.6	Orthodox
Volga-Viatka					
Tatars	61.0	2.9	9.3	−6.4	Islamic
Bashkirs	63.8	4.4	9.7	−5.3	Islamic
Chuvash	53.0	15.6	8.1	7.5	Orthodox
Mari	75.8	15.5	11.4	4.1	Orthodox
Mordvinians	82.3	21.6	12.4	9.2	Orthodox
Udmurts	74.1	22.7	11.2	11.5	Orthodox
Komi-Permiaks	39.2	21.9	6.1	15.8	Orthodox
Caucasus					
Kalmyks	60.2	2.5	9.2	−5.7	Buddhist
Chechens	77.5	1.9	11.7	−9.8	Islamic
Ingush	77.5	2.8	11.7	−8.9	Islamic
Balkars	69.3	3.2	10.5	−7.3	Islamic
Kabardians	69.3	3.8	10.5	−6.7	Islamic
Karachai	78.4	2.2	11.8	−9.7	Islamic
Cherkess	78.4	5.6	11.8	−6.2	Islamic
Adyghes	85.4	4.9	12.8	−7.9	Islamic
N. Ossetes	50.3	5.0	7.7	−2.7	Orthodox
S. Ossetes	7.2	1.0	1.1	− .1	Islamic
Abkhazians	36.5	6.5	5.7	.8	Orthodox
Avarians	43.3	3.2	6.7	−3.5	Islamic
Darginians	43.3	2.8	6.7	−3.9	Islamic
Lezghians	43.3	7.2	6.7	.5	Islamic
Laks	43.3	4.9	6.7	−1.8	Islamic
Kumyks	43.3	2.0	6.7	−4.7	Islamic
Nogais	43.3	1.9	6.7	−4.8	Islamic
Siberia					
Yakuts	72.8	7.3	11.0	−3.7	Orthodox
Altais	87.5	35.0	13.2	21.8	Orthodox
Khakasy	85.3	21.9	12.8	9.1	Orthodox
Tuvinians	79.5	4.1	12.0	−7.9	Buddhist
Buriats	84.5	12.0	12.7	− .7	Buddhist
Central Asia					
Karakalpaks	13.4	.5	2.3	−1.8	Islamic

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[a] The figures pertain only to the official national areas of the groups. The "estimated" per cent Russified (Column 3) is from Equation 3a. Male-female estimates are averaged to obtain the estimates in Column 3.

Table 7. Nationalities Exceeding or Falling Short of Levels of Russification Predicted According to Exposure to Russians, by Traditional Religion, 1959[a]

(Urban Populations Only)

	Religion				
	Orthodox	Other Christian	Muslim	Buddhist	All
Exceeding predicted	14	0	1	0	15
Below predicted	3	3	22	3	31

[a] Cell entries are the *number* of groups exceeding or falling short of the predicted levels of Russification and are derived from Table 6.

of the Armenians is interesting because, once their low exposure to Russians (in their own republic) is statistically controlled, they appear slightly *more* Russified than might be expected. Nonetheless, their "deviation" is rather small and could perhaps be accounted for by the fact that since the Armenians are so highly mobile geographically, a certain percentage of the "returnees" from the RSFSR retain Russian as a native tongue.[51]

Since the preceding analysis has indicated that religion should account for a significant proportion of the unexplained variance in Russification, and because it does appear to be theoretically important, we have introduced it into the model. We have included only a single dummy variable for "Muslim/non-Muslim" in the equation. Given the relationships in Table 7, we should perhaps be wary of lumping all non-Muslims into a single category. But we are primarily interested in the extent to which Muslims differ from other Soviet ethnic groups. Equation 4 presents the new regression of the combined urban and rural populations.

$$\begin{array}{l} \hat{Y} = 1.41 + 3.66X_1 + .60X_2 + .10X_3 - 5.61X_4 \\ \qquad (.99)\ (1.01) \quad (.84) \quad (.02) \quad (.84), \\ \qquad\qquad\qquad \bar{R}^2 = .430 \quad \text{(Eq. 4)} \end{array}$$

where

$\hat{Y}$ = Estimated percentage of non-Russians linguistically Russified in 1959,

X_1 = Urban-rural, X_2 = Male-female, X_3 = Exposure to Russians,

and

X_4 = Muslim/non-Muslim (1 if Muslim, 0 if non-Muslim).

The new term for "religion" adds about 14 percentage points to the amount of variance accounted for by the additive model (compare with Equation 2).

Our chief interest is in examining the incremental change in Russification associated statistically with the Muslim religion, once "exposure to Russians" is controlled. Here one may replicate the earlier procedure of examining urban and rural populations separately. These results are presented in Equations 5a and b.

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to nationhood; and finally, (d) the prestige associated with being termed a "nation." This greater ability of Union Republic nationalities to preserve their identities has been proposed by Vardys, "Communism and Nationalities," 13, and has been suggested by Armstrong for the Ukrainian case. See John A. Armstrong, *Ukrainian Nationalism*, 2nd ed. (New York: Columbia University Press, 1963), p. 306. Avtorkhanov also distinguishes the Union Republics, where native cadres tend to predominate numerically in leadership positions, from the Autonomous Republics and Oblasts, where non-indigenous personnel predominate. The former are termed "self-governing" republics; the latter, "governed." See Abdurakhman Avtorkhanov, *The Communist Party Apparatus* (Chicago: Henry Regnery, 1966), p. 173. Although the argument here would suggest the importance of examining the impact of "official status" on ethnic identity change, this factor is not treated here because, from a statistical standpoint, the test of the "impact of status" is extremely complex.

[51] The cases of the three Buddhist groups may each have different explanations. The Tuvinians entered the USSR only after World War II and therefore have not had a long time for Russianization to have an impact. The Buriats, on the other hand, do show a relatively high level of Russification (12 per cent of the urban population) and fall very nearly at the predicted level of Russification. Certainly in contrast to the Muslims with equally high levels of Russians exposure, they are rather highly Russified. The Kalmyks are one of the nationalities banished by Stalin for alleged collaboration with the German invaders in World War II. Although I shall not attempt to explain their high resistance to Russification, it should be noted that they are barely Russified despite the fact that of all the groups examined here, as of 1970 the Kalmyks showed the highest level of knowledge of Russian as a *second* language. See "Census Data: Age, Education, Nationality," *The Current Digest of the Soviet Press*, 23, No. 16 (May 18, 1971), 16–17.

Urban

$$\hat{Y} = 4.39 + .79X_1 + .16X_2 - 10.30X_3$$
$$(2.14)\ (1.46)\qquad (.03)\qquad (1.47)$$
$$\bar{R}^2 = .429 \quad \text{(Eq. 5a)}$$

Rural

$$\hat{Y} = .50 + .42X_1 + .06X_2 - 1.47X_3$$
$$(.41)\ (.36)\qquad (.01)\qquad (.37),$$
$$\bar{R}^2 = .413 \quad \text{(Eq. 5b)}$$

where

$\hat{Y}$ = Estimated percentage of non-Russians linguistically Russified in 1959,
X_1 = Male-female,
X_2 = Exposure to Russians,
X_3 = Muslim/non-Muslim.

Once again, adding the new term for religion increases the goodness of fit: the increment to the $\bar{R}^2$ for the urban populations is .310, while for the rural it is .097 (compare with Equations 3a and b). From these new equations one may now estimate the magnitude of the "Muslim effect" on Russification: on the average, urban Muslims are 10.30 percentage points less Russified than urban non-Muslims, while rural Muslims are 1.47 percentage points less Russified than rural non-Muslims—*with exposure to Russians held constant*. Thus Hypothesis 4 appears to be supported: Muslims are less susceptible to Russification than other nationalities in both urban and rural settings, regardless of the level of contact with Russians.

To measure the actual differences in the impact of exposure to Russians on the Russification of Muslims and non-Muslims one must proceed in a slightly different way. In Equations 5a and b a single regression coefficient for "exposure to Russians" was imposed on the urban Muslims and non-Muslims, and another single coefficient on the rural Muslims and non-Muslims. In effect, this assumed that the relationship between exposure to Russians, religion, and Russification is additive (within the rural and urban areas taken separately). But to test further the hypothesis about the differential susceptibility of Muslims and non-Muslims to Russification (within the urban and rural areas taken separately), *separate* regression coefficients may instead be calculated for Muslims and non-Muslims. Without presenting the entire set of estimating equations here, the results of this procedure are summarized in Table 8.

Table 8 clearly shows that in both urban and rural areas, the effect of exposure to Russians on the Russification of Muslims is exceedingly small. An increase of 1 percentage point in the proportion of Russians is concomitant with an increase of only .031 percentage points in the Russification of urban Muslims, and only a minuscule .008 percentage points in the Russification of rural Muslims. This contrasts with the comparable regression coefficients for non-Muslims of .223 in the urban and .079 in the rural populations. These results lend further support to the fourth hypothesis: Muslims are not very susceptible to Russification, either absolutely or by contrast with other Soviet ethnic groups. The impact of mobilization on Russification is much smaller (absolutely) on Muslims than on non-Muslims. And the effect of a given change in the exposure to Russians on urban and rural Muslims is only about one-tenth as strong as for non-Muslims.

I have proposed that the Muslim/non-Muslim differences are the result of a Muslim ethnic ideology that inhibits Russification. Other plausible explanations, however, merit attention. First, Muslim contact with Russians may not have been as *prolonged* as that of non-Muslims. Second, Muslim contact with Russians may not have been as *intensive* as that of non-Muslims.

At first glance, the evidence that Muslims

Table 8. The Impact of Exposure to Russians on the Russification of Nationalities in their Official Areas by Urban-Rural and Religious Differences: 1959[a]

Religion:	Urban			Rural		
	Muslim	Non-Muslim	All	Muslim	Non-Muslim	All
Coefficient						
b	.031	.223	.146	.008	.079	.064
β	.355	.551	.370	.560	.628	.567

[a] The coefficients represent regressions of "percentage of natives linguistically Russified" onto "Russian percentage of the population," with sex controlled. A separate equation was computed for each urban-rural by religion combination. The b's are unstandardized partial regression coefficients. The β's are beta weights (standardized partial regression coefficients).

have not had as enduring a contact with Russians might well seem to account for our statistical results. Most of the Central Asian and North Caucasian Muslim groups came under control of the Russian empire only in the nineteenth century, while most of the non-Muslims were incorporated in the previous few centuries. Yet examination of the cases of the Muslims of the Volga region (Tatars and Bashkirs), who have had almost as enduring a relationship with Russians as have their neighboring non-Muslims, tends to reaffirm our argument. As we may observe in Table 6 (above), the Tatar and Bashkir mobilized populations have levels of Russification of 2.9 and 4.4 per cent respectively, in contrast with figures ranging from 15.5 to 22.7 per cent for neighboring Orthodox groups.

We may also illustrate this point by looking at Kazakh communities that have had long exposure to Russians. Kazakhs residing in Orenburg Province in the RSFSR show a low Russification level despite extensive contact with Russians lasting over two centuries: urbanized Kazakhs in Orenburg Province show a level of Russification of 3.2 per cent; rural Kazakhs, 2.7 per cent. Similarly, Kazakhs residing in the heavily Russianized provinces of the Kazakh SSR, including those provinces long settled by Russians, show very little Russification. For example, in the Virgin Islands Territory 2.2 per cent of the urban and 1.0 per cent of rural Kazakhs were Russified as of 1959. Even in North Kazakhstan Province, where Russians comprised nearly 80 per cent of the urban population (and Kazakhs merely 5 per cent), only 4.1 per cent of the urban Kazakhs were linguistically Russified.[52]

A second plausible alternative explanation of our results has more general importance. In the past, definite "native" and "colonist" quarters existed in the old cities of many non-Russian areas. In rural areas as well, natives and colonists usually had restricted contact with one another. Our measure of "exposure to Russians" probably exaggerates the actual level of contact between natives and Russians even today—especially in rural areas, where most villages and collective farms are likely to be more ethnically homogeneous than the gross percentages for the entire rural area would suggest. This limitation might create two types of distortion in the results. First, *if* residential segregation in both urban and rural areas tends to be greater in Muslim than in non-Muslim areas, the measure of "exposure to Russians" overestimates the actual amount of contact between Muslims and Russians more than it overestimates the level of contact between non-Muslims and Russians. Second, if, as seems likely, residential segregation is more common in rural than in urban areas, the measure of "exposure to Russians" exaggerates the level of contact between rural natives and Russians more than it overstates the level of contact between urban natives and Russians. Hence the higher "impact of exposure" on urbanites and non-Muslims than on ruralites and Muslims may partly reflect the currently available measures rather than the greater susceptibility of urbanites and non-Muslims to Russification.

In brief, the measure of exposure to Russians does not fully correspond to the theoretical argument made here: that the amount of contact with Russians affects Russification. Ideally, a measure of such contact should encompass a variety of dimensions of contact, such as the frequency, duration, and social contexts of contact. Instead, the present measure only approximates the probability of such contacts at a single point in time on the basis of a single indicator.

Only field investigation can generate the type of data required to validate fully our argument that Muslim ethnic ideology is the source of the Muslim/non-Muslim differences in Russification. In other words, we cannot completely discount the alternative explanations of the present results. Survey research now being conducted by Soviet scholars may shed additional light on this question, especially after sufficient comparative results are reported for different nationalities.[53] More detailed aggregate statistics in the forthcoming 1970 census report may also provide further corroboration or qualifications of this argument. Until such new information becomes available, however, the mediating influence of a Muslim ethnic ideology stands as one alternative explanation of the Muslim resistance to Russification. The evidence supports the hypothesis that mobilization and exposure to Russians have a Russificatory impact on Muslims and non-Muslims alike, but that Muslims are much less susceptible to Russification than non-Muslims.

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[52] Even these rough comparisons can only be suggestive, however, since one might still pose the argument that it is the *combination* (interaction) of duration and extent of contact that accounts for the results.

[53] There have been a number of such reports concerning attitudes of Tatars and Russians towards mutual collaboration at the workplace, intermarriage, and inter-ethnic friendships. See, for example, Iu V. Arutiunian, "Opyt sotsial'no-etnicheskogo issledovaniia (po materiialam Tatarskoi ASSR)," *Sovetskaia etnografiia,* 1968, No. 4, pp. 3–13; and O. I. Shkaratan, "Etno-sotsial'naia struktura," pp. 3–16. Brief reports of similar studies among other nationalities are to be found in L. M. Drobizheva, "O sotsial'noi odnorodnosti respublik i razvitii natsional'nykh otnoshenii v SSSR," *Istoriia SSSR,* 1967, No. 1, pp. 67–82; and L. M. Drobizheva, "O sblizhenii urovnei kul'turnogo razvitiia soiuznykh respublik," *Istoriia SSSR,* 1969, No. 3, pp. 61–79. An extraordinary wealth of relevant data is presented also in Kholmogorov, *Internatsional'nye cherty.*

Conclusion

Both urbanization and exposure to Russians have been shown to have a substantial impact on the Russification of Soviet minority nationalities. Exposure to Russians affects urban natives far more than rural natives, revealing that Russification is a function not only of the relative proportions of Russians in the area, but of the weakening of traditional village ties and the exposure to modern technology, mass communications, and an urban life-style.

In assessing the impact of urbanization on Russification, however, one must be aware that the magnitude of the "urbanization effect" measured here may be associated primarily with the particular level of urbanization now achieved by the non-Russian groups; that is, the impact of urbanization may be gradually increasing or decreasing over time. To obtain a longer perspective, let us look briefly at figures comparing 1926 and 1959 levels of Russification.

Table 9 shows that between the two censuses there has been an increase in the average level of Russification of both urban and rural populations.[54] The rural level has increased from .2 per cent Russified in 1926, to .7 per cent in 1959; the urban has increased from 3.0 per cent Russified in 1926, to 5.6 per cent in 1959 (see Table 9.B). The gain of .5 percentage points in the rural areas is not large and might well be due in part to outmigration of Russified urban natives to serve in specialized or administrative jobs in the villages.[55] But two long-

Table 9. Mean Percentage of Non-Russians Linguistically Russified: 1926 and 1959 (residents in their official national areas only)[a]

	Rural		Urban		Total	
	1926	1959	1926	1959	1926	1959
A. Raw Mean Scores: All Groups in the USSR at the *Given* Time.[b]						
Mean	0.2	0.7	2.9	5.8	0.3	1.3
(N)	(39)	(42)	(34)	(42)	(39)	(42)
B. Comparable Mean Scores: All Groups in the USSR at *Both* Times[c]						
Mean	0.2	0.7	3.0	5.6	0.3	1.6
(N)	(37)	(37)	(32)	(32)	(37)	(37)

[a] Cases with extremely high scores at either the 1926 or 1959 census dates are excluded from the table for both dates. These are: Belorussians, Karelians, Altais, Khakasy. If such cases are not excluded, then the differences between 1926 and 1959 mean scores are almost nil.

[b] The N's for the urban and rural categories differ in 1926 because five groups had no recorded urban population in 1926 (Adyghes, Cherkess, Kalmyks, Karachais, and Komi Permiaks). The N's for 1926 and 1959 differ because (1) four groups were incorporated into the USSR during World War II (Estonians, Latvians, Lithuanians, Tuvinians), (2) the Mordvinians did not have an official national area until 1928, and (3) the Crimean Tatars and Volga Germans lost their official territories during World War II, and did not regain them.

[c] In order to establish comparability between the 1926 and 1959 figures, the means in this part of the table are based only on groups meeting the following criteria: (1) they had official territories at *both* census dates, (2) they had recorded population in the relevant category (e.g., urban) at *both* dates, and (3) they have not been excluded as "extreme scores." Note that the 1926 territories of the Ukraine, Belorussia, and Moldavia were considerably smaller than the present-day territories. We cannot take these or other border changes into account here, but should note that the annexations of new territory after World War II brought in persons with little Russification and would therefore tend to depress the 1959 Russification figures somewhat.

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[54] The figures and generalizations presented here are not meant to be definitive. In individual cases, in fact, the increment in Russification since 1926 has been negligible and perhaps even negative. Groups such as the Ukrainians and Belorussians, for example, appear to be less Russified in 1959 than they were in 1926 *within the urban and rural populations taken separately*. The methodology for making longitudinal comparisons of ethnic assimilation is very complex and needs more attention. Particular problems are presented by such factors as the changing borders of the Soviet Union, the disbandment of certain ethnic territories between census periods, changing census definitions of nationality and native language, and changing criteria for classifying distinctive ethnic groups in the census reports. For a brief discussion of the last problem, see n. 59, below. In the calculations presented in Table 9, only that portion of the nationality officially designated with the given national label in the census report has been included in the tabulation. Thus, for example, in calculating figures for the Volga Tatars in 1926, the Mishars and Kriashens are excluded; but in the 1959 totals, Soviet statisticians have combined the Mishars and Kriashens with the Tatars, and Table 9 therefore relies upon the combined totals.

[55] Of course, even if the *percentage* of natives Russified remains constant, the absolute *numbers* may increase enormously because of population growth. However, the current aim is not to examine such numbers but to attempt to understand the processes of Russification by measuring the probability that a *given* non-Russian will be Russified under specified conditions (e.g., residence in an urban or rural setting).

term trends working together favor Russification: not only are natives becoming more sification, but the probability that the urban resident will be Russified also appears to be growing. The evidence of accelerated Russification among the youngest urban age cohorts (see Table 2, above) also supports such a conclusion. Thus, whether one examines the impact of urbanization at a single point in time or over a longer time span, the trend definitely favors the gradual Russification of the non-Russian nationalities.[56]

Nonetheless it should be kept in mind that on the whole non-Russians are still heavily concentrated in the rural areas. Of the nationality group members residing within their official national territories, as of 1959 only the Armenians were over 50 per cent urbanized, with 52.2 per cent residing in cities. Of the remaining non-Russian nationalities examined here, only the Estonians (46.9 per cent) and Latvians (46.7 per cent) were as much as 40 per urbanized, and thereby more available for Rus-cent urbanized. Indeed, of the 46 nationalities studied, only 9 were as much as 30 per cent urbanized in 1959—at a time when 48 per cent of the entire population of the USSR lived in urban areas. Thus, although urbanization does contribute to Russification, the non-Russian nationalities are for the most part barely urbanized and have therefore scarcely felt the impact of Russification.

As a further qualification to the current results, we should note that we need to improve our measures of mobilization and ethnic identity before speaking conclusively of the Russification of non-Russians. The measures of "exposure to Russians" and "mobilization" both rely upon aggregate data that are not sensitive to the variety of social processes actually present. The measure of Russification effectively consists of a single indicator, native language. Fortunately, the 1970 census will reportedly offer systematic data on intermarriage rates. Furthermore, data on bilingualism (in addition to data on native language) will be provided for the first time.

For the present, it must again be noted that even without their traditional national languages, most non-Russians might well retain a separate ethnic consciousness. As primary examples of this phenomenon, the cases of the "linguistically assimilated" Jews and Germans are sometimes mentioned by Soviet scholars. More important, however, other foci of ethnic attachment—aside from native language and the group label—must be explored. For example, one prominent Soviet ethnographer has pointed up the importance of attachment to the native land:

> Finding itself in the course of many centuries on the same territory, "enlivening" this territory, a people begins to consider it "native," and links with it its historical fate.[57]

Such a bond between a historic community and a specific territory should be strong among a great number of Soviet nationalities—not excluding some of the smallest, many of which have been among the staunchest opponents to Russification.[58]

I also wish to emphasize that further investigation of the causes and consequences of ethnic identity change and preservation in the USSR is needed. In searching for causes, it is especially important to recognize that the course of ethnic assimilation in the Soviet Union cannot simply be extrapolated from past or current demographic trends. Ethnic processes depend not only upon certain relatively intractable demographic, cultural, or historical factors, but also upon official policies designed to manipulate these and other factors toward desired ends. It has not been my purpose to examine the goals or results of such conscious policy decisions. Rather I have only suggested that one should not exaggerate the singlemindedness of Soviet leaders toward the liquidation of ethnic differences. They have placed severe restraints on the expression of distinctive ethnic values; and they have acted quickly and sternly against any evidence of nationalism among the non-Russians. But to some extent, the growth of national consciousness and the consolidation of ethnic communities in the USSR has been an intentional product of official efforts to break down narrower tribal or regional loyalties or, in the case of Muslims and Turkic-speaking groups, to discourage broader "movements" based on pan-Islamic or pan-Turkic ties.

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[56] The present measures of the Russification effect of urban residence are still imperfect, however, because the levels of Russification among urbanites of different nationalities are dependent partly upon the rates of recent migration of natives to the cities. Where recent urban immigration rates (of largely unrussified rural natives) have been high, one would expect the level of Russification displayed by urban residents to be low; where recent immigration rates have been low, the level of Russification of the urban residents should be high. Without information on the migration rates of specific nationalities, one cannot attempt to compensate for this factor. The 1970 census results will offer some information on this when they become available.

[57] V. I. Kozlov, *Dinamika chislennosti narodov*, p. 29. The quotation marks are in the original.

[58] See Robert Conquest, *The Nation Killers: The Soviet Deportation of Nationalities* (London: Macmillan, 1970).

Finally, the political *consequences* of ethnic consciousness and ethnic diversity deserve more study. Despite enormous social, economic, and cultural change, distinctive ethnic identities are not about to be eliminated in the Soviet Union. It does seem possible that a few minority groups may be completely assimilated over the span of the next several generations. Among the nationalities investigated in this study, the Karelians and Mordvinians appear most likely to suffer such a fate, since they both register absolute losses of population between the 1959 and 1970 censuses. In fact, a number of relatively small groups enumerated in the 1926 census may have been almost completely assimilated, since they are no longer treated as distinctive in Soviet statistical sources—though as a rule these groups have been assimilated (if at all) not by Russians but by other Soviet nationalities.[59] Moreover, several of the smaller groups inhabiting Siberia and the Far East are probably destined for assimilation in the near future. But for almost all of the groups investigated here, complete assimilation is highly unlikely in the foreseeable future.

How policy makers cope with continuing ethnic diversity therefore deserves careful study. For now it should be noted that the preservation of ethnic consciousness need not signify a lack of loyalty to the Soviet regime; much less, support for nationalist activities. As long as ethnic diversity is not transformed into overt political cleavages or conflict, or as long as such conflict as does occur can be contained or resolved within prescribed decision-making structures, ethnic diversity can perhaps be tolerated or even encouraged. But the maintenance of national identities does provide an enduring potential focus of nationalist activity, especially because of the strong emotional content of ethnic symbols. Soviet leaders must therefore be extremely careful to avoid stimulating ethnic resentments; they must weigh any possible goal of assimilation against the probable costs. Crude attempts to hasten the slow pace of assimilation might lead to disruption, particularly among groups with distinctive cultural traditions or with long histories of opposition to Russian control. At the same time, Soviet leaders must be wary of more general political changes that might also precipitate the crystallization of ethnic cleavages, such as the growth of factionalism within the Party, overt appeals by leaders for popular support within specific non-Russian constituencies, or open access to mass communications media by critics of official policies.

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[59] Some of this assimilation may be more apparent than real. For example, the 1959 census report does not list the Adzhars, Mingrelians, Svanians, and Laz, all of which were listed in the 1926 report. However, even if an individual named one of these titles as his subjective nationality in 1959 he was recorded in the census report as a Georgian. Similarly, self-designated Mishars and Kriashens, listed separately in 1926, were counted as Tatars in 1959. These cases of "assimilation" are probably more apparent than real because they result at least in part from administrative decisions to combine certain groups. In fact, census enumerators were provided in 1959 with a "Glossary of Nationalities and Languages" listing 733 ethnic titles, which were then combined into 126 titles in the census report. Unfortunately, figures on the populations of the 733 self-designated groups have not been revealed. On this matter see A. A. Isupov, *Natsional'nyi sostav naseleniia SSSR (po itogam perepisi 1959 g.)* (Moskva: "Statistika," 1964), pp. 11–13.

The Divisive Primary Revisited: Party Activists in Iowa*

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The implications and effects of the direct primary election long have interested political scientists, politicians, journalists, and other party workers. V. O. Key wrote that "The adoption of the direct primary opened the road for disruptive forces that gradually fractionalized the party organization. . . . [T]he primary system . . . facilitated the construction of factions and cliques attached to ambitions of individual leaders."[1] In widely used textbooks, Hugh Bone, Frank Sorauf, and William Keefe have observed the beliefs that primaries exacerbate rifts that seriously affect a party's health.[2] Andrew Hacker wrote that "Common sense, if nothing else, suggests that the very existence of such a primary produces or symbolizes fissures among a party's supporters. The supposition also arises that those who backed the primary loser in the Spring may be less than enthusiastic about aiding his vanquisher in the Fall."[3]

The late Senator Arthur Watkins of Utah, in a chance conversation with one of the authors, declared that the trouble with the Utah pre-primary convention system was that it split the state parties into factions, created organizations loyal to individual candidates, stimulated emotional schisms that were difficult to heal, and usually sent each party into the general election with only half an organization. Recent writers of popular political lore also have noted the impact of the primary. Theodore White, Jr., for example, wrote that "a genuine primary is a fight within the family of the party—and, like any family fight, is apt to be more bitter and leave more enduring wounds than battle with the November enemy."[4]

All of these statements contain assertions that direct primary elections are inimical to political parties, but little attention has been paid to testing the actual effect of the primary on party workers; no one has empirically determined the reactions and postprimary contributions of the campaign activists whose candidates may have won or lost at the polls.

Nine years ago Andrew Hacker, in *The American Political Science Review*, published the results of his investigation of 220 senatorial and gubernatorial elections to determine whether a primary fight adversely affects a candidate's chances in the general election. He concluded (a) that a divisive primary, in and of itself, bears little relation to a candidate's prospects at the general election; (b) that when only one candidate has a divisive primary, such an experience is a handicap; and (c) that the impact of primaries is affected by incumbency, turnout, the hostility or friendliness of a state to a party, and a wide range of causal factors.[5]

Professor Hacker did not examine the influence of the primary on the campaign personnel. Therefore, our study was undertaken to investigate the effect of a primary on a group of people who worked in the 1970 primary and general election campaigns in the First Congressional District in Iowa. We thus

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[1] V. O. Key, Jr., *Politics, Parties, and Pressure Groups*, fifth ed. (New York: Thomas Y. Crowell Company, 1964), p. 342.

[2] Bone stated that "the primaries are also seen as weakening tickets and fostering on adding to party splits," *American Politics and the Party System*, fourth edition (New York: McGraw-Hill, 1971), p. 273; Sorauf noted "the fear that the primary exacerbates party rifts, splits, factions, feuds . . . the resulting wounds are often deep and slow to heal," *Party Politics in America* (Boston: Little, Brown and Company, 1968), p. 211; and Keefe listed the arguments against the primary including the fact that it "has contributed to numerous intraparty clashes; particularly bitter primary fights sometimes render the party incapable of generating a united campaign in the general election," *Parties, Politics and Public Policy in America* (New York: Holt, Rinehart, and Winston, 1972), p. 4.

[3] Andrew Hacker, "Does a Divisive Primary Harm a Candidate's Election Chances?" *American Political Science Review*, 59 (March, 1965), at 105.

[4] Theodore White, *The Making of the President, 1960* (New York: Atheneum, 1961), p. 78.

[5] Hacker, pp. 105–110.

defined the party in our universe as that basic part of the total party represented by the political leaders and followers who contributed their talents, efforts, and money in an attempt to nominate the candidates for the House of Representatives. This definition, relating the effort of declared partisans to the function of influencing the achievement of power, is consistent with Key's description of party,[6] and is similar to the nucleus of the party defined by Joseph Schlesinger as "the collective effort devoted to the capture of a single public office."[7]

Our data were obtained in an attempt to determine the kinds of persons who work for the candidates, the tasks they perform before the primary and subsequently in the November general election campaign, and the impact of a primary and a general election on the party activities and the party activists. We have not equated our respondents with the total party organization in the general election, and we have made no evaluation of the impact of the primary on party competition. We examined, in one set of elections, the extent to which the assumptions of Key, Hacker, and others—that primaries split parties—were accurate when reflected by the coordinated efforts to get candidates elected to the Congress in Iowa's first district.

The Setting

The congressional primary and general elections in Iowa's first district were chosen for investigation because they were unusually competitive and interesting in both the Republican and Democratic parties in 1970.

The Republican primary race was contested by seven-term incumbent Representative Fred Schwengel of Davenport, and State Senator David Stanley of Muscatine, who two years previously had come within five thousand votes (out of more than a million cast) of upsetting the popular Democratic governor, Harold Hughes, for a seat in the United States Senate. Both candidate's campaigns were well financed and well organized in all counties in the district. Schwengel, 63, ran as an experienced and moderate legislator who, as a member of the Committee on Public Works, had been attentive to his constituents and their needs. Stanley, 42, campaigned vigorously from house to house and in frequent professionally made television announcements. He stressed his own state legislative record and invariably charged that Schwengel was insufficiently regular as a Republican—that his party unity score was lowest among all of Iowa's congressmen.[8]

The Democratic primary featured three candidates: Edward Mezvinsky, a thirty-three-year-old lawyer and State Representative from Iowa City, who had gained prominence in the Iowa General Assembly as a vocal protector of consumer interests; William P. Albrecht, a thirty-four-year-old University of Iowa professor of economics who had actively supported Eugene McCarthy in 1968; and William "Blackie" Strout, a forty-year-old sheriff from Davenport.

Mezvinsky, who ran a campaign partially designed by professional manager Joseph Napolitan to exploit the candidate's distinctive name, claimed he would give the district more vigorous and liberal representation. He had an organization in every county, and his effort was well financed. Albrecht, less well known and operating on a much smaller budget, campaigned against excessive national defense spending, the war in Vietnam, pollution, and the declining economy. His campaign was concentrated in the larger cities of the district although he frequently spoke to farm groups in rural counties. Strout, in spite of disavowals that he was a "law and order" candidate, was identified with crime prevention and eighteen years of experience in law enforcement. He had only a skeleton organization outside of his own county (Scott, the most populous in the district) but he campaigned energetically and reportedly had much appeal among conservative Democrats in all parts of the district.

The First Congressional District of Iowa is in the southeastern part of the State. It contains 447,000 people, about half of whom live in four of the twelve counties. Politically, the district is not quite evenly divided between Republicans and Democrats; Democrats have rarely captured the congressional seat, and Schwengel represented the district from 1953 until 1973, except for the 89th Congress when he lost to political science professor John Schmidhauser by 3,345 votes. Schwengel subsequently recaptured his seat by defeating Schmidhauser in 1966 by a narrow margin; he was re-elected in 1968.

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[6] Key, *Politics, Parties, and Pressure Groups,* p. 314.

[7] See Schlesinger's essay on "Political Party Organization" in *The Handbook of Organizations,* ed. James G. March (Chicago: Rand McNally, 1965), p. 774. *Cf.* M. Margaret Conway and Frank B. Feigert, "Motivation, Incentive Systems, and the Political Party Organization," *American Political Science Review,* 62 (December, 1968), at p. 1172.

[8] Schwengel's 1969 party unity score was 47 per cent on *Congressional Quarterly's* overall scale of selected issues in the 90th Congress. He supported President Nixon 72 per cent of the time. *Congressional Quarterly,* January 16, 1970, pp. 175 and 153.

Methodology

In March of 1970, we asked each candidate to identify four of his most important supporters actively working in his organization in each of the eight rural and semiurban counties, and to name six such persons in each of the four most populated counties.[9] If all of the candidates had been fully organized in all counties, we would have received the names of 280 identified party leaders. Such a response was impossible because two candidates did not have organizations in all counties, and several candidates were reluctant to identify at least some supporters in certain counties; Sheriff Strout was unwilling to contribute more than a token list of members of his organization. Consequently, we acquired a list of 220 persons, of whom 209 responded. In answer to our questionnaire, one hundred eleven stated that they were working for Republicans and ninety-one were supporting Democrats; seven did not identify the candidates for whom they were working.[10] Most of the respondents identified themselves as definite partisans; only three per cent called themselves independents.[11] Our survey included 62 per cent males and 38 per cent females from many walks of life, ranging from a few students helping candidates for the first time to many experienced party workers including a national committee member. They were the active, influential, party middlemen and women similar to those described in many studies of local campaign workers.

To each of these activists, we sent an anonymous questionnaire late in April (the primary was held on June 2) and a second questionnaire two weeks before the general election in November. We received 190 responses to our first mailing and 137 to the second. Nineteen persons responded only to the second questionnaire. There was no systematic bias in the return, either in terms of party or candidates; i.e., Republicans and Democrats, supporters of winners and supporters of losers returned their questionnaires in equal proportions.

The preprimary questionnaire contained questions dealing with social characteristics, party and campaign experience, party and campaign activity, political socialization, and attitudes toward party functions. The November questionnaire requested some identical information plus data on changes in activity, level of participation, and voting intentions.

The data were subjected to the frequency chi-square test for independence. The chi-squares for all two-by-two tables were computed using Fisher's Exact Test. Where an ordinal interpretation could be given to variables in a contingency table, we also included the gamma in our analysis. The data are reported in terms of percentages with comparisons between Republicans and Democrats as well as between those respondents who worked for winners and those who supported losers.

Primary Election Results

In the Republican primary, incumbent Fred Schwengel defeated David Stanley, 24,332 to 18,806 votes—winning ten of the twelve counties in the district. Stanley carried his home county of Muscatine and one small, rural county. Half as many Democrats as Republicans voted in the primary election, in which Democratic State Representative Edward Mezvinsky won his party's nomination, carrying nine of the twelve counties with 8,864 votes. He lost three counties to Strout, who had 4,987 votes. Professor Albrecht received 6,186 votes.[12]

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[9] We gave the method of selecting the respondents careful consideration, and decided that by tapping the outstanding workers identified by the candidates, we would poll a unique group not previously examined at the primary level. A survey of a random sample of workers would have been both impractical and irrational; the total number of consistently active participants in the primary effort was too small and too unevenly distributed. Two of the five candidates had only partially developed organizations, but we wanted to question at least those members of their organizations they thought were most important to their campaigns in each county.

[10] Although all partisans were identified by their candidates as members of their party, the questionnaires were completely anonymous and the statement above merely means that seven respondents chose not to answer the party identification question. This omission is reflected in our computations.

[11] The 202 respondents identified themselves by party as follows:

Strong Republican	51	24.4%
Republican	34	16.3
Independent Republican	28	13.4
Independent	7	3.3
Independent Democrat	22	10.5
Democrat	23	11.0
Strong Democrat	42	20.1
Missing	2	1.0
	209	100.0%

[12] For a discussion of the relation between primary competition and party strength that would have predicted this turnout, see Julius Turner, "Primary Elections as the Alternative to Party Competition in 'Safe' Districts," *Journal of Politics*, 15 (May, 1953), 197–210, and V. O. Key, *American State Politics* (New York, Alfred A. Knopf, 1956), pp. 104–113. The primary vote was impressive. *Cf.* the study of turnout for U.S. Representative by Arthur C. Wolfe cited in Austin Ranney, "Turnout and Representation in Presidential Primary Elections," *American Political Science Review*, 66 (March, 1972) at p. 23.

Characteristics of the Party Activists

A plethora of research on partisan leaders has indicated that party activists are an elite group in the social and political system.[13] Our data confirmed these findings, but because they are only tangential to the problem investigated here, we have decided not to present them in detail in this note.[14] Suffice it to say here that the First District supporters of both parties were affluent, well-educated, and had upper-middle-class occupations. Religious differences among our party leaders followed national party alignments; Republicans were 90 per cent Protestant. Nearly half of the Democrats were Catholic. Democrats, more frequently than Republicans, were from urban counties, and on the whole, were younger than the Republicans. About half of those polled had been active five years or less, but a quarter had been in politics for more than sixteen years.[15] The respondents were joiners, and there were only slight differences between Democrats and Republicans in organizational memberships. Somewhat surprisingly, the activists in both parties identified themselves as moderates rather than conservatives.[16]

Recruitment and Campaign Experience. To obtain information about the workers recruited into the 1970 campaign, we asked them to recall who introduced them to their candidates. The party accounted for only about 16 per cent of the introductions with little difference between the Republicans and Democrats. We also asked the respondents to tell us how they became involved in the campaign. More than 40 per cent indicated that the candidate himself requested their aid. Another 23 per cent were self-starters; i.e., they were motivated to help without being asked. The impression that the relationship between the candidates and their workers was intensely personal emerged clearly from the recruitment questions.

About a third of the respondents work in a campaign every election year; another fifth work in nearly every election, and a quarter had worked in several previous campaigns. Thus, three-quarters of the activists are frequent campaign workers. On the whole, the winners in the primary had significantly more active, and also more experienced, workers in their camps. Four-fifths of 183 who answered this question had worked in previous campaigns[17] but, in 1970, three-quarters of the respondents worked *only* in the congressional

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[13] See William J. Crotty, "The Social Attributes of Party Organizational Activists in a Transitional Political System," *Western Political Quarterly*, 20 (Sept., 1967) 669–681; Samuel Patterson and G. R. Boynton, "Legislative Recruitment in a Civic Culture, *Social Science Quarterly*, 50 (Sept., 1969) 243–263; G. R. Boynton, Ronald Hedlund, and Samuel Patterson, "The Missing Links in Legislative Politics: Attentive Constituents," *The Journal of Politics*, 31 (August, 1969) 700–721; Dwaine Marvick, "The Middlemen of Politics," in *Approaches to the Study of Party Organization*, ed. William J. Crotty (Boston: Allyn and Bacon, Inc., 1968) pp. 341–374.

[14] Students interested in specific characteristics of and detailed statistics describing the Iowa partisans involved in this study will be sent tables upon request.

[15]

Years in Politics	Composite	Dem.	Repub.	Winners	Losers
0–5	46.0	47.3	45.0	35.9	56.6
6–15	25.2	30.8	20.7	26.2	24.2
16+	28.8	21.9	34.3	37.9	19.2
	100.0	100.0	100.0	100.0	100.0
$N=202$		NS		NS	

[16] A composite total of 62 per cent of our activists declared that they were most sympathetic with the liberal or moderate wings of their parties. Only 12.8 per cent indicated that their sympathies were with the conservatives. Twenty-five per cent thought that this decision would depend on the issue. This stands in marked contrast to a poll of Iowa citizens taken during the summer of 1971 in which 54 per cent of the sample considered themselves conservatives in politics, 12 per cent were undecided, and 34 per cent called themselves liberals (*Des Moines Register*, July 4, 1971). The party activists divide with statistical significance between Democrats and Republicans, but in both parties, they were more liberal or moderate than the general electorate. For an interesting contrast with our findings and also between occasional activists in the Republican and Democratic Parties between 1956 and 1964, see David Nexon's "Asymmetry in the Political System: Occasional Activists in the Republican and Democratic Parties 1956–1964," *American Political Science Review*, 65 (September, 1971), 716–730.

What wing of party respondent sympathetic with	Composite	Dem.	Repub.	Winners	Losers
Liberal	33.5	62.4	10.1	37.6	29.1
Moderate	28.5	13.8	40.4	30.1	26.7
Conservative	12.8	3.8	20.2	14.0	11.6
Depends on Issue	25.2	20.0	29.3	18.3	32.6
	100.0	100.0	100.0	100.0	100.0
				NS	
$N=179$		$\chi^2=58.11562$ $p = .01$			

[17]

Worked in a campaign before	Composite	Dem.	Repub.	Winners	Losers
Yes	79.2	84.1	75.2	94.6	63.3
No	20.8	15.9	24.8	5.4	36.7
	100.0	100.0	100.0	100.0	100.0
$N=183$		$\chi^2=25.34872$ $p = .001$			

campaign during the primary period[18]—another fact that emphasized the personal attachment of the workers to certain candidates or to an office rather than to the party's ticket. Data on both recruitment and campaign experience confirm the conclusion reached by Cutright and Rossi that local party organization "often means *candidate* organization."[19]

Official Party Experience. Considering that the respondents in our study were designated by candidates as among the most important contributors of effort to their campaigns, and that most had participated in some previous campaigns, it is notable that only half of the party activists had had much variety of experience within the party. They had held relatively few important party positions; and those who had held offices frequently held more than one. Sixty per cent of the respondents were not party officials in 1970, and half had never held party office in the past. Although there were few differences between Democrats and Republicans, the winners of the primaries, Schwengel and Mezvinsky, were consistently supported by significantly more party officials than were the losers.

Campaign Activities

One of the most important facets of the pre-primary study involved the determination of campaign activities performed by the respondents so that comparisons could be made with the same group during the general election campaign. As we shall note in Table 5 the worker-leaders participated in the conventional wide variety of necessary campaign activities. There was little difference between Republicans and Democrats except that G.O.P. workers collected money more frequently than the Democrats.[20] Between supporters of primary *winners* and *losers,* the most interesting differences noticeable were that winners kept significantly more records,[21] and held more meetings for workers than did supporters of losers.

To summarize thus far: Half of the Democratic respondents and two-thirds of the Republican activists held no party office during the primary period; the candidate organizations were notably nonofficial. Nevertheless, the men and women had had frequent experience as campaign workers. They were an ideologically moderate to liberal elite group who tended to be oriented toward *campaign* or *candidate,* rather than party; only a quarter of the activists were involved at other campaign levels for the party. Although they differed significantly by party in regard to certain variables, Democratic and Republican workers supporting both winning and losing candidates were remarkably similar in many ways.

In June, when these activists were working for and dispersed among two or three primary candidates in their respective parties, it was impossible to know the extent to which they were divided, alienated, or permanently opposed to candidates for whom they were not working, but it was evident, as V. O. Key asserted, that the primary facilitated the construction of cliques attached to the ambitions of individual leaders.

The General Election

Shortly before the November election, we resurveyed the respondents seeking to determine how they reacted to the primary election, how energetic they were in the general election, what campaign activities had been undertaken or abandoned, and whether these activities were related to partisan affiliation and to winning or losing in the June election.

We sent a second questionnaire and another follow-up request for cooperation, realizing that many of the 137 workers who responded to our second questionnaire were exceedingly busy during the intensive campaigns waged by both the Schwengel and Mezvinsky organizations.[22] The style of the primary prevailed throughout the general election campaign. Mezvinsky attacked Schwengel and the Nixon administra-

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[18]

Involved in other campaigns now	Composite	Dem.	Repub.	Winners	Losers
Yes	23.9	24.4	23.5	28.7	18.9
No	76.1	75.6	76.5	71.3	81.1
	100.0	100.0	100.0	100.0	100.0
N=184		NS		NS	

[19] Phillips Cutright and Peter H. Rossi, "Party Organization in Primary Elections," *American Journal of Sociology,* 64 (November, 1958), at p. 269. *Cf.* Key, *Politics, Parties, and Pressure Groups,* p. 314, and Austin Ranney and Willmoore Kendall, *Democracy and the American Party System* (New York: Harcourt, Brace and Company, 1956) p. 771.

[20] For an analysis of money solicitors as influential middlemen, see Alexander Heard, *The Costs of Democracy* (Chapel Hill: University of North Carolina Press, 1960) pp. 259–281.

[21] This is often distinctive; party record keeping in the past has been spectacularly lacking. See Schlesinger, "Political Party Organizations," p. 784.

[22] We even tried personally to retrieve questionnaires, but we discovered that many persons, especially teachers, young professionals, and students, had left the district. Others, by November, simply were "tired of politics" and were unwilling to "fill out another form."

tion—asserting that he (Mezvinsky) would be a more productive, aggressive, outspoken, and vigorous congressman than the incumbent. Schwengel defended his moderation, and emphasized his experience, his achievements for the citizens of the district, and his agreement or disagreement with the administration on specific issues. Both candidates again were well organized in all counties; both had well financed campaigns and substantial television exposure. On three occasions the two candidates faced each other in debate. On Election Day the contest proved to be so close that the final result was undetermined for ten hours after the polls closed. Schwengel won by 765 votes of 120,943 cast.[23]

Findings

In an initial effort to discover whether the primary activists had been affected by the outcome of the June election and to assess the possible costs to organizational strength, all respondents were asked to compare their overall campaign activities in the November election campaign at any level to those in the primary. As Table 1 attests, about a sixth of the workers said they were more active in the autumn than in the spring; a third performed at approximately the same level, but *half* of the workers were less active in the general election period. There was little difference between Democrats and Republicans in this defection, although 15 per cent more Republicans than Democrats became less active. The most significant and expected positive relationship was between winning and continued campaign activity in both parties. In contrast, nearly three-quarters (73.4 per cent) of the workers who supported losing candidates became less active in the fall—confirming the assumption that there is a loss of talent to the overall campaign effort after a divisive primary.[24]

Second, this disenchantment carried over into voting. More than 12 per cent of the identified elite activists in each party intended to bolt their affiliation and vote for the candidate of the opposition party. And another 5 to 7 per cent were undecided or intended to vote for

Table 1. Intended Activity of Respondents in November: By Party and Victorious Faction

Characteristic	Composite	Dem.	Repub.	Winners	Losers
Comparison Between Present (November) and Preprimary Activity					
More Active Now	15.9	20.7	12.1	25.0	6.3
About the Same	34.8	37.9	32.6	48.5	20.3
Less Active	49.3	41.4	55.3	26.5	73.4
	100.0	100.0	100.0	100.0	100.0
			NS	$\chi^2 = 29.58768$	
$N = 132$				$p \leq .001$	
Who Are You Voting For in November					
Mezvinsky-Democrat	42.4	81.0	12.2	44.1	40.6
Schwengel-Republican	51.5	12.1	82.4	55.9	46.9
Undecided	3.8	5.2	2.7	0.0	7.8
Neither	2.3	1.7	2.7	0.0	4.7
$N = 132$	100.0	100.0	100.0	100.0	100.0
Do You Intend to Vote A Straight Ticket in November					
Yes	54.7	55.4	54.2	68.7	39.3
No	45.3	44.6	45.8	31.3	60.7
	100.0	100.0	100.0	100.0	100.0
			NS	$\chi^2 = 9.91985$	
$N = 128$				$p \leq .01$	

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[23] *Congressional Directory,* 92nd Congress (Washington: U.S. Govt. Printing Office, 1971), p. 401. This was 5,393 fewer votes than were cast in the previous off year election of 1966, but 8,968 more than were cast in 1962.

[24] One-quarter of the winners also asserted that they were less active in November, possibly reflecting the fact that many party officials and leaders, who avoid sides in a primary in Iowa, participated in the autumn campaign.

Table 2. A Comparison of Nonbolters and Bolters

Time politically active					
	108 Nonbolters	25 Bolters	χ^2	*P*	Gamma
16–plus years	34.3%	8.0%	13.38366	$\leq .001$	.67776
6–15 years	25.9	12.0			
5 years or less	39.8	80.0			
	100.0	100.0			
Party Identification					
	108 Nonbolters	25 Bolters	χ^2	*P*	Gamma
Strong Identifier	50.9%	24.0%	9.95489	$\leq .01$	.44581
Identifier	25.0	32.0			
Independent Identifier	22.2	32.0			
Independent	1.9	12.0			
	100.0	100.0			
Previous campaign work					
	94 Nonbolters	19 Bolters	χ^2	*P*	Gamma
Yes	88.3%	52.6%	11.46274	$\leq .001$	.74329
No	11.7	47.4			
	100.0	100.0			

neither candidate for Congress in the general election. Fissures evidently *were* produced among primary workers. In addition, only slightly more than half (54.7 per cent) of the respondents planned to vote a straight ticket. Among the supporters of the primary losers, 60 per cent (compared to 31.3 per cent among the winners) intended to split their tickets.

In order to discover the characteristics of the bolters who decided not to vote or to vote for the opposition, we ran correlations with many of our other variables but found that the statistical computations were somewhat misleading because of the small number of respondents. Nevertheless, several were sufficiently interesting to bear reporting.

Five bolters were precinct committeemen and three were precinct chairmen. No district, state, or national party officials were among those who shifted, but one was a county party official and more than half of the bolters were, or had been, delegates to county conventions. In comparison with the nonbolters, the alienated workers (a) came equally from both parties; (b) had been active in party affairs fewer years than nonbolters; (c) were less likely to be strong identifiers, and (d) had less experience in campaigns.[25] Bolters represented

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[25] These figures tend to confirm the belief of Philip Converse that there is a parallel positive relationship between involvement and party fidelity. See Philip Converse, "Concept of a Normal Vote" in Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley, 1966), chapter 2, p. 33. For an examination of cross pressures related to voting defections at the national level, see Richard W.

Table 3. Activities of Those Who Supported Defeated Candidates

Characteristics	Composite	Dem.	Repub.
If Your Candidate Lost in June, Describe Your Activities in November			
No Change	27.9	45.5	17.9
Will Vote For Candidate of My Party But Will Not Work For Him	44.2	36.3	48.7
Will Not Vote For Either Candidate	6.6	0.0	10.3
Work for Other Party	21.3	18.2	23.1
	100.0	100.0	100.0
		$\chi^2=6.71803$ NS	
N=61			

19 per cent of 58 Democrats and 17.6 per cent of 74 Republicans. Sixteen males and nine females did not support their parties. Age, sex, religion, education, occupation, income, and wing of party were not significantly related to bolting.

To those who had supported defeated candidates we sent several additional questions about their activities in the November congressional race in the second questionnaire. A quarter (27.9 per cent) indicated the losing experience would produce no change in their work at the congressional level in the general election campaign; but nearly half (44.3 per cent) stated that although they would vote for their party's candidate, they would not work for the man who had defeated their primary favorite. Six per cent of the total group had no intention of voting for either candidate, and one fifth of the workers (21.3 per cent) had decided to work for the opposition party to help defeat the candidates who had defeated their primary election candidates—indicating a serious divisive effect of the primary election among our respondents.

Some of those who supported losers transferred their labors to other campaigns. Table 4 reveals that many (56.9 per cent) campaigned at a different governmental level in the general election. They campaigned selectively, however,

Table 4. Activities of Those Who Supported Defeated Candidates (By Governmental Level)[a]

Characteristics	Composite	Dem.		Repub.
Shifting Efforts to Governor's Campaign				
Yes	9.2	8.0		10.0
No	90.8	92.0		90.0
	100.0	100.0		100.0
			NS	
$N=65$				
Shifting Effort To State Legislature				
Yes	10.8	12.0		10.0
No	89.2	88.0		90.0
	100.0	100.0		100.0
			NS	
$N=65$				
Shifting Efforts to Local Level				
Yes	20.0	20.0		20.0
No	80.0	80.0		80.0
	100.0	100.0		100.0
			NS	
$N=65$				
Will Campaign at Several Levels				
Yes	21.5	36.0		12.5
No	78.5	64.0		87.5
	100.0	100.0		100.0
			NS	
$N=65$				
Continued In Some Campaign Activity	56.9	66.3		52.5

[a] These figures have been corrected taking into consideration the fact that several respondents checked more than one response.

Boyd, "Presidential Elections; An Explanation of Voting Defection," *American Political Science Review*, 63 (June, 1969) 498–514. An excellent summary of research on *Within-campaign switchers* and the regularity of party voting appears in David Sears's chapter, "Political Behavior," in *The Handbook of Social Psychology*, 2nd ed., Vol. 5, ed. Gardner Lindzey and Elliot Aronson (Reading, Massachusetts: Addison-Wesley Publishing Company, 1969), pp. 354–355. In presidential elections, a rather consistent picture of change emerges: 8 to 11 per cent switches among voters is not abnormal. On p. 344, Sears notes, however, that regularity of party voting is considerably greater for strong party identifiers.

Table 5. Percentage of Workers Participating in Specific Activities

Activity	Primary Election				General Election			
	Workers Who Supported Candidates Winning in the Primary		Workers Who Supported Candidates Losing in the Primary		Workers Who Supported Candidates Winning in the Primary		Workers Who Supported Candidates Losing in the Primary	
Distributed Literature	80.9	Not Significant	85.6	$N=184$	70.6	$\chi^2=12.01147$ ($p\leq .001$)	39.1	$N=132$
Recruited Workers	85.3	NS	82.2	$N=185$	79.4	$\chi^2=22.25533$ ($p\leq .001$)	37.5	$N=132$
Phone Campaigns	56.4	NS	57.8	$N=184$	44.1	$\chi^2=6.37327$ ($p\leq .02$)	21.9	$N=132$
Kept Records	50.5	$\chi^2=6.94322$ ($p\leq .01$)	31.5	$N=184$	27.9	NS	15.6	$N=132$
Attended Rallies	83.2	NS	75.6	$N=185$	75.0	$\chi^2=9.84570$ ($p\leq .01$)	46.9	$N=132$
Organized Workers	64.9	$\chi^2=8.19886$ ($p\leq .01$)	42.7	$N=183$	50.0	$\chi^2=11.41166$ ($p\leq .001$)	20.3	$N=132$
Canvassed	35.1	NS	37.5	$N=182$	25.0	NS	18.8	$N=132$
Arranged to have Candidate Speak	69.5	NS	69.7	$N=184$	57.4	$\chi^2=10.31914$ ($p\leq .01$)	28.1	$N=132$
Made Press Releases	33.0	NS	26.1	$N=182$	26.5	$\chi^2=8.34459$ ($p\leq .01$)	6.3	$N=132$
Gave Money	77.9	NS	64.4	$N=185$	74.4	$\chi^2=9.10306$ ($p\leq .01$)	53.1	$N=132$
Collected Money	59.6	NS	45.5	$N=182$	54.4	$\chi^2=14.87488$ ($p\leq .001$)	20.3	$N=132$

Independence was determined by using the Chi-square and a two by two table for each activity. Only the percentages participating in each activity are reported.

choosing the races they found most interesting. There was no identifiable movement toward a particular candidate or office.

To refine and conclude our analysis concerning discrete party work performed by all respondents in October and November, we again asked the entire group to identify activities performed. Table 5 indicates that in every category of campaign work, fewer of the partisans were active in the general election than in the primary.

Tasks were, of course, performed by other party officials and workers; for example, both candidates recruited many persons (who had not worked in the primary) to phone registered party members to urge them to vote and to support them on Election Day. But there was a notable reduction in campaign activity among the 209 partisan elites with whom the study was begun in April; the decline in participation among supporters of primary winners was not striking, but it was clearly significant among the supporters of losers.

We know, from our familiarity with the Stanley, Albrecht, and Strout campaigns that some workers felt intensely about the outcome and refused to support the victors or participate in the general election campaign.[26] And the data clearly indicate this; they lead us to agree with Andrew Hacker that many of those who backed the primary loser in the spring are less than enthusiastic about aiding his vanquisher in the fall. The attrition in both voting strength and campaign support from this group was substantial.

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[26] We did not ask losing activists if they volunteered to work for the primary winners, but we do know from interviews with Schwengel, Mezvinsky, and their chief advisors that there was no intensive or comprehensive effort made by the winners to recruit people who had been active for the opposition in the primary. Only a few, possibly no more than three to five, Stanley workers volunteered to work in the Schwengel campaign; most of those who con-

To summarize: Our data confirm some divisive effects of the direct primary election. Among those who supported losers, 73 per cent anticipated being less active in the general election, 60 per cent anticipated splitting their tickets in November, and 21 per cent intended to "work for the other party" in some capacity. Similarly, among those who planned to vote for the opposition's candidate or who were either undecided or planned not to vote at all, a consistent profile emerged. These participants, whom we have identified as bolters, were more likely to be political amateurs; 80 per cent had less than five years of political experience, 75 per cent were not strong party identifiers, and nearly half had no prior campaign experience. The possibility exists that this group of workers included some who might not have participated in the primary campaign at all if they had not been personally recruited by the candidates. Therefore, the defections need not be equated with enervation within the total party organization or the entire electorate. From the outset, most of the more experienced party regulars had been in the Schwengel and Mezvinsky organizations. Finally, the conventional turnout and the narrowness of the victory after a vigorously fought campaign indicate that in spite of the organizational loss between the primary and the general elections, the parties seemed to have suffered no serious or permanent damage.

Permanence of the Impact

In an endeavor to try to determine the thoughts of the party activists about the impact of the primary on attitudes toward political participation divorced from the 1970 election, we asked questions concerning possible future activities. Among all of the respondents, 20 per cent intended to be more active, 60 per cent thought their participation would remain the same; and 20 per cent intended to reduce their activity. Democrats, who are in the minority in the District, stated nearly three times as frequently as the Republicans that they intended to be more active in the future. Interestingly, losers and winners showed almost no difference in their determination to be active in future campaigns or to take a more responsible position in the party in the future—indicating that the disenchantment over the outcome of the June, 1970, primary may not affect long range political participation by individual activists.[27]

In an effort to refine this observation, we isolated the most alienated workers—the bolters from their parties in the general election—and, as Table 6 indicates, they intended to be both more active and less active than the nonbolters in the future. In addition, the bolters were about as upward-mobile in the party as the nonbolters; one third indicated a willingness to take a more responsible position in the party in the future.

Again the data appear to indicate that participation in a primary does alienate some supporters of the candidates and, to that extent, reduces their activity in the general election; also, it appears that the primary is particularly divisive in terms of voting and campaign activity among those who supported primary losers. But for an important nucleus of the middlemen, this situation may be only temporary. If 80 per cent of the workers intend to be as active or more active in future campaigns, and a third would take more responsible positions in the party organizations, related or unrelated to their success in the June primary, then, to the extent that our data will allow, we might conclude that in the first congressional district in Iowa in 1970, the primary was divisive among selected partisans, but that this does not necessarily indicate a serious party split or necessarily forecast that all of the party fission will remain. Our testing instrument did not reveal all of the reasons that workers or leaders would increase or decrease participation in the political process in the future. But little that happened in this hard-fought primary indicates that the party system will not be staffed and restored in the future.

Summary and Conclusions

In this study, we sought to determine the impact of two vigorously contested primary elec-

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tinued their political work did shift to other campaigns. On the Democratic side, Mezvinsky has no idea how many Albrecht or Strout workers actively helped him, but he thinks some of them did.

[27] For the losers, this could, of course, mean either that they intend to rejoin the victorious wing of the party in the future or that regardless of their temporary setback, they are eager to oppose the primary winner again. On the other hand, this willingness to be active may confirm Richard Boyd's observation that the impact of a candidate on voting defection is substantial but of short duration. Boyd, "Presidential Elections: An Explanation of Voting Defection," p. 510.

Table 6. Future Plans of Respondents: By Party, Victory, and Bolters

Characteristics	Composite	Dem.	Repub.	Winners	Losers	Bolters	Nonbolters
How Active Will You Be In The Future?							
More	19.1	29.3	11.0	19.3	19.0	24.0	17.8
About the Same	60.3	60.4	60.3	63.4	57.2	48.0	63.6
Less	20.6	10.3	28.7	17.3	23.8	28.0	18.6
	100.0	100.0	100.0	100.0	100.0	100.0	100.0
			NS		NS		NS
N = 131							
Would You Take a More Responsible Party Position?							
Yes	36.2	37.0	35.6	35.9	36.5	33.3	36.5
No	63.8	63.0	64.4	64.1	63.5	66.7	63.5
	100.0	100.0	100.0	100.0	100.0	100.0	100.0
			NS		NS		NS
N = 127							

tions on the party activists in one congressional district. We discovered that the party supporters identified by the candidates were an elite group who focused most of their primary campaign effort on the congressional race. They developed loyalties and commitments to individual leaders that were difficult to shift after their candidates had been eliminated from the race.

For example, 12 per cent of the activists in each party were so embittered by the outcome of the primary election that they bolted their affiliation to vote for the opposition in the general election, and nearly half of the partisans split their tickets. More significantly, nearly three-quarters of the workers who had supported losing candidates became less active in the fall—confirming the frequently made but previously unsupported assumption that the loss of working talent to a party after a primary is substantial. Nearly half (44.3 per cent) of the losers indicated that they would vote but not work for candidates nominated by their parties. Seven per cent decided not to vote for either candidate, and 21.3 per cent decided to work for the opposition party.

Our data thus confirmed the divisive impact of the primary election among some identified activists in the 1970 congressional race, but the parties did not appear to be permanently injured. Many of those who were offended were less experienced and less strongly identified with the party than those who remained loyal. Moreover, additional questions and responses indicated that 80 per cent of the activists—winners and losers, Republicans and Democrats alike—intend to be as active or more active in future campaigns, and that a third of all of the respondents would accept a more responsible position in the party if possible. Thus, most of the workers are not alienated from the party system. They intend to work and rise within it.

The investigation of two elections in one Iowa congressional district in one year represents only an initial exploration of the impact of the primary election on party supporters. No claim to generality is made, and we hope similar studies of contests in other localities will be undertaken. Further examination of other congressional races or gubernatorial or senatorial elections on a statewide basis, in both one-party and two-party states, would test our findings and be most valuable. Finally, an effort to determine the extent to which the various presidential preference primaries require the performance of certain activities and divide supporters of presidential aspirants would provide additional insight into the nominating process.

Virtue, Obligation and Politics

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Recently, several students of moral philosophy have pointed out that even our most self-consciously philosophical understanding of morality and ethics is strongly conditioned by the concepts we use in discussing moral and ethical questions. It has been suggested by Anscombe,[1] Cunningham,[2] Frankena,[3] and Hampshire,[4] that the conclusions we draw concerning the answers to ethical questions depend heavily upon the concepts and categories we use in posing and interpreting those questions. One example of this kind of difference between various conceptions of morality is developed in Frankena's discussion of the distinction between an ethics of virtue and an ethics of obligation:[5] our answer to the basic ethical question, What ought I do? will change as our interpretation of that question changes from What is the virtuous thing to do? to What am I obligated to do? Our subsequent ethical theory, it is suggested, will depend upon the way in which we interpret the "ought" of the basic question, whether in terms of "virtue" (or "way of life") or in terms of "obligation." If this is so, then one important task of contemporary moral philosophy becomes the clarification of significant differences between various characteristic ways of conceptualizing the basic ethical question. This project of clarification can have at least two important results: first, it can help us avoid confusion in the process of comparing various ethical theories; and second, we may be able to develop arguments to suggest that one or another ethical language is best equipped to deal with the broadest possible range of substantive ethical questions.

In this paper I am going to suggest that this particular project of clarification is as important for students of politics and political philosophy as it is for students of ethics and moral philosophy.[6] In particular, I want to direct attention to two basic ways of interpreting or understanding the meaning of politics as an activity: politics conceived as a problem of moral and intellectual virtue, and politics conceived as a problem of obligation and legitimacy. While these two are surely not the only ways of thinking and speaking about politics, it may be fair to say that, leaving theological conceptions aside, the politics of virtue and the politics of obligation and legitimacy are the two alternative political languages presented to us most clearly by the history of political thought.

Politics and Virtue

Now the conjunction of "politics" and the problem of "moral and intellectual virtue" is not an ordinary or familiar one, especially to twentieth-century students of politics and political philosophy; indeed, one concern of this paper will be to explain why this pairing may appear to us to be not merely odd, but absurd. At any rate, I think it will be easily admitted that when we think about what constitutes the political, about what distinguishes the political relationship from other kinds of human relationships, such as love or war or trade or scholarship, we are not likely to regard the distinction between moral and intellectual virtue as being of critical relevance. Instead, I think it is fair to say that for the central tendencies of modern political philosophy, the basic *political* question is not about virtue of any sort, but rather about the reconciliation of the requirements (needs and desires) of the individual and the requirements of society as a whole. In other words, the modern answer to the question, What is politics? is characteristically dependent upon our answer to the question of political obligation, Why should I obey the law? Politics thus becomes that activity that occurs within the sphere constituted by legitimate authority.[7] Although it may be excessively simple, I do not think it grossly distorting to say that for modern political thought the fundamental and defining political distinction is not between intellectual (and nonpolitical) and moral (or political) virtue, but between two forms of social control: power (which is nonpolitical) and authority (which is political).[8] These distinctions are intended only as a description of what I take to be the major tendency in modern political thought. I am not suggesting that there is necessarily any logical incompatibility between politics understood in terms of virtue and politics understood in terms of obligation; rather, the distinction points to a difference in emphasis.[9]

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[1] G. E. M. Anscombe, "Modern Moral Philosophy," reprinted in *The Is-Ought Question,* ed. William D. Hudson (London: Macmillan, 1969), pp. 175–195.

[2] Stanley B. Cunningham, "Does 'Does Moral Philosophy Rest upon a Mistake' Make an Even Greater Mistake?," *Monist,* 54 (January, 1970), 86–99.

[3] William K. Frankena, "Prichard and the Ethics of Virtue," *Monist,* 54 (January, 1970), 1–17. Both Cunningham and Frankena take as their point of departure H. A. Prichard's paradigm-setting essay, "Does Moral Philosophy Rest upon a Mistake?," reprinted in Prichard, *Moral Obligation* (New York: Oxford University Press, 1968), pp. 1–17.

[4] Stuart Hampshire, "A New Philosophy of the Just Society," *New York Review of Books,* February 24, 1972, pp. 34–39. Hampshire's article is an extended review of John Rawls, *A Theory of Justice* (Cambridge: The Belknap Press of Harvard University Press, 1971). Both Hampshire and Marshall Cohen (in his review of Rawls's book in *The New York Times Book Review,* July 16, 1972, p. 1) praise Rawls's study; but both, and especially Hampshire, have reservations about the value of Rawls's political theory, reservations which are based on their feeling that Rawls has not sufficiently explored the possibilities of what I will be calling the language of virtue in political philosophy. See Hampshire, p. 38, and Cohen, p. 18.

[5] Frankena argues that "moral philosophy must fully explore the possibility of a satisfactory ethics of virtue as an alternative or supplement to one of obligation . . . ," ("Prichard and the Ethics of Virtue," p. 17).

[6] The distinction between ethics (or morality) and politics is itself the result of a particular way of understanding both ethics and politics. In my terms, the distinction is much more appropriate and important to politics conceived in terms of obligation (for which ethics tends to become the residual class of all nonobligatory "duties"), than to politics conceived in terms of virtue or ways of life.

Perhaps the best-known brief statement of this aspect of the modern view concerning the foundations of politics is Rousseau's: "Man is born free, and everywhere he is in chains . . . How did this change happen? I do not know. What can make it legitimate? I think I can resolve that question."[10] Rousseau's political philosophy, like most serious political thought from the seventeenth century down to the present time, takes as its theme the question of why free individuals should obey the law of society, if they were not in fact compelled to do so. If no plausible answer can be given to this question, then civil or legal authority as we know it is merely a mask for power, and politics is nothing more than a disguised or sublimated version of war or incarceration.[11] On this view, the problem of political philosophy becomes that of defining the difference between the political relationship and the conditions that obtain on battlefields and in prisons. All of this, on the surface at least, is very remote from the question of moral and intellectual virtue. The preoccupation of that political philosophy which develops around the question of political obligation seems to be with the difference between politics and slavery, rather than the difference between politics and philosophy.[12] It is by no means obvious that anything is intrinsically wrong with this lowering of the horizon of political philosophy. I will try to suggest, however, that the most important types of theory that characteristically result from an obligation conception of politics may be unsound insofar as they are unable to give an adequate account of some important political phenomena that can better be discussed in terms of moral and intellectual virtue.

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[7] Throughout this paper I will refer to "contemporary political philosophy" as if there were one single position or school that could be identified in this way. This is surely an oversimplification, but I think such an identification is plausible, as well as useful for the purposes of my argument. For examples of this position, consider *Concepts in Social and Political Philosophy*, ed. Richard E. Flathman (New York: Macmillan, 1973); *Political Philosophy*, ed. Anthony Quinton (London: Oxford University Press, 1967); and David Raphael, *Problems of Political Philosophy* (New York: Praeger, 1970). There are undeniably some notable nonconforming summary conceptions of the tasks of political philosophy, such as that of George Kateb, *Political Theory* (New York: St. Martin's Press, 1968), p. 3.

[8] A different and more complex formulation of the distinction between power and authority is presented by Hannah Arendt in *On Violence* (New York: Harcourt, Brace & World, Inc., 1970), pp. 44–45.

[9] See Below, p. 90. For a similar characterization, see J. Peter Euben, "Walzer's *Obligations*," *Philosophy and Public Affairs*, 1 (Summer, 1972), 438–459.

[10] *Social Contract*, book I, chapter 1, in Jean-Jacques Rousseau, *Oeuvres Complètes*, Bernard Gagnebin and Marcel Raymond, eds. (Paris: Bibliothèque de la Pléiade, Editions Gallimard, 1959–), III, 351. Cf. *Social Contract (First Version)*, book I, chapter 3, *O.C.* III, 289: "Man is born free, and *nevertheless* he is everywhere in chains" (emphasis added). Speaking of the paradigmatic character of this passage, John Carnes makes the following comment: "This paragraph . . . might be taken as the motto, not only of social contract theory, but of the whole of political theory." "Myths, Bliks, and the Social Contract," *Journal of Value Inquiry*, 4 (Summer, 1970), 105–118, at 114.

[11] Or, less dramatically, of the marketplace. The view that the disappearance of politics is a necessary consequence of the fundamental premises of modern moral and political philosophy is stated in a plausible manner by R. P. Wolff: "If all men have a continuing obligation to achieve the highest degree of autonomy possible, then there would appear to be no state whose subjects have a moral obligation to obey its commands. Hence, the concept of a *de jure* legitimate state would appear to be vacuous, and philosophical anarchism would seem to be the only reasonable political belief for an enlightened man." *In Defense of Anarchism* (New York: Harper and Row, 1970), p. 19. See also Wolff, "On Violence," *Journal of Philosophy*, 66 (October 2, 1969), 601–616. The less than plausible aspect of Wolff's argument is that once he has shown that the political problem can not be solved in terms of the language of obligation and legitimacy, he concludes that the problem is simply insoluble. This conclusion neglects the possibility that the problem might be solvable in some other terms, or (as I shall try to show) that the problem itself is the *result* of certain prior philosophical presuppositions, and hence is only one possible philosophical conception of politics among several, all of which must be considered before we say that political philosophy as such secretes philosophical anarchism. An interesting discussion of the relationship between descriptive conceptions of politics and normative political rules is provided by Charles Taylor, "Neutrality in Political Science," in *Philosophy, Politics and Society*, 3rd series, ed. Peter Laslett and W. G. Runciman (New York: Barnes and Noble, 1967), pp. 25–57.

The discussion of politics by way of the question of moral and intellectual virtue is a procedure followed not by Rousseau (at least, not in the *Social Contract*), but by ancient political philosophy. In the political works of Plato and Aristotle, the question of legitimate authority appears to be subordinate to the question, How ought human beings to live? or, What is the best life for man?[13] Plato and Aristotle both seem to suggest that before it is possible to consider the question of legitimate authority, it is necessary to consider why anyone should choose to enter a political relationship in the first place. We enter into an economic relationship, for example, in signing a contract for the sale or purchase of some product, because by doing so we can expect to obtain something we desire. But what analogous but distinct value can be obtained from choosing to become a citizen? The answer to this question is surely difficult and complex, but at least we might begin by saying that any such answer would depend upon the answer to yet another question, namely, What kinds of things are good for human beings? That is to say, the problem of defining the political as distinct from (for example) the economic has something to do with the problem of distinguishing the public good from the private goods of individuals. The definition of the political involves the movement from the private to the public, and from the private to the public perspective on the question, What is desirable? Now, in order to give a perfectly adequate account of the public good, it is necessary to say something about the good of that most inclusive of all publics, the human species. Of course, in order to understand what the human good is, it is necessary to understand what the human, as such, is. In this way, it seems that there is a direct path from the political question to the human question, since we can fully answer the question of whether or how political life is choiceworthy only on the basis of an understanding of what human beings are—an understanding, that is to say, of human nature.[14]

For example, suppose we were to assert that the central or defining characteristic of human activity is the attempt to maximize (privately defined) pleasure or to minimize (privately defined) pain or both.[15] Human beings could then have nothing in common but the common pursuit of individually determined goals. The political life, then, distinguished by a concern for the common or public good, would be worth following only if it proved to be instrumental or useful in terms of our nonpolitical (for instance, economic) goals. Of course, some people might simply find their private happiness in public life, but given this understanding of human nature, there can be no common or communicable reason for choosing politics for its own sake.[16] On the other hand, if it were presupposed that human life activity is constituted or defined by the possibility of creating and obeying nonpersonal (or nonsubjective) public standards or goals, then it might reasonably be argued that political life is *always* choiceworthy so long as it is *not* only an instrument of private gain, and regardless of what particular public goal stands at the center of the political order.[17]

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[12] I do not mean to suggest that these two concerns are necessarily mutually exclusive, although a concentration on one of these distinctions might well require an abstraction from the other (since each distinction tends to appear insignificant when viewed from the perspective of the other). Consider, for example, Aristotle's abstraction from (or at the very least, obscuring of) intellectual virtue when he presents the grounds for distinguishing between slaves and free men (citizens) in *Politics* 1. 5. 1259b22–1260a34.

[13] See Leo Strauss, *The City and Man* (Chicago: Rand McNally, Inc., 1964), chapters 1 and 2. For Rawls, and for modern political thought in general, this question can not be rationally answered. See *A Theory of Justice*, section 50.

[14] Anscombe's criticism of the ethics of obligation rests in part on the argument that an adequate moral philosophy is impossible without an adequate philosophical psychology; that is, it is impossible to say what a good action is until we are clear about "what a human action is at all." "Modern Moral Philosophy," p. 179. Stuart Hampshire argues that any idea of human goodness depends on some idea of "the distinctive powers of humanity." *Thought and Action* (New York: The Viking Press, 1967), chapter 4. Stephen Clark discusses and defends the Aristotelian argument from "distinctive powers" to moral principle in "The Use of 'Man's Function' in Aristotle," *Ethics,* 82 (July, 1972), 269–283.

[15] This is the basis of John Stuart Mill's proof of the utility principle in *Utilitarianism,* chapter 4. The difficulty here is that in this view of human action, rational interpersonal comparison becomes impossible. Rawls (p. 174) attempts to overcome this difficulty by specifying the existence of certain objective "primary social goods, things that every rational person is presumed to want whatever else he wants." A critical account of Rawls's attempt is given by Adina Schwartz, "Moral Neutrality and Primary Goods," *Ethics,* 83 (July, 1973), 294–307.

[16] This view is suggested by, among others, Hobbes: "The passions that incline men to peace, are fear of death; desire of such things as are necessary to commodious living; and a hope by their industry to obtain them. And reason suggesteth convenient articles of peace, upon which men may be drawn into agreement." *Leviathan,* ed. Michael Oakeshott (Oxford: Blackwell's Political Texts, Basil Blackwell, 1946), chapter 13, p. 84. The classical criticism of

The procedure by which a discussion of the meaning of politics is linked to a discussion of moral and intellectual virtue by way of a consideration of the question of human nature, is followed explicitly by Plato and Aristotle; a brief summary of this procedure may be useful to show one way in which virtue and politics may be thought of as interdependent. Now although Plato's *Republic* and Aristotle's *Politics,* for example, differ in many important respects, both works conceive human activity as being fundamentally threefold. Like other animals, human beings have a capacity for growth and a desire to promote that growth. We eat, drink, mate, and experience the pleasures and pains connected with these and similar movements. This range of experience is said to be private or nonpolitical in the sense that these activities and feelings would always occur, whether there were such things as politics (or publics) or not. This is not to say that survival and growth are politically irrelevant; however, ancient political philosophy as a whole appears to contend that if this were all there were to human life there would be no such thing as politics, strictly speaking. In fact, since these activities are in no way peculiar to the beings we call human, since they are common to many animals, it would be reasonable to say that if these activities were descriptive of the entire range of human activities there would be no such thing as a separate human species, strictly speaking. This is perhaps what Aristotle means by saying that there is no human *virtue* or excellence in being healthy.[18] Human activity connected with the provision of the commodities that can support and secure human life does not, by itself, yield an answer to the question of the best human life. The only virtue belonging to our desire for private possessions—food, wealth, ornament, and so on—lies in the subordination of that desire to some other principle. From the perspective of ancient political philosophy, then, there is no such thing as a good or excellent or virtuous economic man (meaning by this term someone who is entirely devoted to and adept in the art of survival).[19]

Roughly speaking, there are two ways in which this subordination can take place: the political life and the philosophic life. Politics here is understood to be a relationship among individual human beings in which some public value or law takes the place of private desire as the most authoritative guide to action. Politics, from this perspective, is not understood as being constituted by any contract or obligation, but rather by the attempt to replace the human capacity for selfishness by the human capacity for justice (however understood) and self-control as the principal motivating factor in human action. Politics is said to be a choiceworthy way of life because it is the medium within which the development of moral virtue

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this conception of politics as an alliance for the purpose of avoiding death is presented by Aristotle, *Politics,* 3. 5. 1280a25–1281a9. According to Aristotle, such an alliance is a necessary precondition of politics, but is not itself political.

[17] This conception of human nature and politics is drawn from some modern writers who might be called existentialist or historicist, such as F. Nietzsche, *Beyond Good and Evil,* Section 188; Ortega y Gasset, *Revolt of the Masses,* chapter 13 (beginning). Jean-Paul Sartre's contention that man defines himself by his "project" seems to be in line with this development; see *Search For a Method,* trans. Hazel Barnes (New York: Knopf, 1963), pp. 150 ff. A case might be made that such a view also informs Kant's moral philosophy; at least, it seems to be present in the neo-Kantian interpretations of Rousseau, such as those of Ernst Cassirer and Robert Dérathé. For example, see Dérathé's discussion in his *Le Rationalisme de Jean-Jacques Rousseau* (Paris: Presses Universitaires de France, 1948), pp. 182ff.

[18] Aristotle *Politics* 7. 13., *Nicomachean Ethics* 1. 13.

[19] I will be using the opposition economic man/political man throughout this paper. It is intended to express the distinction between a life directed by private or personal desire or inclination, and a life directed by a strong sense of public duty. "Economic," as I use it, then, is not to be equated with "commercial" (since it could also refer to crime, self-defense, art, and hobbies), although commercial activity is one of the most common and important forms of economic activity, in my sense of the word. The idea of economic activity (in this broad sense) as opposed and in some way prior to political activity is discussed by Aristotle *Politics* 3. 5., and by Plato in his description of the immediate predecessor of the genuine *polis* in the *Republic* Book II, 371d4ff. The applicability of the concept of economic man to early modern political thought is suggested by C. B. Macpherson, *The Political Theory of Possessive Individualism* (London: Oxford University Press, 1964), and by Leo Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1953), chapter 5. John Rawls places himself squarely in this tradition of political thought when he argues for the appropriateness of the model of rational (economic) choice for all moral and political situations. For Rawls, political philosophy is understood to be a special case (choice under uncertainty) of the theory of rational choice. *A Theory of Justice,* p. 172.

or virtues (such as justice and self-control) are possible. Man is by nature the political animal not because he ordinarily lives in things called cities or polities, but because it is through politics and the political relationship (as opposed, say, to the economic) that human beings can achieve that excellence of character (moral virtue) which is potential in their nature. To say that an individual is living politically or according to moral virtue is to say something about the principles according to which he *acts,* the goals he tries to attain and the values he tries to maximize in his decisions and his practices. The character of these principles, goals, and values provides the critical difference between the private or economic life and the public or political life. Politics, then, is understood as the pursuit of a certain way or style of life, rather than as obedience to a certain type of authority;[20] what distinguishes politics from other activities are its ends or purposes rather than its manner of institution.[21] The difference between these two conceptions of politics appears in the two questions which they might pose in the process of determining whether a particular association were political (as opposed to merely economic or despotic): Aristotle and Plato would ask, Is it according to nature (Does it enhance the strictly human aspects of human nature)?,[22] while the greater part of modern political philosophy, following Rousseau, would say, Is it legitimate (Is control founded on consent)? For both Aristotle and Rousseau, a theoretical understanding of politics requires a distinction between what is *called* political and what is genuinely political; the two differ with respect to the terms in which this distinction should be drawn.[23]

The problem of distinguishing politics as a separate way of life does end with the exhibition of the line between the political and the subpolitical; for ancient philosophy, the question, How ought we to live? can not be restated as the choice between pursuing politics and pursuing private (economic) goals. There is a third possible alternative for human beings, resting upon a third potentially dominant principle of human nature, and that is the life of the philosopher, the way of life displayed by Socrates. This life is the attempt to actualize to the fullest possible extent the human capacity for rational understanding. If the economic life is dominated by the love of self (or of life), and the political life by a love of the city (and of having a "good name" in the city), the philosophic life is controlled by the love of truth or of being.[24] The excellence which belongs to this way of life is called, by Aristotle, intellectual virtue. According to both Plato and Aristotle, the philosophic life is unquestionably superior to the political life for the same reason that politics is superior to the economic life: it corresponds to a superior aspect of human capacity, to a higher part of human nature.

One way to understand this determination of superiority is to compare the three possible ways of life with respect to their self-sufficiency. Economic man's needs are practically limitless; he is Hobbes's natural man, committed to a perpetual and (finally) perpetually hopeless search for security and well-being. Political man is less concerned with security than with acting well, being just, courageous, and so on. But political activity of this sort requires more than the simple possession of a good character, even where good character is accompanied by good judgment; opportunity and means are as necessary as motive for the commission of political or moral acts, and neither a citizen nor a city has much hope of achieving political excellence if restricted by poverty, weakness, and isolation. Although the dependence on contingency is less, successful politics, like the quest for security, requires favorable external circumstances. But this is not true of the wise man, of the person who can successfully pursue the philosophic way of life and claim intellectual virtue.[25] All that the wise man requires to exercise his wisdom is his mind and the universe. Of course, a wise man is still a human being and not a disembodied spirit; he at least needs food and shelter, just as you and I, and as such he is surely dependent upon circumstance in the way that all human beings are. But the philosopher *as* philosopher, that is, while he is engaged in the activity of understanding which marks him as a man of intellectual virtue, is utterly self-sufficient in a way that can not be matched by the man of moral virtue, the political man, in *his* characteristic activity.

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[20] Rawls excludes the issue of ways of life from the range of rational deliberation on fundamental political questions. Rational public decisions can only be made about the distribution of primary social goods, not about the encouragement of certain life styles or ways of life making use of those goods. Rawls, pp. 142–145. Both Hampshire, p. 38, and Cohen, p. 18, in their reviews of Rawls, regard this as a shortcoming of Rawls's theory of justice.

[21] This is not to say that the ends of a polity and its manner of institution (or integration) may not have real consequences for one another, but that what is most particularly important about *politics* (as a distinct human activity) are its ends or goals.

[22] Plato is surely not as firmly committed as Aristotle to the appropriateness of the natural standard for evaluating politics. This will appear, I think, if we compare Book 1 of the *Politics* with the cave story and the myth of Er in the *Republic.* Plato's doubts, however, seem to center not on the suitability of the natural standard for judging politics, but on whether the differences among polities are significant in the light of the natural standard. In other words, the doubt is not about the standard, but about politics.

[23] For Aristotle, the distinction has to be made in teleological terms. See *Politics* 3. 5. Consider also his discussion of citizenship in Book 3 of the *Politics,* which turns on the difference between those who are *really* citizens and those who are only called citizens. The difference between the merely conventional and the real citizen is stated in terms of the purpose of politics.

[24] The description and comparison of different ways of life in terms of their desiring or erotic dimension is provided by Plato in the *Republic* Book 5, 474c8 to the end of Book 5.

This discussion began with a distinction between two kinds of political inquiry, the one (the more modern and familiar) beginning with the question, Why should I obey the laws? (or, What can make obedience legitimate?), and the other with the question, What is the best life for man? It was suggested that one difficulty with the first approach is that it may not be able to distinguish authority from power, or to give an account or defense of politics as an independently valuable kind of activity. But now it seems that the second variety of political philosophy leads by a different route to a similar difficulty. In the first case, politics threatens to slip beyond the horizon of human aspiration, while in the second it descends beneath human dignity. For if, according to Plato and Aristotle, the philosophic life is the best life for man, the way of life which best answers to the potentiality of human nature, then what becomes of politics, the way of life whose virtue is principally moral (of character or disposition) rather than intellectual (of understanding)? I have suggested that for ancient philosophy politics is understood to claim our admiration because at its best it can turn us from selfishness to moral virtue. But how can one continue to praise moral virtue in the light of the enormous superiority of intellectual virtue? From the perspective of the philosophic life, at least, differences between the best statesman and the worst murderer or tyrant may appear to be insignificant or largely accidental.[26]

Now the thrust of this implicit attack on the political life is tempered to a certain extent because the perspective of philosophy is also presented by the ancients as being incredibly difficult to obtain, so difficult that a general recommendation to live a life of pure intellection would be as absurd as recommending a life consisting of an infinite series of four minute miles. Still, I do not mean to suggest that the assertion of the difficulty (and for most men, the impossibility) of living the philosophic life in any way removes the difficulty about justifying moral virtue or politics. No matter how few individuals have the natural equipment to aspire to a life of intellectual virtue, it remains the best life for human beings as such (and hence, in principle, for all human beings insofar as they are human); this conclusion about the best life has the force of consigning all but a very few to lives which are subhuman in that they can not be justified or defended by word or reason (*logos*) rather than by brute force. The human situation seems to be defined by the not altogether free choice between two alternatives: the philosophic life, which is incredibly difficult but of superhuman sublimity; and the private life, which involves satisfying the most powerful of human passions and desires, and is thus easy, but (nonmetaphorically) brutally inhuman. Politics and moral virtue, if they exist at all, are located between these two variously disquieting human possibilities. Strange as it surely sounds to our ears, the problem of politics, the question of whether or not (and in what way) politics is a valuable and justifiable life style appears, in this analysis, to be identical with the problem of moral virtue. Before turning to some of the consequences of this conception of the problem of politics, let me step back for a moment and try to clarify the basic vocabulary of the political problem understood as the problem of moral virtue.

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[25] In speaking of "philosophers" here I am not referring to the substantial professional group that sometimes goes by that name. I am also obscuring, because of the nature of this introductory context, any possible differences between a philosopher (a lover of truth and wisdom) and a wise man (a possessor of truth).

[26] It can be argued that this is, in fact, the Platonic view. See *Statesman* 257b2–4, and *Republic* Book 10, 619b7–d1. Sometimes Plato does suggest there can be substantial differences in quality among different nonphilosophic ways of life. But these suggestions often appear to rest upon what are for Plato suspect (or nonphilosophic) premises, such as the quantifiability of human happiness (*Republic* Book 9, 587b10–588a10; *Protagoras* 356c4–357b5), or the adequacy of traditional piety (*Crito* 53a9–54d1). This is not to say that these differences are unimportant from some nonphilosophic perspective—such as that of the citizen.

Kinds of Virtue

I have been employing the concepts "moral" and "intellectual virtue" without defining them directly, hoping to indicate some of their sense contextually. Any definition would have to begin by confessing that they are, in fact, fairly literal translations of the Greek expressions *aretē ethikē* and *aretē dianoētikē*. Undoubtedly, some more idiomatic and familiar translation would have been possible, but I have chosen to say "moral virtue" and "intellectual virtue" in spite of the odd and stilted sound of these phrases because I want to emphasize, rather than conceal, the fact that these concepts *are* foreign to our contemporary political vocabulary and political understanding. The value of discussing politics by means of these terms depends in large measure upon their difference from the political language we have become accustomed to using, the language which is informed by political obligation and legitimacy. If, as I shall try to suggest, there is some plausible doubt concerning the value or the usefulness of the language of political obligation, it would be foolish to try to translate all unfamiliar political philosophy into that language, as for instance by setting out to discover Aristotle's or Plato's theory of political obligation, when it would be much more important to know why these writers do not speak of a theory of political obligation, but speak of moral and political virtues instead. So it will be necessary to risk seeming pompous and stuffy, for the sake of exploring the meaning and consequences of this generally forgotten or rejected way of considering politics.

One of the major problems in translating *aretē* by "virtue" is that the Greek word has a much more extensive signification than does the modern English one. *Aretē* does mean virtue or goodness, but it also refers to a quality we would be more likely to call "excellence." For example, a skilled shoemaker, or painter, or athlete might be said to possess virtue in the sense of *aretē;* the same would be true for a fast horse, a strong ox, or a prize pumpkin: generally, a subject is said to be virtuous (possess the quality of *aretē*) when he displays skill in or aptitude for a particular sort of activity. A thing done well thus becomes a thing done virtuously, and the doer is said to be virtuous insofar as that *particular* activity is concerned; this does not mean that a virtuous shoemaker, or craftsman, or merchant is necessarily a virtuous human being. This is a very broad meaning of "virtue," much different from our own which seems to refer mainly to chastity or innocence and perhaps also to a rather more than slightly unbalanced asceticism. From this perspective, it naturally seems very odd to see Plato and Aristotle talking about virtue being the principal concern of politics. The oddness is certainly and genuinely there, but it is possible and necessary to climb out of the confines of our ordinary language at least to the extent of not confusing oddness with unintelligibility. My purpose in thus contrasting the ancient and the contemporary meanings of virtue is certainly not to sneer at any supposed moral decay, but rather to warn against the warm and comfortable feeling that we have somehow or other gone beyond confusing virtue and politics. In a sense, we have "gone beyond" understanding virtue as being politically relevant—good conduct being understood for the most part as being a strictly private matter—but it is not true that this going beyond simply involves the obviously desirable rejection of an absurd and oppressive dogma.

It appears that according to ancient philosophy, the relationship between politics and virtue can be stated as follows: the city (or polity or political community) is that structure (or pattern of relationships) which has as its aim the development of moral virtue among its citizens. Polities or political systems can, in principle, be evaluated on the basis of how successfully they carry out this function, bearing in mind that the success or failure of a polity, like that of moral virtue itself, is dependent upon circumstance as well as upon intention. One can hardly blame a poor city or nation, or one which is under severe and continual military attack, for failing to educate its citizens in those virtues which require leisure and peace for their exercise. Still, the principle of moral virtue provides the basic rule for judging politics, and for deciding whether the political life (either as such or in a given city) is justifiable: the meaning and the possibility of politics stands or falls with the meaning and the possibility of moral virtue. This is not to say that the standard of virtue is an easy or a clear one to apply;[27] a much clearer assessment can be made by applying one of the more typical contemporary rules, like level of economic growth, or level of individual liberty, or even level of legitimacy (perhaps defined as the extent of public approval of the regime, or the consent of the governed). The great difficulty with moral virtue as a rule of political evaluation comes from the difficulty of saying just what moral virtue is, even if we grant the possibility that it is, in fact, some thing at all.

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[27] The most consistent application of a standard of this kind is to be found in Aristotle's *Politics,* perhaps most clearly in Book 7. J. J. Mulhern explains the complexity of the Aristotelian standard in "Pantachou Kata Physin hē Aristē," *Phronesis,* 12 (1972), 260–268. See also Plato *Republic* Book 10, 599c6–d4, Montesquieu, *De l'Esprit des Lois,* vol. I, Book 5, and Rousseau, *Discourse on the Arts and Sciences.* The applicability of such a standard to contemporary politics is considered by Walter Berns, "Pornography vs. Democracy: A Case for Censorship," *Public Interest,* 22 (Winter, 1971), 3–24, and by Wilson Carey McWilliams's response to Berns, in the same issue, pp. 32–38.

But leaving aside for the moment the question of what the content of moral virtue is, we are in a position to say what *kind* of thing it is we are looking for: moral virtue is that quality on account of whose presence we praise actions or characters as being good. Furthermore, it appears to be something different from and inferior to intellectual virtue, a quality which distinguishes good understandings or minds. Since human life can presumably be primarily devoted either to action or to contemplation (or understanding), the moral question of the greatest importance becomes how, and under what conditions, one should choose to commit oneself either to action or to contemplation. As suggested above, when the problem is stated in this way it becomes very difficult to see how anyone could defend, as opposed to simply excusing, the choice of the political life. But perhaps this problem can be avoided by examining more closely some other aspects of the relationship between moral and intellectual virtue, aside from the simple assertion of the superiority of intellectual virtue with respect to the criterion of self-sufficiency.

The complexity of the relationship begins to appear when we notice that to state the problem of moral and intellectual virtue in terms of the necessity to choose between them obscures the fact that action generally involves thought of some sort (like the choice to act or not to act in a certain way) and that even the most abstract contemplation is in some sense relevant to action, at least in the sense that it involves or requires abstention from action. We might say that political or moral men necessarily philosophize to a certain extent, and that philosophers are politicians whether they will it or not. In other words, that thought and action cannot, as a matter of fact, be indifferent to one another; no account of politics can be complete without a consideration of the effect of politics upon contemplation, and any account of the pursuit of wisdom or scientific inquiry would have to be concerned with the political or moral consequences of following the life of intellectual virtue.[28] For example, if we praise intellectual virtue (as ancient philosophy does) as being the best life a human being can choose to follow, we are not only providing an implicit criticism of the political or moral life but actually recommending a course of conduct which will tend to detract from the amount of human energy devoted to politics. Given the inaccessibility of the intellectual or scientific life for most people, it might well be that the praise of contemplation involves the commission of a very great wrong. Whether or how this is so can only be known if we can give an account of the value of moral virtue, if we can justify the political life as being preferable to the life devoted to the private pursuit of privately defined goals, which I have called the economic life. The problem of political philosophy, beginning from the question, What is the best life for man? becomes the problem of intellectual and moral virtue: How, on what grounds, can one justify the pursuit of a way of life which falls far short of the horizon of human potentiality? That this is a genuine *question,* one to which more than one answer is possible, is clear, I think, to anyone who has puzzled over the work of Plato and Aristotle,[29] or, for that matter, of Jean-Jacques Rousseau, who in a curious way appears to employ at different times both the language of virtue and, as noted earlier, the language of obligation.[30]

I am suggesting here that the idea of moral and intellectual virtue is not in itself a *doctrine* or *theory,* but rather a *question* or *perspective* on the basis of which moral or political philosophy can be formulated.[31] One might say that it provides the beginnings of a language for the discussion of political questions. As we have seen, the question of virtue turns out to involve a series of questions, starting with, What is the best human life? From this question arise the issues concerning the merits of different styles or ways of life founded upon the devotion to actions of various kinds, and of various styles of inactivity or contemplation: what is the good of being a craftsman or a statesman or an entrepreneur or a soldier, and what is good about being a philosopher or a scientist or an artist or a mystic. Once these questions have been considered, we are then faced with the question of the relationship between the virtues of contemplation and the political or moral virtues: Are they compatible? Are they simply the reflections of one single human excellence in different contexts? Is any mediation or compromise possible between them, and so on. As I say, these are simply questions, to which there are several conceivable answers, as might be seen from a comparative study of the works of Aristotle and Rousseau. Nevertheless, it seems appropriate to stress the nature of these questions because they are fundamentally different from the questions which inform serious political inquiry at the present time. Given this relative absence of the concept of virtue from the vocabulary of contemporary political philosophy (as compared with the ubiquity of concepts like liberty, authority, obligation, legitimacy and related contract-linked concepts),[32] two questions present themselves: *why* we no longer speak of virtue when we speak of politics, and *whether* there is any reason to be dissatisfied with the present state of affairs. In the remainder of this paper I will suggest that this transformation is by no means accidental or superficial, but is rather linked with particular conceptions of the purpose of politics and of the character of meaningful discourse about politics. I will also try to indicate that this conception of politics may be unsatisfactory by virtue of being too narrow to deal with many important phenomena that appear to be politically relevant. This criticism by no means calls for a rejection of the language of political obligation as in itself misleading or erroneous; rather, I suggest that there is a sufficient doubt concerning the utility of the modern conception of politics to make the serious consideration of an alternative conception a reasonable and even necessary undertaking.

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[28] Aristotle *Nicomachean Ethics* Book 10, 1178b1–10; *Politics* Book 8, 1325a–b; Plato *Republic,* Book 1, 347b5–d9. One of the great questions of the *Republic* is that of the compatibility of the requirements of justice with the requirements of the happiness of the philosopher. As Simon Aronson says, "If Plato does opt for making the city happy, and thus devises ways of persuading the philosopher [to be just], his recognition of the possible need to 'compel' (520a8) indicates his awareness that the tension is a real one." "The Happy Philosopher: A Counter-Example to Plato's Proof," *Journal of the History of Philosophy,* 10 (October, 1972), 383–398, at 396.

[29] Consider, for example, Aristotle's criticism of Plato's analysis of the *polis* in *Politics* 2. 1., and of the Platonic analysis of the good in *Nicomachean Ethics* 1. 6.

[30] Compare the understanding of politics displayed in the *Social Contract* with that of the *Discourse on the Arts and Sciences.* For a discussion of the problem of Rousseau's political language, see Michel Launay, "Jean-Jacques Rousseau, Ecrivain Politique," *L'Information Littéraire,* 22 (September, 1970), 157–163. Rousseau's awareness of the problem is indicated in his "Préface d'une Seconde Lettre à Bordes," *O.C.,* III, p. 105.

[31] Another way of putting this would be to say that virtue is to be taken here as a general *concept* rather than a *particular* conception. For a discussion of this distinction with reference to American constitutional concepts and conceptions, see R. Dworkin, "Nixon's Jurisprudence," *New York Review of Books* (May 4, 1972), pp. 27–35.

Alternatives to Political Virtue

The central question of the language which understands virtue and politics to be nearly inseparable considerations is What is the best human life? Now if it can be shown that this question is absurd and unintelligible, if in principle no reasonable answer to it can be provided, then the question and the substantive political teachings which follow from it are meaningless and fit to be discarded. Plato and Aristotle presupposed that a rational, nonidiosyncratic answer[33] to this question is possible; such a presupposition is necessary if one is going to speak about politics in terms of virtue, although there is no necessity that the implicit assertion must itself always remain a presupposition: one may sooner or later be able to give a reasoned account of *why* the question is answerable, but in the beginning it is necessary to presuppose the meaningfulness of the question as a question. Now one of the defining characteristics of early modern philosophy is that it was, in several ways, engaged in the business of calling this presupposition into question, along the way to rejecting it as absurd. First of all, it was asserted that although we can give an answer to the question of how we ought to live, this answer will be so far removed from how we do in fact live as to be practically or politically irrelevant. According to this view, knowledge of how we ought to live is not in any way a reasonable or a sensible guide to conduct. The most famous expression of this assertion is Machiavelli's, in Chapter 15 of *The Prince*. According to this position, the question of moral virtue may be appropriate in some contexts, but not in the sphere of politics. It is not a very great step from asserting the impracticality of a political philosophy that takes its bearings from the question of virtue to an assertion of the unintelligibility or absurdity of this kind of political understanding—an understanding which claims to provide the true conception of practice or action, which insists upon the interdependence of morality and politics.[34]

But if moral virtue is *not* a political term, then it can apply only to private relationships, that is to say, to relationships that are not regulated by any common authoritative standard. There is then something fundamentally wrong with a political system which claims to have as its primary concern the production of moral virtue in its citizens; morality may be the appropriate concern of churches, of families, of voluntary associations, but not of politics and government. For Hobbes and Locke, the proper concern of the polity, the reason for which the social contract comes into being, is the protection and security of the individuals who, as it were, hold shares in the polity. Political authority, law, and constraint are justified not insofar as they tend to produce political or moral man, but rather insofar as they tend to protect economic man, the individual who is free to pursue whatever he desires. This idea of the purpose of politics, and of the true meaning of "political" or "civil," is expressed with admirable concision by John Locke in *A Letter Concerning Toleration:*

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[32] This characteristic of the modern language of moral and political philosophy is discussed by Anscombe, "Modern Moral Philosophy."

[33] The special quality of the answer in question could be expressed succinctly by the Greek *logō*, which would suggest an answer by means of that reason which is expressed in human speech. The argument that the task of any moral philosophy is to supply "the ultimate grounds for preferring one way of life to another" is made by Stuart Hampshire, "Morality and Pessimism," *New York Review of Books* (January 25, 1973), 26–33, at 27.

[34] This step is very concisely set forth in the first paragraph of Chapter 11 of Hobbes's *Leviathan*.

> The commonwealth seems to me to be a society of men constituted only for the procuring, preserving and advancing their own civil interests.
>
> Civil interests I call life, liberty, health and indolency of body; and the possession of outward things, such as money, lands, houses, furniture and the like.[35]

Politics is not properly concerned with the promotion of a specific and distinct way of life, but only with the protection of privately determined enterprises.

The mention of Locke in this context no doubt tends to emphasize the connection between this view of politics—the view that the proper concern of politics is the service of economic man—and the liberal tradition. This conception of politics is surely linked with liberalism, but it is just as surely not identical with liberalism, being much more inclusive than that particular doctrine. The pervasiveness of this understanding of politics in modern times will appear if we consider that the beneficiary and the justification of the antiliberal and revolutionary politics of Karl Marx is presented not as a political man or a man of moral virtue, but as a free spirit. The purpose and justification of communist society is that it "makes it possible for me to do one thing today and another tomorrow, to hunt in the morning, fish in the afternoon, breed cattle in the evening, criticize after dinner, just as I like. . . ."[36] In spite of the undeniable and important points of opposition in the substantive political teachings of John Locke and Karl Marx, both appear to understand true politics (as opposed to mere tyranny) to be fundamentally concerned with the protection of what I have been calling economic man. Thus what seems to many to be the principal political alternatives of our time are both animated (and thus to a certain extent defined) by a concern for liberty rather than for virtue, and by the understanding that the answer to the question of the best human life can not receive a political (or any sort of public) solution. For both Locke's householder and Marx's interested amateur, the question of how we ought to live must be treated as a matter of taste.

The first modern criticism of the question of the best life for man is, then, that it is politically irrelevant. The second criticism to be considered here, and one which serves as the epistemological or theoretical foundation of the first, is the assertion that the question cannot be answered in a rational way. To inquire about the best life for man presupposes that there is some "best" or most "virtuous" life which is distinct from the lives which any number of particular individuals may choose to lead. In other words, it presupposes the intelligibility of the distinction between what is good *for* human beings as human beings and what is pleasant to (or desired by) individual men and women. But it can be doubted that goodness or virtue exists, or, at least, that these qualities can be perceived by the human mind as having an existence distinct from that of pleasure. It would be impossible to summarize here all the arguments that have been developed to support this doubt, and so I will simply refer to some of the better-known conclusions. The most fundamental of these may well be the assertion that the question of virtue, of the best life, cannot be settled by rational inquiry. Our ideas of virtue are (on this view), like our feelings of pleasure, the consequence of private and particular sensation, rather than of publicly demonstrable reasoning.[37] We may say what virtue or the best life is, but (it is asserted) we cannot defend (by referring to supporting reasons) our answer against any other moral proposal or preference. The attempt to philosophize about the best life appears to rest upon a mistaken notion of our idea of virtue; it has become almost a philosophical commonplace to say that the attempt to discover the truth about how we ought to live is founded on a logical error. Indeed, it is claimed, there is no rational way to distinguish virtue from pleasure, or what is needed from what is wanted, or what is desirable from what is desired.[38] Moral and political philosophy become theoretically incapable of deciding among the claims presented by different life styles and callings; all that philosophy can do is to show that no way of life, whether of hunter, cattle raiser, entrepreneur or critic, has any reasonable claim to preferential treatment or regard over any other way of life.[39]

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[35] Locke, *Second Treatise of Government and A Letter Concerning Toleration*, ed. J. W. Gough (Oxford: Blackwell's Political Texts, Basil Blackwell, 1946), p. 128. See also *Second Treatise*, chapter 11, section 134, p. 67.

[36] Marx and Engels, *German Ideology*, Part I, in *Writings of the Young Marx on Philosophy and Society*, ed. and trans. Lloyd D. Easton and Kurt H. Guddat (Garden City, N.Y.: Anchor Books, Doubleday & Company, Inc., 1967), pp. 424–425. Even in his direct confrontation with the liberal view, *On the Jewish Question* (in Easton and Guddat), Marx criticizes the liberals not in the name of equality or community, but of liberty or emancipation. An interesting commentary on the passage in question is provided by Michael Walzer, "A Day in the Life of a Socialist Citizen," in Walzer, *Obligations* (Cambridge, Mass.: Harvard University Press, 1970), pp. 229ff.

The question of political obligation seems to arise almost naturally from the situation created by the demise of the question of how we ought to live; it is the logical candidate to fill the vacuum in political philosophy left by the rejection on epistemological and metaphysical grounds of the question of virtue.[40] If no way of life can authoritatively and finally claim to be superior to any other, and if each individual is thus in principle free to choose or create his own standards or rules of conduct, what are we to say about the ordinary human situation characterized by a submission to authority and an obedience to laws we never made? To ask this question is to state the modern paradox of liberty and authority posed in classic form by Rousseau in the *Social Contract.* What is the ground, the justification, of the obligation or duty to obey the law? When is obedience the result of obligation rather than of oppression and coercion? Perhaps the most obvious solution is to say that freedom itself is the ground of obligation: obedience to law alone makes possible that security which is the necessary condition of freedom.[41] In this manner, politics would appear to be justifiable or legitimate (and "authority" thus different from "power") insofar as politics exists for the sake of economic man. In other words, we ought to obey the law because it is in the interest of our freedom to do so. Politics thus conceived appears as a second-rate and inconvenient activity, yet one which is necessary to protect us in our real (i.e., economic or private) existence. Public obedience is the necessary, though unpleasant, price of private freedom.

But the argument which thus employs a reference to liberty as the ground of political obligation creates certain difficulties. According to this argument, we are bound to politics by an obligation which is only prudential (valid only so long as it is in our interest) rather than strictly moral (always and necessarily valid as a matter of duty).[42] If it is not in my interest (as economic man) to obey the rules

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[37] Perhaps the most influential and painstaking development of this position is David Hume's, in *A Treatise of Human Nature,* Bk. III, Part I, sections 1–2.

[38] As in J. S. Mill's famously ambiguous claim that "the sole evidence it is possible to produce that anything is desirable, is that people do actually desire it." *Utilitarianism,* chapter 4, in *The Philosophy of John Stuart Mill,* ed. Marshall Cohen (New York: The Modern Library, Random House, 1961), p. 363.

[39] Rawls attempts to demonstrate the rationality of a rule of justice requiring preferential treatment for the least favored members of society. As Rawls indicates, however, the rationality of this rule depends upon the rationality of something like what game theorists call a maximum strategy in matters of fundamental political choice. That this strategy is the rationally appropriate one in this circumstance is open to question, as in the reviews by Cohen (p. 18) and by Hampshire (p. 39) and by Kenneth J. Arrow, "Some Ordinalist-Utilitarian Notes on Rawls's Theory of Justice," *Journal of Philosophy,* 70 (May 10, 1973), 245–263.

[40] This is not at all to say that the prominence of the question of obligation was historically *caused* solely or even primarily by events in epistemology or metaphysics. At least part of the reason for the pre-eminence of obligation can plausibly be ascribed to a change in the form of the prevailing patterns of social interaction, roughly described by the transition from face to face communities to the distinction between state and society. See Euben, "Walzer's *Obligations,*" pp. 439–440. Similarly, a strong case can be made for assigning the decisive part in this transition to Christianity, as suggested by Hegel, *The Philosophy of History,* trans. J. Sibree (New York: Dover, 1956), introduction, p. 18 and part IV, p. 342, and by Bergson, *The Two Sources of Morality and Religion,* trans. R. Ashley Audra and Cloudesley Brereton (Garden City, N.Y.: Doubleday, 1935), chapter 4. My point is rather that the philosophic significance of the question of obligation (and thus its theoretical, if not its historical, justification) can be grasped by a consideration of the theoretical grounds for discarding the question of virtue.

[41] Such a view is almost formulaic in early modern political thought. Among others, see Montesquieu, *De l'Esprit des Lois,* Vol. I, Book 12, Chapters 1–2 (Paris: Garnier Frères, 1961), pp. 196–197, and Hobbes, *Leviathan,* chapter 21.

[42] This distinction rests on Kant's distinction between a kind of hypothetical (nonmoral) and a categorical (moral) imperative. See *Foundations of the Metaphysics of Morals,* trans. Lewis W. Beck (Indianapolis: Library of Liberal Arts, Bobbs-Merrill, 1959), section II, p. 33. For the use of this distinction in the context of the question of moral obligation, see Alan Gewirth, "Must One Play the Moral Language Game," *American Philosophical Quarterly,* 7 (April, 1970), 107–118. See also H. A. Prichard, *Moral Obligation,* pp. 90–91.

(as political man), then these rules are no longer legitimately binding or obligatory, so far as I am concerned. There can be no legitimate authority which runs counter to individual interest, since political (obligated) man is only a specialized role or aspect of economic (free) man. Law and government are either an exercise of power which is in my interest or one which is counter to my interest, but there is no reason to conclude that "authority" is anything more than a name we give to the useful or beneficial (to us) exercise of power (by others).[43] What we call "political obligation" turns out to be nothing more than a rather unimportant aspect of the general maximizing strategy pursued by economic man.[44] Now this conclusion may be absolutely true; I am not here suggesting that it rests upon false premises or bad argument. But leaving aside the question of its truth or error for the moment, it should be noted that the attempt to resolve the problem of political obligation by reference to the principle of liberty ends by calling into question the meaning of political activity as anything more than a special case of economic activity. This particular approach to the problem of political obligation, which we might without too much distortion identify as the liberal or utilitarian approach,[45] seems in the end to be unable to do what it set out to do, namely, to distinguish the political from the nonpolitical without recourse to the no longer accepted language of moral virtue and the question of the best life.[46] If it were not for the powerful influences of custom and coercion, the public order would lose its privileged status, and the private order would rise up to claim what is, after all, legitimately its own.

Perhaps the most interesting contemporary alternative to the appeal to liberty as a ground of obligation is the appeal to "community." This view, perhaps most forcefully presented by Hannah Arendt,[47] is in an important way a direct response to the difficulties that I have claimed are endemic in the liberal position.[48] I will ignore here the question of the foundations of the "community" position (except to say that it also, like the liberal view, rejects the orientation provided by the question of the best life), and merely present what I take to be its most important conclusions or assertions. Principally, it asserts that genuine political activity can have absolutely nothing to do with the needs of what I have referred to as economic man; a truly political relationship (such as the relationships of obligation and authority) can have no connection with private self-interest of any sort.[49] Politics itself must be considered a valuable activity, and the obligation to obey the law is simply a consequence of membership in the political community. As soon as we demand that politics be good for something other than itself (except insofar as it may be understood to satisfy the irreducible human need for communal ties) it ceases to be politics. Needless to say, political relationships of this sort are extremely rare in those activities we ordinarily call political, at least at the present time. Politics in this sense, or political community as the true ground of political obligation, appears as something to be achieved or recovered.

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[43] This confusion of authority and power would still exist even if we were to assume the rather unlikely condition that obedience to law will always be in our (or everyone's) interest.

[44] "Unimportant" in that it deals only with minimal and instrumental necessities, the items Rawls identifies as "primary social goods," and does not concern the more important question of what to do with the goods.

[45] Although this position is characteristic of a certain kind of modern political thought, it was by no means unknown to Plato and Aristotle. Consider Glaucon's speech about the value of justice in *Republic* Book II; the speech of Callicles in the *Gorgias* 483b4–484c4; Aristotle's criticism of the sophist Lycophron in *Politics* Book 3, 1280b11–13; and especially the statement by Antiphon the Sophist, *On Truth,* Fragment B44 in *Die Fragmente der Vorsokratiker,* 7th ed., ed. H. Diels and W. Kranz (Berlin: Weidmann, 1951–54), vol. 2, 346–355. Something of a confrontation between this view and the position of ancient political philosophy is presented by Xenophon in his descriptions of three conversations between Socrates and Antiphon, *Memorabilia* Book I, chapter 6. The basic disagreement appears to be over the issue of human needs. On this meeting, see Leo Strauss, *Xenophon's Socrates* (Ithaca: Cornell University Press, 1972), pp. 28–31.

[46] One of the most systematic attempts to accommodate the question of the best life to the question of liberty is made by Mill in *Utilitarianism,* especially in the context of the distinction between higher and lower pleasures drawn in chapter 2 of that essay. Whether Mill was successful is extremely controversial. A neo-Kantian refutation of Mill is given by R. P. Wolff, *Poverty of Liberalism* (Boston: Beacon Press, 1968), chapter 1. That Mill was unsuccessful in his attempt to reconcile liberty and duty is also argued by Hilail Gildin, "Mill's *On Liberty,*" in Joseph Cropsey, ed., *Ancients and Moderns* (New York: Basic Books, 1964), pp. 288–303. An argument for Mill's consistency is given by Rex Martin, "A Defence of Mill's Qualitative Hedonism," *Philosophy,* 47 (April, 1972), 140–151.

[47] See especially Hannah Arendt "What is Freedom?" in *Between Past and Future* (Cleveland: World Publishing Company, 1963), pp. 143–171, and *The Human Condition* (Chicago: The University of Chicago Press, 1958), chapters 2 and 5. Arendt's argument that the source of this view can be traced to Greek political practice seems to me highly questionable; but this problem has no bearing on the significance of the conception of politics involved. See also Kirk Thompson, "Constitutional Theory and Political Action," *Journal of Politics,* 31 (August, 1969), 655–681.

[48] Wolff's argument in *The Poverty of Liberalism* proceeds from a rejection of the liberal position as logically inconsistent, to an attempt to demonstrate the existence (in principle) of a political community which can serve as the source of authority and obligation.

[49] Just as, for Kant, a truly good action can have no connection with self-interest. *Foundations of the Metaphysics of Morals,* Preface, p. 6; Section I, p. 13. On this point, Rawls is much closer to Mill than to Kant, in his argument that political principles must be in the interest of each individual for social control to be just or legitimate. See *A Theory of Justice,* Section 29.

At first sight, these two relatively modern understandings of politics, revolving about the concepts of "liberty" and "community" respectively, appear to be diametrically opposed to one another. In fact, it may not be a great exaggeration to say that they constitute the poles of whatever contemporary debate there is about the nature of politics and the character of political relationships. But I want to suggest that these two positions have a great deal more in common than is ordinarily supposed, and that they do not exhaust (as we are too apt to suppose they do) the possibilities for understanding and evaluating political activity. One indication of the similarity of these two is that they both identify politics as the necessary condition of human freedom. According to Arendt, the political community (like the moral community for Kant) is *the* sphere of freedom;[50] according to liberals, politics is indeed the realm of constraint, but of a kind of constraint that is necessary to protect and enhance the realm of true freedom. To be sure, "freedom" is understood quite differently in the two different cases (the former generally implying a variety of self-determination, while the latter generally refers to a straightforward absence of external restraint), but the debate between these two positions seems not to be about the respective merits of individuality versus communality as ways of life, as much as it is about the true meaning of being free.[51] The reverse side of this concern with freedom is an almost complete avoidance of any serious consideration of virtue, or of any of the various questions which I indicated might come to light from an investigation of the problem of moral and intellectual virtue. Just as Plato and Aristotle can be understood to be quarreling over the question, Under what conditions can men become truly virtuous? modern political philosophy seems to be engaged in a debate over the question, Under what conditions can men become truly free?[52]

Conclusion

Is there any reason for dissatisfaction with this transformation of the basic political question? Surely, it can be argued that the modern position represents an enormous improvement by being much more in accord with the genuine limitations of human knowledge. If such modesty is in fact an intellectual virtue, then whatever the merits of the ancient position, it might be based on the undoubtedly immodest presupposition that one can give an intelligible answer to the question of the best human life. Claims and criticisms of this kind raise a question of the greatest importance, but one that will not be discussed here. That question, as difficult as it is important, concerns the determination of the reasonableness of a presupposition. Here, I would like only to suggest why I think it is worthwhile to go to the trouble of trying to revive a buried presupposition. To do this I will try to show why the two modern formulations of the political question that have been considered here are, taken together, unsatisfactory. I am not here concerned to provide a conclusive showing of the wrongness of these formulations, but only to show why it seems advisable to think seriously about alternatives.

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[50] "Freedom as a demonstrable fact and politics coincide and are related to each other like two sides of the same matter" ("What Is Freedom?," p. 149). This conception of freedom and politics as coterminous and inseparable is similar to Marx's assertion that unalienated (or free) human activity is species (or political) activity. See "Alienated Labor," in Easton and Guddat, p. 294. Another directly related formulation is implicit in Rousseau's position that since freedom and citizenship are inseparable, and since citizenship is not always pleasant or in one's interest (and hence not always or necessarily immediately chosen for its own sake), some men will have to be "forced to be free" (*Social Contract,* bk. I, chap. 7, *O.C.*, Vol. III, 364). For a comparison (from a liberal viewpoint) of something like the two concepts of liberty I have been considering here, see Isaiah Berlin, "Two Concepts of Liberty," in his *Four Essays on Liberty* (London: Oxford University Press, 1969), pp. 118–172.

[51] An excellent illustration of the organization of the debate in terms of a conflict over the true meaning of freedom is provided by Marx in his attack on liberalism in the name of genuine liberation in the essay *On the Jewish Question.* In criticizing liberalism for achieving "political emancipation" only, Marx is criticizing the liberal insistence on the separation of politics and society, in which society stands for the realm which is emancipated *from* political control. But, according to Marx, the real liberation of man as species-being is the emancipation of a creature who has evolved beyond the stage of "man as an isolated monad" (or free economic man) and has "taken back into himself the abstract citizen [of liberalism] and in his everyday life, his individual work, and his individual relationship has become a *species-being,* . . . only then is human emancipation complete" (emphasis in text). *On the Jewish Question,* in the Easton and Guddat edition, pp. 235–241. A similar distinction is drawn by Hegel in his argument for political (or universal) freedom in preference to individual (or particular) freedom in *Philosophy of History,* Introduction, p. 38, and in *Philosophy of Right,* trans. T. M. Knox (London: Oxford University Press, 1967), sections 182–187.

[52] A good discussion of this in the context of constitutional issues is that by Walter Berns, *Freedom, Virtue and the First Amendment* (Baton Rouge: Louisiana State University Press, 1957), especially chapter 10.

When I say that the modern formulations neglect virtue, I am using that word to refer to *any* possible answer to the question of the best way of life, and not to some particular answer: when we say how men ought to live, we say what human virtue is.[53] Now, in a sense, the modern political formulations *do* have something to say about how we ought to live (although this question, for them, arises only incidentally): we ought to be free,[54] and politics is either the most reasonable means to that end or itself the process within which the end is realized. In a very broad sense, we are even presented with a choice between two ways of life each of which has certain claims to be considered the most worthy of praise or virtuous: the life of individual liberty (of economic man liberated from unnecessary political control) versus the life of the autonomous citizen in the free community (political man liberated from the impurities of economic life). But just what sort of a choice do these alternatives offer us? Both appear to involve what amounts to a one-dimensional understanding of politics, in which politics are classified and evaluated according to the degree to which either liberty or community is said to be present.[55] I do not mean to deny that this dimension is an important one; but it would be difficult to show that it is in fact *the* primary political dimension of variance, from which all other aspects of political life are derived.

Let us try to see what might happen if we were to take seriously the idea that the question of the presence or absence of freedom (defined in either of the two ways considered to this point) is the key or essential political question. Consider the following situation: let us assume a fairly constant level of either individual liberty or community, and then ask whether at this level we will find other differences which appear to call for other, unrelated, distinctions, or whether knowledge of the level of liberty or community tells us, in principle, all we need to know about the polities in question. If we were to choose several relatively strong communities—say, the early Catholic Church, Sparta, the People's Republic of China and the Mafia—and several relatively liberal polities—say, the United States of America, Athens, Great Britain and Sodom—I believe that we would be confronted by a problem in political understanding which could not be resolved by the language of liberty or community alone. In this example, we should want to be able to say something about the values or the goals which are characteristic of each of these polities, in addition to considering the matter of liberty and community. These examples would appear to suggest the possibility that differences in the uses of liberty, and in the purposes for the sake of which communities may be organized, may be decisive for the character of the polity in question. If this is so, then a political philosophy which is incapable of explaining and evaluating these differences may turn out to be of very little use in the face of the most important and the most difficult political questions.[56] The purity of community appears to match, in narrowness and blindness to a wide range of politically relevant things, the well-known poverty of liberalism.[57]

That political philosophy which takes its beginnings from the question of political obligation appears to end by abstracting from the variety of purposes or goals which may be said to belong to different polities. This abstraction is not accidental or unprepared, but follows from the kinds of questions that are regarded as the proper subjects of modern political philosophy, questions about liberty, obligation, legitimacy, and so on. These questions are all focused, in various ways, on the manner in which the polity is constituted, rather than on the goals or values of life styles which the polity explicitly encourages or implicitly rewards. These latter considerations appear to lie outside the perspective provided by the two principal varieties of modern political philosophy discussed in this paper (although they form the major theme of ancient political philosophy). What we might describe as the shift from the virtue paradigm to the legitimacy paradigm appears to have been accompanied by a severe narrowing of the range of questions which inform philosophic inquiry into the political things.[58] This narrowing, as I have tried to suggest, *may* be distorting with respect to our grasp of political reality, in confining our attention to an insufficient, and perhaps even occasionally unimportant, range of political phenomena. Now I want to be very clear in indicating that I have in no way "refuted" the legitimacy (or obligation) paradigm; I have made no effort in this discussion to deal with the epistemological, logical and moral issues which are involved in the question of the justifiability of the paradigm change.[59] What I have tried to do is to suggest that there are serious objections which can be made to the necessary products or consequences of the legitimacy paradigm, and that these objections provide sufficient warrant to examine the possibilities of another approach to the problem of understanding and evaluating political relationships. That alternative approach is one which formulates the problem of the best human life in terms of the problem of intellectual and moral virtue.[60]

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[53] Again, at this point in the argument "virtue" is intended as a concept rather than a conception, in terms of the distinction referred to in note 31.

[54] Freedom here is understood very broadly, and in this sense can include an idea of security or secure preservation. An example of this usage can be found in the passage in Montesquieu referred to in note 41 above: "Political liberty consists in security, or at least in the opinion that one has of one's security."

[55] This is the source of the distinction between the open and the closed society. The not so remote vulgarizations of these paired oppositions are the popular divisions of contemporary politics into Free World vs. Slave World, and Third (communitarian nationalist) World vs. Imperialist (capitalist) World.

[56] Of course, these may be classified as "cultural" differences, and since (given cultural relativism) they are therefore incommensurable (at least morally), they are not fit subjects for a generalizing and evaluative political philosophy. I am not concerned here with the possible truth of this claim (it would be necessary to examine the plausibility of the asserted moral incommensurability of cultural phenomena); but note that this position implies a political philosophy which, at least in its explicitly evaluative procedures, must ignore the political consequences of "culture."

[57] This point is brilliantly, though perhaps too briefly, made by Benjamin DeMott in his essay, "Pure Politics," which reviews the work of Arendt and others. DeMott, *You Don't Say: Studies of Modern American Inhibitions* (New York: Harcourt, Brace and World, Inc., 1966), pp. 169–182.

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[58] I am using "paradigm" here only for clarification, and with almost the same meaning that I wish to convey by the word "language." Paradigm refers to the heart or grammar of the language, the rules for the proper ordering of concepts and vocabulary, the element that gives the language its particular character and structure. This usage is like the one established by Thomas S. Kuhn in *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), chap. 5; however, I do not share, and the use of "paradigm" here should not be taken to imply, Kuhn's relativist assertion of the incommensurability of competing paradigms. The case for considering the history of political ideas in terms of paradigms is presented by Bhiku Parekh and R. N. Berki, "The History of Political Ideas: A Critique of Q. Skinner's Methodology," *Journal of the History of Ideas,* 34 (April, 1973), 163–184, and by W. H. Greenleaf, "Hume, Burke and the General Will," *Political Studies,* 20 (1972), 131–140, especially 139–140.

[59] Leo Strauss's controversial discussions of these issues are of continuing importance. Relevant here are the Introduction to *The City and Man,* Strauss's "Epilogue" to *Essays on the Scientific Study of Politics,* ed. Herbert J. Storing (New York: Holt, Rinehart & Winston, Inc., 1962), and his essays "What Is Political Philosophy" and "On Classical Political Philosophy," reprinted in Strauss, *What Is Political Philosophy?* (Glencoe: Free Press, 1959). The strongest and broadest defense of the modern understanding of political philosophy is still Karl Popper, *The Open Society and Its Enemies* (London: Routledge, 1945), Vol. I. An interesting discussion and criticism of some of the central features of the modern paradigm is provided by Bruce Aune, "The Paradox of Empiricism," *Metaphilosophy,* 1 (April, 1970), 128–138. An interesting consideration of the theoretical alternatives underlying the alternative conceptions of morals and politics is presented by Kenneth Dorter, "First Philosophy: Metaphysics or Epistemology?," *Dialogue* 11 (March, 1972), 1–22.

[60] The importance and interest of Rousseau for a study of the strengths and limits of the two conceptions of politics discussed here can hardly be overemphasized. Rousseau's treatment of politics in the *Social Contract* and elsewhere presents one of the best known uses of the legitimacy paradigm, "community" variation. And yet Rousseau also insists, in a way that other legitimacy theorists (like Hobbes and Locke) do not, on the intimate connection of (a kind of) virtue and politics. Moreover, no reader of Rousseau can avoid being impressed by the depth, complexity, and even by the uncertainty, of his concern with the question of the best life. Unlike almost any other modern writer, Rousseau was led by this concern to consider not only the question of the best political life, but also that of the best alternatives to politics or citizenship, thereby compelling his readers to engage in the process of comparing political virtue with nonpolitical virtue or virtues. In the terms of this analysis, Rousseau holds a unique position as an uncommonly brilliant (though not necessarily successful) link between the language of legitimacy and obligation on the one hand and the language of virtue on the other.

Generational Change in American Electoral Behavior*

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The persistent relationship of social class to partisan choice is one of the most extensively documented facts of American political life.[1] Despite unprecedented prosperity since World War II,[2] the tendency of the working class to support the Democratic party still persists. But the economic, social, and political changes of postwar America have eroded the relationship between social class and partisan choice.

There are numerous reasons why economic prosperity tends to reduce class divisions. In a prosperous society the differential material rewards that the working and middle classes receive are increasingly narrowed. Differences in life style, which in the past were linked with visible signs of status, are reduced. In an advanced industrial economy the middle class (which since the time of Aristotle has been associated with temperate politics) tends to expand. Class conflict becomes increasingly institutionalized. Working class demands are channeled through a legitimate (and increasingly conservative) trade union structure. Left parties abandon or de-emphasize direct appeals to the working class in an attempt to gain middle-class support. All these factors tend to reduce the relationship of social class to partisan choice.

In this article I will demonstrate that the relationship between social class and partisan choice has declined, largely as a result of generational change. Strong relationships between class and party persist among older voters, but among younger voters these relationships are weak. The change largely results from relatively increased Democratic voting by younger members of the middle class. This shift among the young middle class cannot be explained in simple economic terms, nor is it the result of the differential social composition of the young middle class as compared with their elders. In examining these changes, this article will document the changing basis of American party alignments and, more important, will examine a process through which partisan realignments may occur.

The data base for the analysis will be the six presidential election surveys conducted by the Survey Research Center of the University of Michigan from 1948 through 1968.[3] The point of deprature is the discussion of social class and generational change by Angus Campbell and his colleagues.[4]

The Effect of the Depression upon Class and Party

The authors of *The American Voter* developed a measure of "status polarization" to assess the relationship of occupational ranking and direction of partisanship. The greater the tendency of the lower occupational groups to support the Democratic party and of the upper groups to support the Republicans the higher the measure of status polarization.[5] They found that status polarization had been declining in the years following World War II and suggested it would continue to remain low. "In view of the economic axis of class feelings," they write, "we would readily assume that status polarization should increase in time of depression, and decrease in periods of prosperity."[6] By examining changes in status polarization in postwar elections, they conclude:

> The most striking feature of the polarization trend in the recent past has been the steady and rapid

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[1] See Seymour Martin Lipset, *Political Man: The Social Bases of Politics* (Garden City, N.Y.: Doubleday, 1959), 285–309 for an extensive summary of findings through the mid-1950s.

[2] Robert E. Lane provides a useful summary of postwar economic changes, as well as a provocative discussion of the possible political consequences of affluence, in "The Politics of Consensus in an Age of Affluence," *The American Political Science Review,* 59 (December, 1965), 874–895. In particular, see pp. 884–889 for Lane's discussion of the effects of affluence upon partisan choice.

[3] These data were made available by the Inter-University Consortium for Political Research. The Consortium bears no responsibility for the analyses and interpretations presented here.

[4] See Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 356–361.

[5] For a discussion of status polarization, see Campbell et al., pp. 344–346.

[6] Campbell et al., pp. 356–357.

depolarization between 1948 and 1956. This decline occurred in a post-war period when the nation was enjoying a striking ascent to prosperity and a consequent release from the pressing economic concerns that had characterized the Depression.[7]

To strengthen their interpretation Campbell and his colleagues focused on the behavior of persons who were in their twenties and thirties during the Great Depression. In the 1948 and 1952 presidential elections, status polarization was highest among the "Depression generation." High status polarization among this age-group, they suggested, was the result of the economic conditions of the Depression era. They acknowledged that "when we deal with age we cannot be sure that patterns that make historical sense are not due instead to the phenomena of the life cycle,"[8] but they did not systematically compare a life-cycle and a generational explanation for their findings.

Their analysis of the 1956 election revealed that status polarization had dropped among every age group, but most strongly among the Depression generation. "It is this portion of the electorate," Campbell and his colleagues write, "that showed the greatest relative depolarization and that best illustrates the fading effects of the Depression."[9] But although age-group differences had all but disappeared, traces remained, and the authors surmised that generational effects may have become latent. They correctly concluded that data covering a longer time span were needed.

In his study of the changing relationship of social class to partisan choice, Robert R. Alford examined data over a 24-year time span.[10] Alford objected to Campbell's conclusion that the relationship between class and party was declining. He argued that in choosing 1948 as the starting point for their analysis, Campbell and his colleagues selected an atypical election. The relationship between class and party choice was higher in 1948 than in any previous election for which survey data are available, and by using 1948 as the beginning of a time-series analysis, one may incorrectly conclude that the relationship of class to party is rapidly declining. Based upon his own analysis, Alford concluded that "there has been no substantial shift in the class bases of American politics since the 1930's, despite the prosperity since World War II and despite the shifts to the Right in the Eisenhower era."[11] Alford also objected to the inferences that Campbell and his colleagues made about age-group differences. He argued that whether "age differences at one point in time truly reflect past behavior and the differential impact of a historical crisis is an inference which may or may not be justified."[12] But Alford did not conduct his own analysis of age-group differences.

My analysis of age-group differences over a twenty-year period suggests that Campbell and his colleagues were correct in their conclusion that the relationship of class to partisan choice was declining. Their historical interpretation of age-group differences was based upon inference, but my analysis suggests those inferences were correct. But the authors of *The American Voter* were premature in their judgment that generational effects had been eliminated. They overlooked the beginnings of a trend toward much lower levels of status polarization among young voters, a trend that largely accounts for the declining relationship of social class to partisan choice.

Social Class and Partisan Choice in Six Presidential Elections

Figures 1 through 6 present the proportion of middle- and working-class[13] voters who supported the Democratic presidential candidate[14] in each election from 1948 through 1968. The data are presented according to age-cohorts.[15] Like both Campbell et al. and Al-

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[7] Campbell et al., p. 357.

[8] Campbell et al., p. 357.

[9] Campbell et al., p. 359.

[10] Robert R. Alford, *Party and Society: The Anglo-American Democracies* (Chicago: Rand McNally, 1963), pp. 219–249.

[11] Alford, p. 226. Alford provides more recent analysis, which includes results through 1964, in "Class Voting in Anglo-American Democracies," in *Party Systems and Voter Alignments*, ed. S. M. Lipset and Stein Rokkan (New York: Free Press, 1967), pp. 84–88.

[12] Alford, *Party and Society*, pp. 224.

[13] Social class was defined on the basis of the head of household's occupation. If the head of household was not employed, class was defined on the basis of his occupation when he worked. Professionals, semi-professionals, self-employed, managerial, supervisory, and other white-collar workers were classified as middle class; skilled workers, semi-skilled workers, unskilled workers, service workers, and protective service workers were classified as working class. (This dichotomous division is identical to that employed by Alford in his analysis of the S.R.C. surveys.)

[14] Direction of vote is based upon the postelection report of how the respondent voted. Persons who did not vote, or who voted for minor parties, have been excluded from these figures, although the effects of including Wallace voters in the 1968 analysis are reported below.

[15] The term age-cohort is used to designate any group of persons born during the same time period. Except for the 1952 and 1968 surveys, in which year of birth was recorded, year of birth has been calculated from data about respondent's age.

To maximize longitudinal comparability the arbitrarily defined ten-year cohorts employed in the 1948

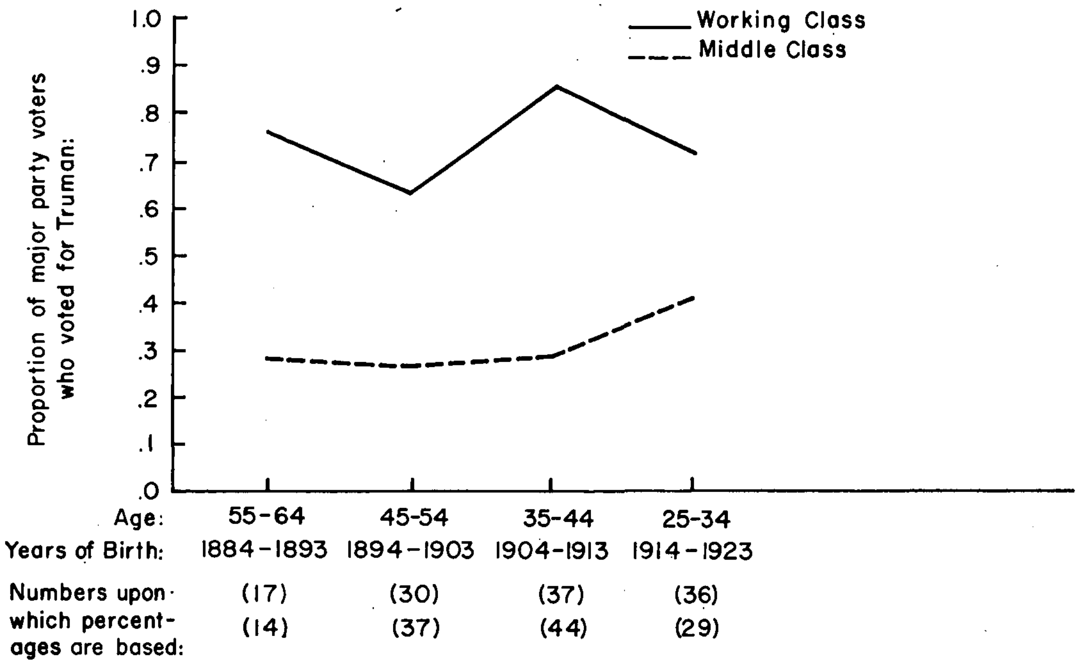

Figure 1. Proportion of Major Party Voters who Voted for Truman in 1948, by Social Class and Age (Among whites only; not including farmers).

ford, the present analysis excludes farmers. Unlike them, it excludes nonwhites, since the greatly increased turnout by blacks after 1960 makes longitudinal comparison difficult.[16]

One can compare similar age-cohorts across time by tracing age-groups from Figure 1 onward, but to facilitate longitudinal comparison I have used two summary measures of the relationship of social class to partisan choice. Class voting is the proportion of working-class respondents who voted Democratic minus the proportion of middle-class respondents who

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survey were used, even though more detailed data about age were available for subsequent surveys. For persons born after 1923, eight-year cohorts were used. Ideally four-year cohorts should have been employed since presidential elections are held every four years, but the small Ns precluded this refinement.

[16] As blacks are largely working class and heavily Democratic, to include blacks in the analysis would increase the proportion of working-class respondents who voted Democratic. Including blacks thus increases the relationship of social class to party choice, having its greatest impact upon that relationship when black turnout is high and when the proportion of white workers voting Democratic is low. Including blacks has little effect on the overall relationship between class and voting except in 1968, and these effects are noted below (see note 23).

Table 1. Class Voting[a], by Years of Birth in Six Presidential Elections[b]

Years of Birth:	1884–1893	1894–1903	1904–1913	1914–1923	1924–1931	1932–1939	1940–1947	X̄ for Total White Electorate
Year of Election:								
1948	48	36	57	31	†			44
1952	34	20	24	25	4			20
1956	13	12	8	6	5	†		8
1960	1	2	18	19	1	21		12
1964	24	26	25	25	12	8	†	19
1968	†	15	15	27	1	6	−3	10

[a] Class voting is the proportion of working-class respondents who voted Democratic minus the proportion of middle-class respondents who voted Democratic.

[b] Among whites only; not including farmers.

† Because of small Ns the index of class voting has not been presented.

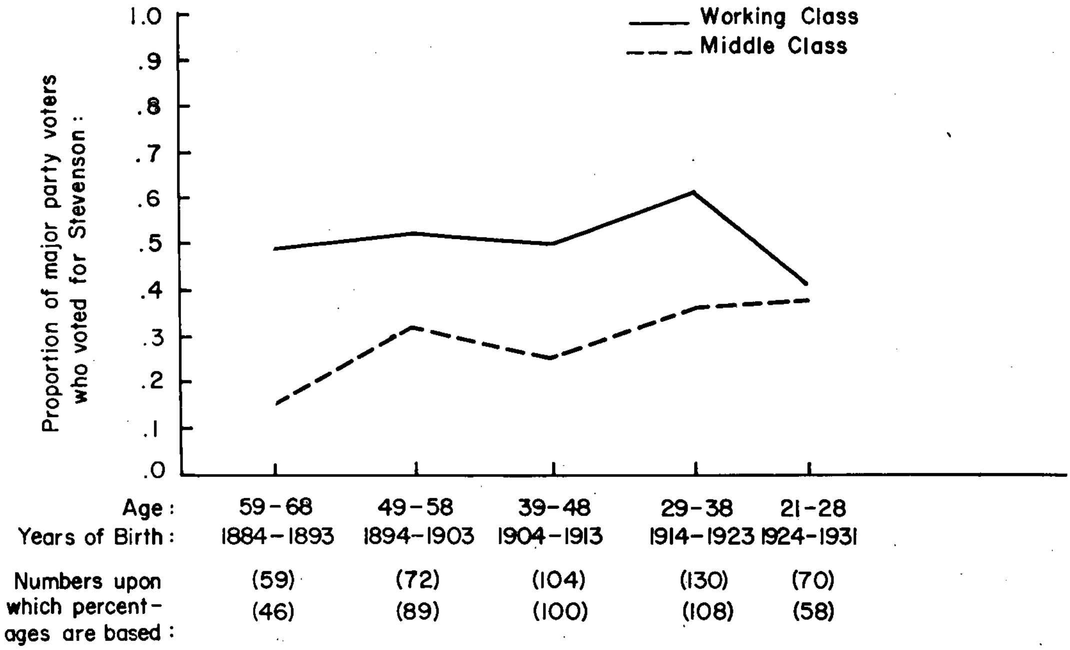

Figure 2. Proportion of Major Party Voters who Voted for Stevenson in 1952, by Social Class and Age (Among whites only; not including farmers).

voted Democratic.[17] Levels of class voting for each cohort in each election, as well as the mean level of class voting for the electorate, are presented in Table 1. In addition I have computed a measure of association between occupational ranking, classified on a five-point ordinal scale,[18] and direction of presidential vote using a Kendall's tau-c that closely approximates the statistical techniques used in *The American Voter*.[19] Following the Michigan authors, I have labeled this measure "status polarization." Levels of status polarization for each cohort in each election, as well as the over-all level of status polarization for the electorate, are reported in Table 2.

The overall relationship of social class to

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[17] For a discussion of this measure see Alford, *Party and Society*, pp. 79–86.

[18] The fivefold ordinal classification of occupational ranking was as follows: professionals and semi-professionals; self-employed, managerial, and supervisory; other white-collar; skilled and semi-skilled workers; unskilled workers, service workers, and protective service workers.

[19] Campbell and his colleagues used a tau-b to measure this relationship. It was impossible to replicate precisely the measure of occupational status used by the University of Michigan authors since there are some ambiguities in their description of their measure. However, my overall measures of status polarization for the total population closely approximate their results.

Table 2. Status Polarization[a], by Years of Birth in Six Presidential Elections[b]

Years of Birth:	1884–1893	1894–1903	1904–1913	1914–1923	1924–1931	1932–1939	1940–1947	$\bar{X}$ for Total White Electorate
Year of Election:								
1948	.62	.40	.65	.26	†			.48
1952	.38	.23	.25	.26	.01			.21
1956	.03	.14	.11	.08	.09	†		.10
1960	.11	.13	.20	.12	.01	.28		.12
1964	.35	.26	.24	.30	.19	.05	†	.21
1968	†	.16	.21	.28	.04	.12	−.03	.12

[a] Status polarization is the Kendall's tau-c relationship between a five-fold ordinal measure of occupation and direction of presidential vote.

[b] Among whites only; not including farmers.

† Because of small Ns the level of status polarization has not been presented.

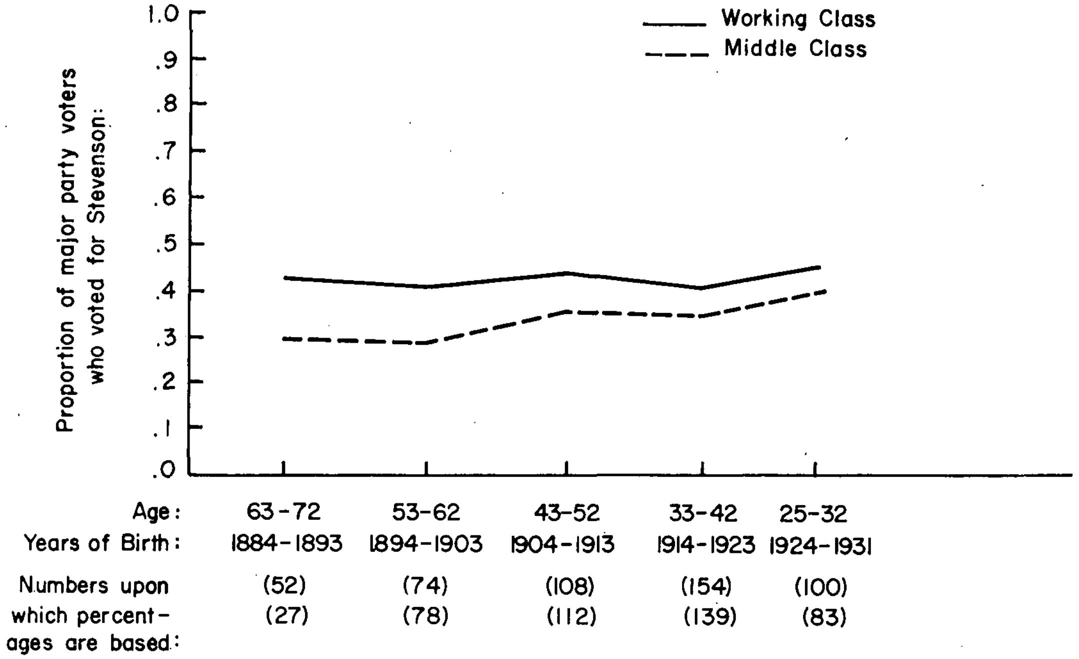

Figure 3. Proportion of Major Party Voters who Voted for Stevenson in 1956, by Social Class and Age (Among whites only; not including farmers).

partisan choice shows a marked decline during the postwar years. Admittedly class voting was atypically high in 1948. Alford's analysis showed class voting to have been +16 in 1936, +25 in 1940, and about +18 in the wartime election of 1944.[20] My own analysis of the National Opinion Research Center survey of the 1944 electorate shows class voting[21] among whites to have been +19.[22] Given the atypically high relationship between social class and party choice in 1948 it may be misleading to use that year as the starting point in a time-series analysis. But even if one uses 1952 as a starting point, overall levels of class voting have been declining. Class voting was +20 in 1952 and did not reach this level in any subsequent election (see Table 1). Similar conclusions are reached by examining changes in levels of status polarization (see Table 2). Status polarization was .21 in 1952; it was markedly lower in three of the four subsequent elections.[23] Moreover, the relationship of class to voting in 1968 is even lower if Wallace voters are included in the analysis, for Wallace drew disproportionate strength from the working class.[24] Including Wallace voters as Republicans reduces class voting to only +6, status polarization to only .08.[25]

Although Campbell and his colleagues were correct in suggesting that the relationship of class to partisan choice was declining, they may have been premature in their judgment that the effects of the Depression had faded away. The Depression cohort born between 1904 and 1913 had the highest level of class voting and of status polarization in 1948. With the exception of the 1956 election, in which differences between age-groups were all but eliminated, that cohort had higher than the

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[20] See Alford, *Party and Society*, pp. 352–353.

[21] In the calculation of levels of class voting in 1944, service workers were classified as working class. In his analysis of the 1944 N.O.R.C. surveys, Alford classified service workers as middle class. Regardless of whether service workers are considered working class, middle class, or are excluded from the analysis, respondents between the ages of 30 and 39 (born between 1905 and 1914) manifested higher levels of class voting than did the other cohorts studied. A strong cautionary note is in order. Given that the 1944 sample was of the civilian population, the youngest cohort sampled in 1944 may be highly atypical since a large proportion of the men were in the armed forces.

[22] A measure of status polarization comparable with that used in this study cannot be calculated, since the N.O.R.C. did not use the same fivefold occupational ranking employed by the Survey Research Center.

[23] As noted above (see note 16), including blacks has an impact on the association between class and voting in the 1968 election. Including nonwhites raises class voting in 1968 to +15, status polarization to .18.

[24] Among working-class white voters, 15 per cent voted for Wallace; among middle-class white voters, 10 per cent did.

[25] If nonwhites are included, class voting rises to +11, status polarization to .14.

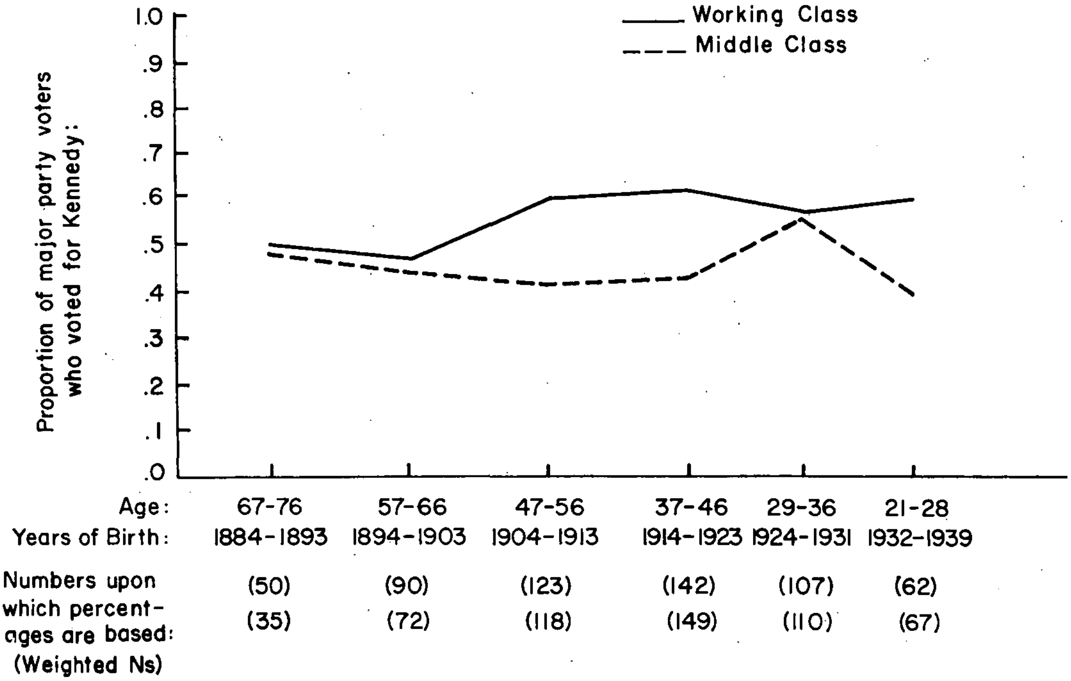

Figure 4. Proportion of Major Party Voters who Voted for Kennedy in 1960, by Social Class and Age (Among whites only; not including farmers).

mean level of class voting in all subsequent elections. It was the only cohort that never had lower than the mean level of class voting in any presidential election (see Table 1). Moreover, the 1904–1913 cohort was the only age-group that consistently had higher than the mean level of status polarization in all six elections (see Table 2).

But focusing on the distinctive effects of the Depression upon a single cohort leads one away from the analysis of far more striking age-group differences. In their analysis of the 1952 election Campbell and his colleagues overlooked the marked differences between all older cohorts and the youngest cohort that had entered the electorate after World War II. Whereas social class was strongly related to partisan choice among the cohorts born before 1924, there was virtually no relationship between class and presidential vote among voters born between 1924 and 1931 (see Figure 2, Tables 1 and 2). Campbell and his colleagues may have felt it prudent to overlook this difference. Their only data point after 1952 was 1956, when cohort differences were negligible. Any age-group differences discovered at only one point in time could be the result of sampling error. But, more important, the low relationship of social class to partisan choice among young voters could be due to "the phenomena of the life cycle." Indeed it is easy to develop a plausible life-cycle explanation for this low relationship.

A life-cycle explanation for this relationship assumes that young persons are less affected by their social class than older persons are because the young have had less experience in the work force than their elders and have had less time to learn the social and political norms of their social class. Moreover, many of the young will have recently crossed class lines as a result of intergenerational social mobility. Both upwardly and downwardly mobile individuals may be slow to accept the norms of their social class. A life-cycle explanation suggests that as young persons gain more experience in the work force they will become more fully socialized to the norms of their social class. As this socialization occurs, an increasing proportion will learn to support the prevalent party of their class. The net effect of this socialization (and resocialization) will be an increasing relationship between social class and partisan preferences. If the life-cycle explanation were valid, the low relationship of class to party among young voters in 1952 could not be interpreted as the beginning of a trend, since that relationship would rise in subsequent elections.

But the low relationship between class and party among the young could also be ex-

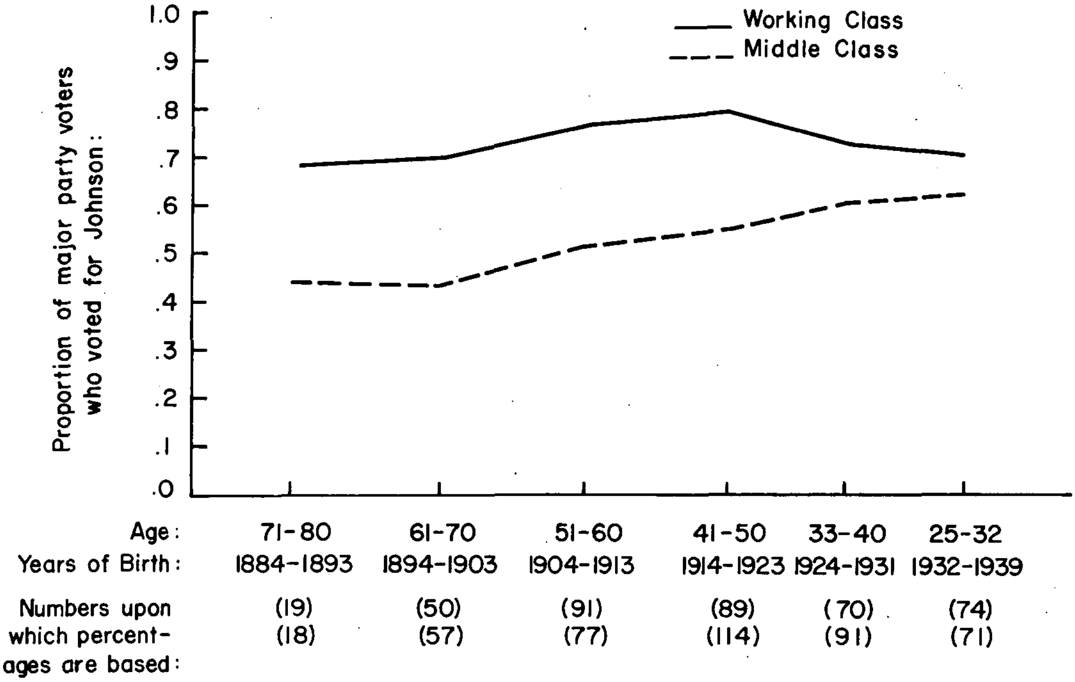

Figure 5. Proportion of Major Party Voters who Voted for Johnson in 1964, by Social Class and Age (Among whites only; not including farmers).

plained by a generational explanation similar to that used (but not fully developed) by Campbell and his colleagues to account for the behavior of the "Depression generation." The young may have had formative experiences different from those of their elders. Whereas their elders may have had formative experiences that contributed to a high relationship between social class and partisan choice, young voters may have had experiences that contributed to a low relationship. Before attempting to discriminate between explanations, it is necessary to examine more fully the assumptions that constitute a generational explanation.

A political generation is "a group of persons who have undergone the same basic historical experiences during their formative years."[26] A generational explanation for age-group differences assumes that there is a formative socialization period during which fairly enduring attiitudes are formed; after that, attitudes tend to become relatively stable. While such assumptions are not valid for all political attitudes, there is a good deal of evidence that they may be valid for party loyalties.[27] Many persons learn and accept their parents' party preferences, a factor that mitigates against partisan change. The most important experiences in changing a person's partisan loyalties probably occur between the time he enters the work force and the first few elections in which he participates.[28] If economic, social, and political conditions contribute to a strong relationship between class and partisanship during this formative period that relationship will tend to persist. Even if many individuals change their partisan preferences in a given election, the net relationship between class and party for the generation will tend to be high.[29] But

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[26] Marvin Rintala, "Political Generations," in *International Encyclopedia of the Social Sciences* (New York: Macmillan, 1968), vol. 6, p. 93. Such experiences, as Rintala notes, can create divisions within generations as well as between generations.

[27] See Converse, "Of Time and Partisan Stability," *Comparative Political Studies*, 2 (July, 1969), 139–171. See also, Campbell et al., *The American Voter*, pp. 125–128.

[28] Obviously, many individuals will have formative socialization experiences at later stages in their life. Some individuals will experience career mobility from one class to another. The generational model assumes that for most persons late adolescence and early adulthood will be especially significant in shaping partisan attachments, and that for many persons these attitudes will be relatively stable.

[29] As is apparent from the data in this article, the net relationship between class and party for any given cohort will vary from election to election. Even for the Depression cohort that relationship was weak in 1956 when the conditions of that election contributed to a low relationship between class and party.

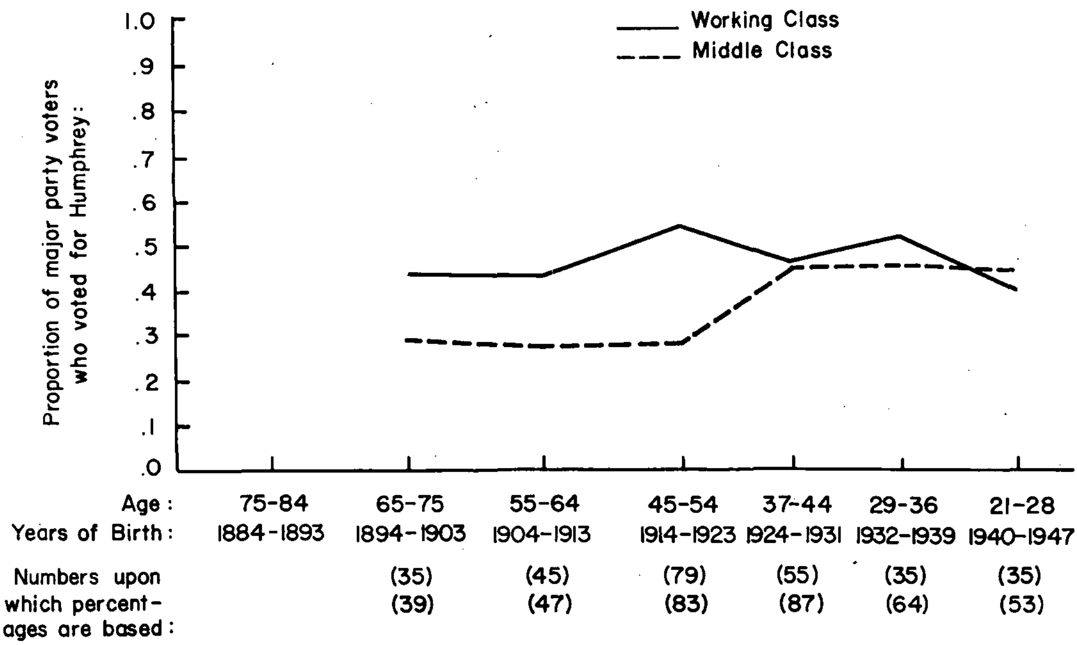

Figure 6. Proportion of Major Party Voters who Voted for Humphrey in 1968, by Social Class and Age (Among whites only; not including farmers).

if conditions during this formative period contribute to a low relationship between class and party, that relationship will continue to remain low.

If the assumptions that constitute the generational explanation are correct, most persons between the ages of 21 and 28 in 1952 had their critical socialization experiences during and after World War II. The 1952 election was the first presidential election in which most of them participated,[30] and many would still be relatively open to partisan change. The nature of that change would depend upon the historical experiences of the young during the years after 1952. A generational explanation would not necessarily predict either a rise or decline in the relationship of social class to partisan choice. But, given the absence of class-based politics during the Eisenhower period, one would expect the relationship of class to partisanship to remain relatively low.

The life-cycle and generational explanations thus lead to contrasting predictions about the future relationship between social class and partisanship.[31] If the low relationship of social class to partisanship among the youngest cohort in 1952 resulted from its relatively recent entry into the work force, we would expect that relationship to increase as that cohort gained more experience in the work force and as persons became more fully socialized to the norms of their social class. The relatively low relationship of class to party would thus be seen as a temporary phenomenon, and therefore overall relationships between class and party would be expected to continue for the proximate future. But if the low relationship between class and partisanship among the young resulted from fundamental differences between the socialization of these respondents and older respondents, one would predict that the overall relationship between social class and partisan choice would decline as new cohorts, less polarized by virtue of their historical experience, entered the electorate.

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[30] Based upon recall questions in the 1952 survey, 42 per cent of the voters born between 1924 and 1931 voted in 1948. (Among 1952 voters born between 1924 and 1927, 68 per cent had voted in 1948.) Even among those who had voted in 1948, class voting in 1952 was only +6. Since the Ns for this subset of 1948 voters are low, one should be cautious in making inferences from this finding. But there is little evidence that participation in the 1948 election was a formative experience contributing to high levels of class voting among first-time participants in that election.

[31] It should be clear from this discussion that there could be circumstances under which these two explanations would lead to similar predictions. In such cases a longitudinal comparison could not distinguish between the explanations.

These explanations can be tested by a longitudinal cohort analysis.[32] Since there are no panel data over a long time-span, one cannot compare identical respondents at different points in time; but one can compare each cohort sampled in 1952 with similar cohorts sampled in subsequent elections.[33] A life-cycle explanation predicts that the relationship of class to party will rise among the 1924–1931 cohort as the mean age of that cohort increases; a generational explanation predicts that the relationship will remain relatively low.

By tracing the relationship of class to partisan choice in four subsequent presidential elections, one finds striking support for the generational explanation. Although levels of class voting undergo minor fluctuations, class voting among the 1924–1931 cohort consistently remains low (see Table 1). A similar pattern is found by examining levels of status polarization (see Table 2). Although there are some minor fluctuations, status polarization for this cohort consistently remains below the mean level for the electorate. Although the mean age of the 1924–1931 cohort has risen by sixteen years by 1968, the relationship of social class to partisan choice remains negligible.

The 1932–1939 cohort was first analyzed during the 1960 presidential election (see Figure 4). The relatively high relationship of social class to partisanship among this cohort is probably an anomaly.[34] Nevertheless, alternative explanations of partisan change lead to different expectations about the future behavior of that cohort. A life-cycle explanation predicts that the relationship between class and party will rise as that cohort gains greater experience in the work force. But, given the absence of class-based conflict in the 1960s, a generational explanation predicts that class voting will not rise; to the extent that the young are relatively flexible in their partisan attitudes, the relationship between class and party, indeed, might decline.

By tracing the relationship of class to partisan choice in two subsequent elections, one finds little support for the life-cycle explanation. In 1964, contrary to the predictions of the life-cycle explanation, both class voting (see Table 1) and status polarization (see Table 2) declined markedly. Class voting among this cohort declined further in 1968, although status polarization rose to the mean level for the electorate; including Wallace voters reduces class voting to 0, status polarization to .03.

The youngest cohort sampled in 1968 (see Figure 6) presents the only case among 32 comparisons in which the middle class more often voted Democratic than did the working class (see Table 1). It is the only cohort for which there is a negative correlation between low occupational ranking and voting Democratic (see Table 2). Because Wallace drew disproportionate strength from the young working class, to include Wallace voters in the calculations (classifying them with the Republicans) further reduces both class voting and status polarization; class voting falls to −7, status polarization to −.08.[35] Since this finding is based upon only one point in time, one cannot discriminate between alternative explanations. But the reversal of the relationship between class and party among young voters may prefigure the trend discovered by surveys in 1972: Young college voters were more likely to support the Democratic nominee than were young blue-collar workers.

On balance a longitudinal cohort analysis provides substantial evidence that the relationship of class to partisan choice is much lower among those who entered the electorate after World War II than among those who entered the electorate before and during the War. The low relationship of class to party among the postwar electorate appears to be the result of generational change. The argument that age differences result from differential historical experiences is, of course, based upon inference, but such inferences become stronger when patterns are consistently found at many points in time and when alternative explanations can be rejected.

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[32] For an excellent discussion of the methodology of cohort analysis, see Herbert H. Hyman, *Secondary Analysis of Sample Surveys: Principles, Procedures, and Potentialities* (New York: Wiley, 1972), pp. 274–290.

For a cohort analysis that tests the explanations developed here within the context of European political behavior, see Paul R. Abramson, "Social Class and Political Change in Western Europe: A Cross-National Longitudinal Analysis," *Comparative Political Studies,* 4 (July, 1971), 131–155.

[33] A basic assumption of cohort analysis is that one has randomly selected members of each cohort. All six S.R.C. surveys were based upon national random samples. Even when a random sample is used, however, the probability that subgroups will represent the universe of persons who belong to those subgroups is less than the probability that the total sample will reflect the total universe of persons from whom the sample was drawn. On the other hand, when the same relationships occur among a subgroup in numerous surveys based upon separately conducted samples one can have more confidence that the relationships that result have not occurred by chance. (It should be noted that more than half the respondents sampled in 1960 were drawn from a panel survey begun in 1956.)

[34] The low relationship of social class to partisan choice among the two oldest cohorts in 1960 results from high levels of Republican voting among working-class Protestants. But I do not have a satisfactory explanation for the high relationship of class to party choice among the youngest cohort.

[35] Norval D. Glenn combined the results of five Gallup surveys conducted shortly after the 1968 election and his results are based upon Ns substantially larger than those in my study. He found that among whites between the ages of 21 and 29 white-collar workers were more likely to vote Democratic than blue-collar workers, a difference which became more pronounced if Wallace voters were included in the analysis. See Norval D. Glenn, "Class and Party Support in the United States: Recent and Emerging Trends." *Public Opinion Quarterly,* 37 (Spring, 1973), 1–20.

The *overall* decline in the relationship of social class and partisan choice has largely occurred because an ever-increasing number of voters born after 1923 have entered the electorate.[36] Voters born before 1924 have shown fluctuations in the relationship of class to voting from election to election, but only a slight decline between 1952 and 1968. Thus among voters born between 1884 and 1923 class voting was +25 in 1952, although it fell to +8 in 1956 and was only +13 in 1960; class voting among this cohort was +25 in 1964 and +19 in 1968. A similar pattern emerges for levels of status polarization. Among voters born between 1884 and 1923 status polarization was .27 in 1952, but dropped to .10 in 1956 and was only .12 in 1960; status polarization among this age group rose to .28 in 1964 and was .22 in 1968. (If Wallace voters are included, class voting drops to +15, status polarization to .17.) But among voters born *after* 1923, both class voting and status polarization remained low in all five elections. Obviously, the proportion of the electorate born after 1923 grew in each election (from 15 per cent of the major party voters in 1952 to 49 per cent in 1968) and, quite obviously, will continue to grow in each subsequent election.

Partisan Change among the Middle Class

The low relationship of class to party among the postwar electorate is mainly the result of high levels of Democratic voting among middle-class voters born after 1923.[37] In every election middle-class voters born after 1923 were more likely to vote Democratic than middle-class voters born before 1924.[38] The tendency of the postwar middle-class electorate to vote Democratic appears to be a generational difference, not the transient result of its youth. The relatively high level of Democratic voting among the youngest middle-class cohort in 1952 persisted in all subsequent elections (see Figures 2 through 6). Although support for Kennedy was low among the youngest middle-class cohort in 1960, that cohort manifested relatively high levels of Democratic voting in the two subsequent elections (see Figures 4 through 6). And the youngest middle-class cohort in 1968 also provided a relatively high level of support for the Democrats (see Figure 6).

Despite substantial differences between the 1964 and 1968 elections,[39] in both of them the younger members of the middle class were more likely to vote Democratic than their elders. In 1964, among major party voters, 51 per cent of the older middle class voted for Johnson, whereas 62 per cent of the young middle class voted Democratic.[40] Among major party voters in 1968, 29 per cent of the older middle class voted for Humphrey, whereas 45 per cent of the younger middle class voted Democratic.[41] A more extensive analysis of middle-class voting behavior in these two elections provides an opportunity to test additional explanations for the Democratic preferences of the postwar middle-class electorate.

The tendency of those in the younger middle class to vote Democratic cannot be explained by their economic circumstances. My analysis of the 1964 and 1968 S.R.C. surveys shows that in both years younger middle-class voters tended to occupy higher occupational positions within the middle class than did their elders. In 1964 the younger middle-class voters had marginally lower family incomes than did their elders, but in 1968 the younger middle class had higher family incomes than older members of the middle class. Nor was the greater Democratic voting of the younger middle class due to a tendency to perceive themselves as members of the working class. In both 1964 and 1968 younger members of the middle class were more likely than their elders to identify themselves as middle class. In short, younger members of the middle class voted more Democratic than their elders even though their economic circumstances, as well as their subjective class identifications, should have tended to make them vote Republican.

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[36] There is some historical basis for this division. Persons born before 1924 were first eligible to vote before or during World War II; persons born after 1923 first entered the electorate after the War. But the generational explanation obviously does not suggest sharp cut-off points between persons born in one year and those born the next, since the age at which formative socialization occurs will vary from individual to individual.

[37] In 1952, the decline in class voting was more the result of the low level of support for Stevenson among the young working class. In all subsequent elections there were only negligible differences in levels of Democratic voting among workers born before 1924 and those born after 1923.

[38] Such differences are obvious for 1952, 1956, 1964 and 1968 merely by inspecting the appropriate figures Even in 1960, however, the mean level of Democratic voting among middle-class voters born after 1923 was marginally higher than that of middle-class voters born before 1924. Data for 1948 also support this trend, bu the Ns are too small to be meaningful.

[39] For a comparison of the social bases of partisan support in these two elections, see Philip E. Converse Warren E. Miller, Jerrold G. Rusk, and Arthur C Wolfe, "Continuity and Change in American Politics Parties and Issues in the 1968 Election," *The American Political Science Review*, 63 (December, 1969), 1083-1105.

[40] These percentages include persons born between 1940 and 1943 who were not included in Figure 5.

[41] These percentages include persons born betwee 1884 and 1893 who were not included in Figure 6.

In part, the Democratic leanings of the younger middle class can be explained in terms of its political inheritance and its ethnic origins. Because of the net movement of persons into the middle class, many with working-class Democratic fathers attained middle-class positions. Moreover, the movement into the middle class has been especially marked among ethnics, because second and third generation Americans have had greater opportunities for advancement than their parents had.[42] As a result of these changes, younger members of the middle class are more likely to have Democratic fathers than their elders. They are also more likely than their elders to be Catholics.[43] Since both having a Democratic father and being a Catholic contribute to voting Democratic, the Democratic tendencies of the younger middle class could result from inherited Democratic loyalties and the political norms and traditions of their religious group.

In both 1964 and 1968 controlling for father's party and respondent's religion provided a partial explanation for the Democratic preferences of younger middle-class voters. As may be seen by examining Table 3, in 1964 the tendency of those in the younger middle class to vote Democratic more often than their elders was reduced among voters with Republican fathers. As may be seen by examining Table 4, in 1968 the tendency of the younger middle class to vote Democratic was reduced among respondents with both Republican and Democratic fathers. In both 1964 and 1968 controlling for respondent's religion reduced the tendency of the younger middle class to be more Democratic than their elders (see Tables 3 and 4). Nonetheless, in both years the ten-

Table 3. Proportion of Middle-Class Major Party Supporters Who Voted for Johnson in 1964, by Years of Birth, Father's Party, and Religion[a]

Years of Birth: Age:	1884–1923 41–80		1924–1943 21–40		Democratic Advantage among Younger Middle-Class Voters
Father's Party Preference:					
Democratic	57%	(115)[b]	70%	(97)	+13
Republican	40%	(94)	47%	(59)	+7
Respondent's Religion:					
Catholic	70%	(67)	74%	(54)	+4
Protestant	41%	(175)	49%	(109)	+7
Father's Party Preference and Respondent's Religion:					
Father Democratic, Respondent Catholic	76%	(34)	80%	(35)	+4
Father Democratic, Respondent Protestant	48%	(71)	57%	(51)	+9
Father Republican, Respondent Catholic	44%	(16)	58%	(12)	+15
Father Republican, Respondent Protestant	38%	(74)	39%	(41)	+1

[a] Among whites only; not including farmers.
[b] Numbers in parentheses are the totals upon which percentages are based.

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[42] See Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* (New York: Wiley, 1967), pp. 231–238.

[43] In this analysis being Catholic provides an indirect measure of having ethnic origins that contributed to voting Democratic. Although the Survey Research Center surveys for 1964 and 1968 provided detailed data about ethnic origins, there was a tendency for respondents to report themselves as "American only" if both parents were born in the United States. When ethnic origin was used as a variable, the younger middle class was only marginally more likely to have Irish, East European, and Southern European origins than was the older middle class.

Table 4. Proportion of Middle-Class Major Party Supporters Who Voted for Humphrey in 1968, by Years of Birth, Father's Party, and Religion[a]

Years of Birth:	1884–1923		1924–1947		Democratic Advantage among Younger Middle-Class Voters
Age:	45–84		21–44		
Father's Party Preference:					
Democratic	45%	(71)[b]	58%	(100)	+13
Republican	15%	(72)	26%	(62)	+11
Respondent's Religion:					
Catholic	49%	(35)	58%	(60)	+10
Protestant	22%	(136)	28%	(117)	+6
Father's Party Preference and Respondent's Religion:					
Father Democratic Respondent Catholic	55%	(22)	69%	(35)	+14
Father Democratic, Respondent Protestant	36%	(44)	37%	(49)	0
Father Republican Respondent Catholic	(2)	(4)	38%	(16)	
Father Republican, Respondent Protestant	14%	(66)	20%	(44)	+7

[a] Among whites only; not including farmers.
[b] Numbers in parentheses are the totals upon which percentages are based.

dency of the younger middle class to vote Democratic more often than their elders persisted. In both 1964 and 1968 the tendency of the younger middle class voters to be Democratic persisted even when both father's party and respondent's religion were simultaneously controlled (see Tables 3 and 4).

The tendency of the younger middle class to vote Democratic seems to go beyond the differing social origins of the young middle class. In part it reflects attitudinal change. Younger members of the middle class were more likely than their elders to support an active federal government. In 1964, young middle class voters were more supportive of an active federal government than their elders on a wide variety of domestic issues. In 1968, age differences were not as marked, but the young were still more supportive of government activism on a large number of issues. Given the comparability of items used in the 1964 and 1968 surveys it was possible to construct an identical index of support for federal government activism for both years.[44] Such an index provides a convenient mechanism for comparing levels of Democratic support among younger and older middle-class respondents while controlling for the more liberal policy preferences of the young.

In both years, but especially in 1964, support for government activism was strongly re-

Table 5. Proportion of Middle-Class Major Party Supporters Who Voted for Johnson in 1964, by Years of Birth and Score on Government Activism Index[a]

Year of Birth:	1884–1923		1924–1943		Democratic Advantage among Younger Middle-Class Voters
Age:	41–80		21–40		
Score on Government Activism Index:					
High	78%	(64)[b]	87%	(60)	+9
Medium	58%	(99)	60%	(68)	+3
Low	28%	(103)	37%	(57)	+9

[a] Among whites only; not including farmers. See note 44 for a description of this index.
[b] Numbers in parentheses are the totals upon which percentages are based.

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[44] The four items used measured support for federal government involvement in aiding education, promoting jobs and prosperity, supporting school integration, and guaranteeing public accomodations for Negroes. (The specific items were Variables 66, 78, 100, and 408 in 1964 and Variables 60, 66, 75, and 78 in 1968.)

Respondents scoring "high" on the index generally favored federal government activism in three or more of these areas; those scoring "low" generally opposed government involvement in three or more of these areas.

Table 6. Proportion of Middle-Class Major Party Supporters Who Voted for Humphrey in 1968, by Years of Birth and Score on Government Activism Index[a]

Years of Birth: Age:	1884–1923 45–84		1924–1947 21–44		Democratic Advantage among Younger Middle-Class Voters
Score on Government Activism Index:					
High	42%	(52)[b]	61%	(76)	+18
Medium	25%	(55)	43%	(65)	+18
Low	22%	(72)	27%	(63)	+5

[a] Among whites only; not including farmers. See note 44 for a description of this index.
[b] Numbers in parentheses are the totals upon which percentages are based.

lated to voting Democratic. In both years (see Tables 5 and 6) the tendency of the younger middle class to be more Democratic than their elders was somewhat reduced once controls for policy preferences were introduced. Nonetheless, the Democratic tendencies of the young persisted regardless of attitudes toward an active federal government.

This analysis strongly suggests that younger members of the middle class will continue to vote more Democratic than their elders. In part, this tendency will result from the social composition of the younger middle-class voters, in part from their liberal policy preferences. If new middle-class cohorts continue to provide disproportionate support for the Democrats, and if those in the young working class continue to be no more supportive of the Democratic party than their elders, the relationship between social class and partisan preference will continue to decline as less polarized cohorts make up a continually larger proportion of the electorate.

Conclusion

Several qualifications are necessary to delimit what this analysis has demonstrated and, just as important, what it has not. The study was restricted to the relationship between social class, occupationally defined, and party preference, defined as a choice between the two major parties. It leads to the projection that, unless unforeseen events occur, the relationship between social class and party choice will continue to decline. The analysis does not address itself to the possibility that new forms of class conflict may emerge outside the party system, within a major party, or through a third party movement. Nor does it examine the possibility that as stratification systems become more complex, newer bases of social cleavage may emerge.

Despite these limitations, the findings are important for two reasons. First, the changing class bases of partisan support may have considerable impact upon the nature of party competition in America, since such change may dramatically alter the composition of the Democratic coalition. Although the Democratic party benefits by receiving more support from the postwar middle-class electorate than from older members of the middle class, this advantage will not necessarily contribute to electoral success. The problem for the Democrats is not just to fare better among the postwar middle class than among their elders, but to gain the support of a majority of the electorate. Yet, the Democrats have gained a majority of the young middle-class votes only in the Johnson landslide of 1964. Moreover, Democratic electoral success in postwar elections has usually been based upon high levels of working-class support. In the six elections studied, the Democrats won two of the three elections in which the relationship of class to partisan choice was relatively high.

These findings are also important because they tell us a great deal about one process through which partisan realignments may occur, namely through generational change in which the relationships between social characteristics and partisan preferences among older generations tend to persist, although older generations are gradually replaced by newer generations in which different relationships prevail.

Membership Stability in Three State Legislatures: 1893–1969*

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Introduction

It is often reported that membership turnover in American state legislatures is quite rapid. It is also reported that this short tenure in office is not caused as much by electoral defeat as by unwillingness on the part of incumbents to seek re-election.[1] Many legislators have been characterized as rank amateurs who seek office for a wide variety of reasons and have little or no commitment to a legislative career.[2]

These statements about the short tenure and rapid turnover of state legislators are usually based on the high proportion of first-term members in many legislatures. It has been asserted, for example, that "more than half" of America's approximately 7,800 state legislators "must be replaced every other year."[3] This assertion is frequently echoed in current textbooks.[4]

To what extent is the assertion based on empirical evidence? This article attempts to answer that question, as well as four related questions: (1) To what extent have there been changes over time in the number of first-term legislators? (2) To what extent is the number of first-term legislators a valid measure of membership stability? (3) What other measures of membership stability might be employed, and how have such measures changed over time? (4) What are the possible causes and consequences of membership stability in state legislatures?

The Need for Historical Research

The frequent assertion that more than half of America's state legislators must be replaced every two years is based on a 1954 report by the APSA Committee on American Legislatures.[5] That report included data on the length of service of all state legislators serving in 1950. Although the report explicitly states that "over half" of the legislators are new at each session, the Committee's data do not completely support the statement. Computations based on the data indicate that, in 1950, the composite percentage of freshmen members for upper and lower houses was 42 per cent.[6]

In addition to the Committee's report, there are only three other sources of comparative data on the number of freshmen in state legislatures: a 1966 article by Lockard,[7] a frequently cited 1938 article by Hyneman,[8] and *The Legislative System* by Wahlke, Eulau, Buchanan, and Ferguson.[9] In varying degrees, all these studies make interstate comparisons, but none of them attempts to be historically comparative.[10] That is, all of them offer data on the number of freshmen legislators at a fixed point in time, but none of them seek to identify changes in that number over time. Even the Hyneman article, which draws its data from six different legislative sessions, inexplicably lumps the data into a single set of composite figures. Thus there has been no single, systematic attempt to identify historical trends in the turnover of state legislators.

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* The author is indebted to John F. Manley, Heinz Eulau, Robert Packenham, and Scott Richardson for their criticism of an earlier draft of this article.

[1] Daniel R. Grant and H. C. Nixon, *State and Local Government in America,* 2nd ed. (Boston: Allyn and Bacon, 1968), p. 242.

[2] Duane Lockard, "The State Legislator," in The American Assembly, *State Legislatures in American Politics* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), p. 103.

[3] James David Barber, *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven: Yale University Press, 1965), p. 8.

[4] For examples, see Grant and Nixon, p. 242; Thomas R. Dye, "State Legislative Politics," in *Politics in the American States,* ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown, 1965), p. 170; and G. Theodore Mitau, *State and Local Government: Politics and Processes* (New York: Scribners, 1966), p. 63.

[5] Belle Zeller, ed., *American State Legislatures: Report of the Committee on American Legislatures, American Political Science Association* (New York: Thomas Y. Crowell, 1954), pp. 65–67.

[6] The computation is reported in Lockard, p. 103.

[7] *Ibid.*

[8] Charles S. Hyneman, "Tenure and Turnover of Legislative Personnel," *Annals of the American Academy of Political and Social Science,* 195 (January, 1938), pp. 21–31.

[9] John C. Wahlke, Heinz Eulau, William Buchanan, and LeRoy C. Ferguson, *The Legislative System: Explorations in Legislative Behavior* (New York: John Wiley and Sons, 1962).

[10] The Lockard article presents data on the number of first-term legislators serving in all 50 states in 1963. *The Legislative System* includes similar data for four states in 1957. The Hyneman article presents length-of-service data on legislators in ten states during the period 1925–1935.

While one could seek such trends through secondary analysis of the available data, attempts to do so have yielded contradictory results. In one instance, Jewell and Patterson drew upon the data presented by Hyneman, the APSA Committee, Wahlke et al., and several single-state case studies.[11] For fourteen states, they were able to compare the percentage of first-term legislators in at least two different time periods. Of the fourteen states considered, twelve showed a marked decline in the percentage of first-term legislators.[12] In another study, Lockard compared Hyneman's 1925–1935 data with his own 1963 data.[13] In 1925–1935, the ten state legislatures considered by Hyneman contained an average of 35.4 per cent freshmen. In 1963, according to Lockard's data, the legislatures of all 50 states contained an average of 34 per cent freshmen. Thus while Jewell and Patterson report a marked decline in the number of first-term legislators, Lockard, considering the same general period and using some of the same data, reports that "just over one-third of all legislators are newcomers, a percentage that apparently has not varied greatly during the last few decades."[14]

Which of these conflicting views is correct? This is a straightforward and strictly empirical question, but the simple truth is it cannot be answered on the basis of available data. There has simply been too little historical research on turnover in state legislatures.

A Look at the Record

This paper is a first step toward meeting the need for such research. It begins by examining the number of freshmen members in three state legislatures during the period 1893–1969. During this period, there were forty biennial sessions of each legislature. This paper examines every second session, beginning with 1893, for a total of twenty sessions. The three legislatures considered are those of Connecticut, Michigan, and Wisconsin. These states were chosen on the basis of data availability.

It is in no way suggested that these three states are necessarily representative of other states. While some interstate variations will be noted, the principal focus is on intrastate variations over time. In other words, the twenty legislative sessions examined are assumed to be representative of the forty sessions which actually occurred in each state.

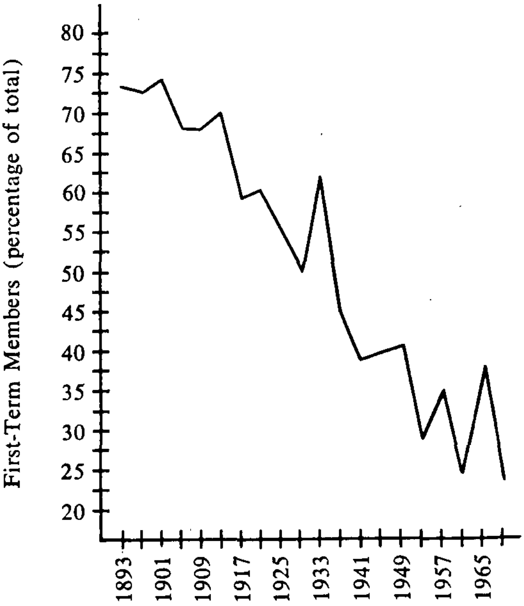

Figure 1. First-Term Legislators. (Composite average for six legislative houses.)

The percentage of freshmen in each house of the three legislatures is presented in Table 1. The figures in this table were computed from biographical sketches and other data available in various state publications.[15] While the figures in Table 1 speak for themselves, four points should be emphasized:

(1) All six houses have had a substantial decline in the number of first-term legislators. This decline is vividly exemplified by the Michigan House of Representatives, which contained 80 per cent freshmen in 1893 and only 14 per cent in 1969.

(2) While this substantial decline in the number of freshmen legislators has been gradual and undeniable, there have been a number of small, temporary reversals within the larger trend. (See Figure 1.) It is this irregularity which can mislead observers who examine legislative turnover at only two or three points in time.

(3) In addition to the irregularity of the decrease, there have been occasional "plateau"

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[11] Malcolm E. Jewell and Samuel C. Patterson, *The Legislative Process in the United States* (New York: Random House, 1966), pp. 119–121.

[12] *Ibid.*, p. 120.

[13] Lockard, pp. 103–106.

[14] *Ibid.*, p. 103.

[15] Data were taken from appropriate issues of the following documents: State of Connecticut, *State Register and Manual* (Hartford: Office of the Secretary of State, published annually); State of Michigan, *Michigan Manual* (Lansing: Office of the Secretary of State, published biennially); State of Wisconsin, *The Wisconsin Blue Book* (Madison: Wisconsin Legislative Reference Bureau, published biennially).

Table 1. First-Term Legislators[a]

(As a percentage of total membership)

	1893	1897	1901	1905	1909	1913	1917	1921	1925	1929	1933	1937	1941	1945	1949	1953	1957	1961	1965	1969
Connecticut House of Representatives	64	81	82	75	78	75	66	71	68	50	51	52	49	42	46	37	42	36	32[b]	24
Connecticut State Senate	62	75	75	91	74	80	63	83	71	69	60	54	51	56	42	61	58	31	27[b]	17
Michigan House of Representatives	80	72	69	58	68	60	42	42	48	40	62	36	21	39	39	18	19	16	50	14
Michigan State Senate	69	69	78	56	63	66	50	44	34	53	69	50	22	38	38	9	21	15	61	34
Wisconsin Assembly	70	65	64	66	59	70	59	66	42	47	71	27	37	22	36	22	28	33	30	26
Wisconsin State Senate	85	61	73	64	70	73	78	64	73	48	64	61	58	45	45	30	48	24	30	33
Average (Mean)	71	70	73	68	68	70	59	61	56	51	62	46	39	40	41	29	36	25	38	24

[a] Legislators serving first term in each house as indicated, disregarding any prior service in another house.

[b] Based on the 1963 legislature. The 1964 General Assembly election was cancelled by federal court order, on the grounds that the Connecticut legislature had failed to re-apportion itself as directed. Thus the 1965 legislature contained *no* first-term members.

periods, during which the percentage of freshmen in a given house seems to have hovered within a fairly narrow range. This phenomenon is demonstrated by the Connecticut House of Representatives during the periods 1897–1913 and 1929–1941. One might argue that the Wisconsin Assembly has entered a prolonged "plateau" period, perhaps to such an extent that it is an exception to the gradual decline in the number of freshmen which can be observed in the other five houses. From 1937 to 1969, the number of freshmen in the Wisconsin Assembly has fluctuated between 22 and 37 per cent, reaching its low point in 1945 and again in 1953.

(4) While the legislatures of all three states show a general decline in the number of first-term legislators, there are still some clear variations among the states. For example, the number of freshmen dropped below 50 per cent in both houses of the Michigan legislature as early as 1921. This did not happen in Wisconsin until 1929, and not in Connecticut until 1949.

But just what do all these data tell us? The percentage of first-term legislators is obviously intended as a measure of membership turnover, but what does membership turnover tell us? For one thing, a high turnover rate is supposed to be "one of the indicators" of the "amateurism" of state legislatures.[16] As noted earlier, it is argued that members of state legislatures are much less professionalized than members of the U. S. Congress, and are much less committed to their careers as legislators.

While this may be entirely true, can it really be inferred from membership turnover, as measured by the number of first-term legislators? Furthermore, is a high percentage of first-term legislators necessarily evidence of low career commitment? The answer is simple: not necessarily. It *might* be such evidence, but it might also be evidence of something completely different. For example, intense electoral competition could produce a high percentage of freshmen legislators, even though the incumbents were deeply committed to professional legislative careers.

The point is simply that the number of first-term legislators is not a very good measure of career commitment. A better and much more obvious measure would be willingness to seek re-election. The number of legislators who sought re-election in the three states under consideration is presented in Table 2.

According to these figures, all six houses show a substantial increase in the percentage

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[16] Jewell and Patterson, pp. 118–119.

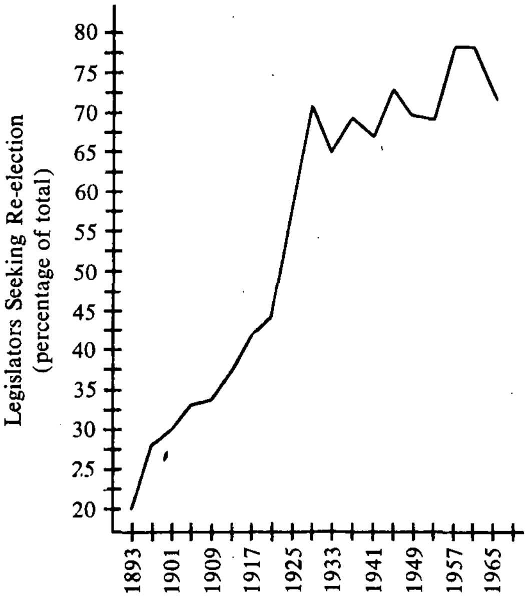

Figure 2. Legislators Seeking Re-election. (Composite average for six legislative houses.)

of legislators who have sought re-election. In 1893, only 9 per cent of the Connecticut House of Representatives sought re-election, a figure which rose to 30 per cent in 1917, 61 per cent in 1937, and 80 per cent in 1969. For all six houses, the increase in legislators seeking re-election is less irregular than the gradual decrease in freshmen legislators reported in Table 1. While there are still some reversals of the overall trend, these temporary reversals are fewer and smaller. (See Figure 2.) Although the two variables (number of freshmen and number of legislators seeking re-election) are surely related, they are either related in a nonlinear way, or—more probably—they have a linear relationship but are also related in complex ways to many other variables. Either possibility would explain why the figures in Table 1 and Table 2 sometimes vary directly and sometimes inversely.

While there has been a general increase in the number of legislators seeking re-election, there have also been clear differences among the states, as with freshman legislators. In both houses of the Michigan legislature, more than 50 per cent of the members were seeking re-election as early as 1909. The Wisconsin legislature did not reach this point until 1925. The Connecticut House of Representatives followed in 1933, but the Connecticut Senate did not reach the 50 per cent mark until 1953.

This type of career commitment (willing-

Table 2. Legislators Seeking Re-Election (As a percentage of total membership)

	1893	1897	1901	1905	1909	1913	1917	1921	1925	1929	1933	1937	1941	1945	1949	1953	1957	1961	1965	1969
Connecticut House of Representatives	9	10	12	14	24	24	30	29	42	48	51	61	59	60	62	65	60	71	46	80
Connecticut State Senate	17	33	13	17	17	20	40	37	46	46	49	43	40	47	39	58	56	75	42	61
Michigan House of Representatives	35	47	44	57	55	56	54	70	83	89	85	81	88	90	82	80	91	90	94	84
Michigan State Senate	28	28	41	47	56	62	50	56	66	78	69	81	78	72	78	75	88	74	90	NYA[a]
Wisconsin Assembly	21	28	34	34	40	33	40	44	53	87	76	86	79	84	90	77	95	90	88	NYA[a]
Wisconsin State Senate	18	27	39	34	15	30	39	31	52	82	67	64	61	88	73	61	78	70	76	NYA[a]
Average (Mean)	21	28	30	33	34	37	42	44	57	71	66	69	67	73	70	69	78	78	72	—

[a] NYA indicates data not yet available.

Table 3. Average (Mean) Prior Service
(In terms per member)[a]

	1893	1897	1901	1905	1909	1913	1917	1921	1925	1929	1933	1937	1941	1945	1949	1953	1957	1961	1965	1969
Connecticut House of Representatives	0.3	0.3	0.3	0.3	0.3	0.4	0.4	0.5	0.5	0.8	0.8	0.9	1.1	1.4	1.2	1.4	1.5	1.4	1.7[b]	2.0
Connecticut State Senate	0.5	0.3	0.3	0.1	0.3	0.3	0.5	0.3	0.4	0.5	0.7	0.9	0.7	0.7	1.0	0.7	0.7	0.9	1.4[b]	2.5
Michigan House of Representatives	0.2	0.3	0.3	0.5	0.5	0.6	0.9	1.1	1.0	1.4	1.0	1.2	1.9	2.0	1.9	2.3	2.4	3.0	1.9	2.6
Michigan State Senate	0.3	0.3	0.3	0.6	0.7	0.5	0.7	0.8	1.1	1.0	0.6	0.7	1.6	1.6	1.8	2.7	2.4	3.4	1.4	1.4
Wisconsin Assembly	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	1.0	1.2	0.6	1.8	1.4	2.1	2.1	2.4	2.3	2.1	2.2	2.5
Wisconsin State Senate	0.2	0.3	0.3	0.5	0.5	0.4	0.3	0.4	0.3	0.6	0.7	0.7	0.8	0.8	1.0	1.3	1.1	1.7	2.1	2.2
Average (Mean)	0.3	0.3	0.3	0.4	0.5	0.5	0.6	0.6	0.7	0.9	0.7	1.0	1.3	1.4	1.5	1.8	1.7	2.0	1.8	2.2

[a] All houses have two-year terms, except the Wisconsin Senate, which serves four-year terms. (Beginning in 1966, the Michigan Senate also serves four-year terms.)

[b] Based on the 1963 legislature. The 1964 General Assembly election was cancelled by federal court order. Thus prior-service data based on the 1965 legislature would be artificially inflated. (It would appear as if 100 per cent of the legislators had sought re-election in 1964, and 100 per cent of them had won.)

ness to seek re-election) is a first requirement of legislative professionalization, but it does not ensure that result. If a legislature is to become increasingly professionalized, it is not enough that incumbents are willing to seek re-election. Obviously, at least some of them must win. To put it simply, a second requirement for legislative professionalism is membership stability. This is so because professionalization is produced (at least in part) by the acquisition of more and more legislative experience by more and more legislators.

But how does one measure membership stability? As noted above, willingness to seek re-election does not necessarily produce low turnover. It has also been noted that inferences about membership stability have usually been based on the number of first-term legislators. While there is some logic in using this measure, it is far from ideal. For one thing, the percentage of freshmen legislators tells us virtually nothing about non-freshmen legislators. They could all be serving their second term in office or their tenth term, and in either case the percentage of freshmen would be the same. A better measure of membership stability would be the average (mean) prior legislative service, expressed in terms per member. Such figures for the legislatures under consideration are presented in Table 3.

For all six houses, the figures show a gradual but substantial increase in average prior legislative service. The trend is most sharply delineated by the Michigan House of Representatives, which had an average prior service of 0.2 terms per member in 1893. This figure rose to 1.1 terms per member by 1925, and 2.6 terms per member by 1969. The composite average (mean) for all six houses was 0.3 terms per member in 1893, rising to 2.2 terms per member in 1969. These composite averages are presented graphically in Figure 3.

As was the case in Tables 1 and 2, the data in Table 3 include a number of temporary reversals to the larger overall trend. Considering the trends portrayed in all three tables (the decrease in freshmen legislators, the increase in legislators seeking re-election, and the increase in average prior service), the third trend has been the most gradual and the least irregular.

It is apparent that the relationships among the three variables are complex and irregular, undoubtedly because numerous other variables are involved. A comparison of Tables 1 and 3 shows that average prior service sometimes varies directly and sometimes inversely with the number of freshmen legislators. The same is true for average prior service and the number

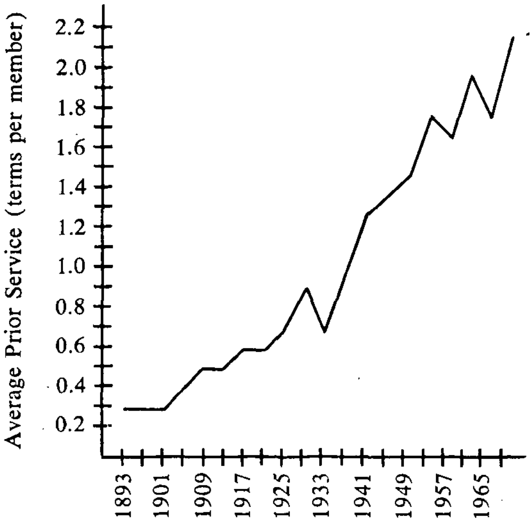

Figure 3. Prior Service of State Legislators. (Composite average for six legislative houses.)

of legislators seeking re-election. In one case, the average prior service of the Wisconsin Assembly remained completely constant from 1893 to 1921, even though the number of freshmen fluctuated between 59 and 70 per cent, and the number of legislators seeking re-election rose from 21 per cent to 44 per cent.

Membership Stability As A Dependent Variable

As noted, the data indicate that membership stability has substantially increased in the three legislatures under consideration. But why has this occurred? One might begin to answer that question by suggesting that membership stability is determined by two "sets" of factors: (1) those which affect an incumbent's willingness to seek re-election, and (2) subsequent factors which affect an incumbent's electoral success.

With regard to the first "set" of factors, an incumbent legislator presumably decides whether or not to seek re-election by weighing the benefits of continued legislative service against its costs. Some of those costs and benefits are readily observable, such as salary, amount of time and work required, and amount of campaigning necessary for re-election. But other costs and benefits are less observable, such as the psychological gratifications derived from legislative service, the other alternatives perceived to be available, and the benefits of legislative service for one's non-legislative business or profession. Personality factors are obviously important in a legislator's calculation of the costs and benefits of re-election. As Barber has demonstrated, legislators with different psychological needs find different levels of satisfaction in the legislative experience.[17]

With regard to the second "set" of factors, there are many elements which may affect the electoral fortunes of incumbent legislators seeking re-election. A list of such factors would include the degree of party competition in the legislator's district, the extent of opposition to the legislator in a primary election (or other nominating process), variations in voting behavior between presidential and off-year elections, and whatever effects there might be from "landslide" presidential elections.

Both "sets" of factors probably contribute to membership stability in state legislatures. If these determinants of membership stability could be defined operationally, one could conceivably collect data on the changes in each determinant over time, as well as the changes in membership stability over time. Using techniques of multivariate statistical analysis, one could then develop and test a multivariate causal model, depicting how the numerous determinants combine to produce a certain level of membership stability.[18] Since the fifty state legislatures provide fifty comparable cases, such a multivariate causal model could be tested and refined to a much greater extent than is usually possible in political science research. Such an undertaking is obviously beyond the scope of the present paper. It is simply noted that the undertaking is feasible, and that it would be a substantial contribution to empirical political theory. Without such a multivariate model, one cannot assess the effect of a single causal variable on membership stability unless the effect of other causal variables is held constant.[19] Unfortunately, historical data rarely provide this idealized form of "experimental control."

Membership Stability As An Independent Variable

For all the above discussion, one must not lose sight of the obvious: The many factors which affect membership stability are important only to the extent that membership stability is important. It is frequently asserted that increased membership stability, by producing more professionalized legislators, will lead to more effective legislative bodies.[20] But while this assertion is widely accepted, it is also completely untested. There has been no effort to document the presumed consequences of membership stability.

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[17] Barber, *The Lawmakers*, pp. 212–258.

[18] Such a procedure is discussed in generic terms in Joseph M. Firestone, "Remarks on Concept Formation: Theory Building and Theory Testing," *Philosophy of Science*, 38 (December, 1971), 570–604.

[19] *Ibid.*

Whether or not such consequences actually occur is an empirical question, provided that one can operationally define "more effective" legislatures. To answer the empirical question, it would be necessary to collect historical data on membership stability in a large number of state legislatures. It would also be necessary to collect comparable data from the same states on various measures of legislative performance. Such measures would represent the operational definition of "more effective" legislatures, and would be of crucial importance. It might be useful to design a new measure especially for this purpose, or to use several existing measures, such as Walker's innovation index,[21] or Grumm's legislative professionalism index.[22] Whatever measure is chosen, it would then be possible to determine its relationship (or lack of relationship) with increasing membership stability. The data requirements are enormous, but only in this way can the presumed merits of legislative professionalization be demonstrated or refuted.

On a more modest scale, it might be useful to turn away from overall measures of legislative performance and turn to more specific consequences of membership stability. It would be feasible, for instance, to examine the possibility that changes in membership stability lead to changes in the rules and norms employed by a legislative body. To take one example, it would be possible to test what might be called the Schattschneider-Price theory, which argues that increasing membership stability and legislative professionalization lead to a system of committee rank determined by committee seniority.[23] The Schattschneider-Price theory is based on the experience of the U.S. House of Representatives and argues that the national realignment of 1896 sharply increased House membership stability. This led to committee stability and gradually to committee seniority.

While the Schattschneider-Price theory was based on the experience of the U.S. House of Representatives, its general validity could be tested with membership stability data drawn from state legislatures. The results of such a test could help students of congressional seniority judge among the Schattschneider-Price theory and alternative explanations, such as that proposed by Polsby, Gallaher, and Rundquist (which traces committee seniority in the U.S. House to the 1910 "revolt" against Speaker Joseph Cannon and the consequent weakening of the prerogatives of the Speaker).[24]

If increased membership stability in state legislatures is in fact leading to pressures for a committee seniority system, the Schattschneider-Price theory will be strengthened. Research on this subject is currently in progress. It is one example of how historical data on membership stability can contribute to current research on legislative behavior. Yet, at present, the bulk of such data remains virtually unexamined.

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[20] The rationale behind this argument is summarized in William J. Keefe and Morris S. Ogul, *The American Legislative Process: Congress and the States* (Englewood Cliffs, N.J.: Prentice-Hall, 1964), pp. 126–127.

[21] Jack Walker, "The Diffusion of Innovations Among the American States," *American Political Science Review*, 63 (September, 1969), 880–899.

[22] John Grumm, "Structural Determinants of Legislative Output," in *Legislatures in Developmental Perspective*, ed. Allan Kornberg and Lloyd Musolf (Durham: Duke University Press, 1970).

[23] H. Douglas Price, "The Congressional Career: Then and Now," in *Congressional Behavior*, ed. Nelson W. Polsby (New York: Random House, 1971), pp. 14–27.

[24] Nelson W. Polsby, Miriam Gallaher, and Barry Spencer Rundquist, "The Growth of the Seniority System in the U. S. House of Representatives," *American Political Science Review*, 63 (September, 1969), 787–807.

The 3/2's Rule in Presidential Campaigning*

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Our main thesis in this article is that the winner-take-all feature of the Electoral College—i.e., that the popular-vote winner in each state wins all the electoral votes of that state—induces candidates to allocate campaign resources roughly in proportion to the 3/2's power of the electoral votes of each state. This creates a peculiar bias in presidential campaigns that makes the largest states the most attractive campaign targets of the candidates, even out of proportion to their size.

We shall establish the 3/2's rule first *logically* by showing how the manner in which presidential candidates *should* allocate their resources among the fifty states in order to maximize their expected *electoral* vote can be derived from a mathematical model. We shall then demonstrate *empirically* that the actual campaign behavior of presidential and vice-presidential candidates *does* in fact well approximate this normative rule in terms of the number of campaign appearances each of the major candidates made in each state in the 1960 through 1972 presidential campaigns.

We shall compare this rule with an allocation rule based on the assumption that a candidate desires to maximize his expected *popular* vote, which would be consistent with the goal of winning if the Electoral College were abolished and the president were elected by direct popular vote. On the basis of the 1970 census and the electoral votes of each state through 1980, we shall indicate a presidential candidate's "optimal" expenditures of resources in all fifty states and the District of Columbia under both the Electoral-College system and the popular-vote alternative. In the case of the Electoral College, the optimum we shall derive will be shown to be unstable: any allocation of resources can be "beaten" under this system. This is not the case for the popular-vote alternative, which, because of the stability of its optimum, would tend to relieve the candidates of the necessity of making some of the manipulative strategic calculations that are endemic to the present system.

We shall conclude our analysis by comparing the ability of the two different systems to translate the attention (in time, money, and other resources) that presidential candidates—and after the election, incumbent presidents looking to the next election—pay to their state constituencies as a function of their size. Our comparison will reveal that the nonegalitarian bias of the Electoral College, which makes a voter living in one state as much as three times more attractive a campaign target as a voter living in another state, would be eliminated if the president were elected by direct popular vote.

The Need for Models to Assess the Consequences of Electoral Reform

Probably the most important reason that the structural reform of major political institutions is so controversial is that reforms often produce shifts in the distribution of power among political actors. When the precise effects of these shifts are uncertain, then confusion tends to beset and compound controversy. As Senator Birch Bayh of Indiana said on the floor of the United States Senate on September 8, 1970 about hearings that had been held on the reform of the Electoral College:

> I must say, sitting through two or three volumes of hearings over the last 4 or 5 years was not at all times an inspirational experience. Some of the testimony was repetitive. Nevertheless, as chairman of the Subcommittee on Constitutional Amendments, I sat there. I thought it amusing, if not ironic, that on the last day—and I am not going to name individuals or organizations—and after 4 years of study, the last two witnesses appeared before our committee. One witness came before our committee suggesting the present [electoral] system should be maintained because it gave an advantage to the larger States and the next witness suggested the present system should be main-

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* This article is a condensation of a paper presented at the 1972 Annual Meeting of the American Political Science Association, Washington, D.C., September 5–9, 1972. The original paper includes, among other things, mathematical derivations and proofs, and a description of a sequential-strategy model, not included in this article. This back-up material has been published in Steven J. Brams and Morton D. Davis, "Resource-Allocation Models in Presidential Campaigning: Implications for Democratic Representation," *Annals of the New York Academy of Science (Proceedings of the Conference on Democratic Representation and Apportionment: Quantitative Methods, Measures and Criteria)*, Vol. 219, ed. Lee F. Papayanopoulos (New York: New York Academy of Science, 1973). We wish to acknowledge the very helpful comments made on an early formulation of the models by John C. Harsanyi, Anatol Rapoport, and Lloyd S. Shapley, and on the original paper by Walter Dean Burnham and Norman Nie, none of whom should be held responsible for the present analysis. We are also grateful to Roman Frydman, Elizabeth Gidengil, and Jeffrey Richelson for valuable research assistance.

tained because it gave an advantage to the small States.[1]

After several thousand pages of testimony before both Senate and House committees and subcommittees in the last few years,[2] there remains today a good deal of confusion and controversy about the possible effects of various proposed changes in the U. S. Constitution—relating to the election of a president—on the creation of new parties and minor candidates, the political influence of small and large states and groups and individuals within these states, governmental stability, and a host of other aspects of the electoral process. This is true despite the plethora of proposals for electoral reform that have been extensively discussed, if not analyzed, in congressional hearings and in numerous books and articles.

This discussion and analysis have in many cases been shallow, however, producing controversy based not on genuine differences of opinion but rather on a confused understanding on the part of different analysts of what consequences will follow from what changes in the electoral system. The main reason for this confusion seems not to stem from any paucity of factual information on national elections. Rather, there has been a lack of rigorous *deductive models* which can be used to explore the logical and quantitative *consequences* of different electoral systems. As we shall try to show, such models not only can aid one in deducing consequences from a limited set of assumptions but also can help one to clarify the kinds of evidence appropriate to testing the empirical validity of these consequences. In this manner, models can be used to lay the groundwork for the construction of better democratic theory.

The consequences that would flow from electoral reform cannot be assessed simply by asking how hypothetical changes in the present system—specifically, in procedures for aggregating popular votes that produce a winner in presidential elections—would have affected previous elections, as has been frequently done in the past. When an alternative procedure would be likely to have changed the campaign strategies of candidates in these elections to produce returns in states different from those that actually occurred, it is evident that the past returns cannot be held constant, with only the hypothetical procedure for aggregating them being allowed to vary, for purposes of estimating what consequences alternative aggregation procedures *would have had* on previous outcomes. As Alexander Bickel has argued, "Any [major] change in the system . . . may induce subtle shifts in electoral strategies, rendering prediction based on past experience hazardous."[3] Curiously, it is just such "hazardous" predictions that Bickel conjures up to support his argument for retention of the Electoral College.

Toward improving this state of affairs, we shall outline and test some mathematical models of the presidential campaign process that will help us to assess the consequences produced by the present electoral system and its most prominent alternative, direct popular-vote election of the president.[4] We have chosen to focus on the direct popular-vote alternative, and not proportional, district, and other plans for electing the president, because it has been the most widely discussed of the proposed alternatives to the Electoral College.[5] With a provision for a run-off election between the two top candidates if neither secures as much as 40 per cent of the popular vote in the initial election, this alternative was approved by the House of Representatives on September 18, 1969 by a vote of 338 to 70, considerably more than the two-thirds majority required for the proposal of constitutional amendments.[6] This plan fared less well in the Senate and eventually became the victim of a filibuster by southern and (strangely, as we shall see) small-state senators; cloture motions on September 17 and 29, 1970 won the approval of a majority of senators but failed to receive the required two-thirds endorsement needed to cut off debate.[7] This plan, nevertheless, has been strongly supported by the American public, receiving 66–19 percent approval (15 per cent undecided) prior to the three-way 1968 presidential election, 81–12 per cent approval (7 per cent undecided) after this election.[8]

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[1] *Congressional Record,* September 8, 1970, p. 30813.

[2] In the Senate, the most recent hearings have been *Electoral College Reform,* Hearings before the Committee on the Judiciary, United States Senate, 91st Congress, Second Session (1970); in the House, *Electoral College Reform,* Hearings before the Committee on the Judiciary, United States House of Representatives, 91st Congress, First Session (1969).

[3] Alexander M. Bickel, *Reform and Continuity: The Electoral College, the Convention, and the Party System* (New York: Harper and Row, 1971), p. 35.

[4] From a critical review of recent literature on campaigning, Gerald Pomper concludes that "in future research, more attention needs to be directed to the effects, rather than the characteristics, of campaigns." See Gerald M. Pomper, "Campaigning: The Art and Science of Politics," *Polity,* 2 (Summer 1970), 533–539, at p. 539. Toward this end, mathematical models of the campaign process are developed in John Ferejohn and Roger Noll, "A Theory of Political Campaigning" (Paper delivered at the 1972 Annual Meeting of the American Political Science Association, Washington, D.C., September 5–9, 1972); and Gerald H. Kramer, "A Decision-Theoretic Analysis of a Problem in Political Campaigning," in *Mathematical Applications in Political Science, II, ed.,* Joseph L. Bernd (Dallas: Southern Methodist University Press, 1966), pp. 137–160. In the Kramer article, a resource-allocation model is used to analyze the effects of different canvassing techniques on turnout and voting from the vantage point of one candidate—and not his opponent(s) directly, whose possible strategies our later game-theoretic models explicitly take into account; for an empirical test of the effects of canvassing in recent elections, see Gerald H. Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," *Public Opinion Quarterly,* 34 (Winter, 1970–71), 560–572. The most explicit treatment of different strategic factors in a campaign, developed from a coalition-theoretic perspective, is John H. Kessel, *The Goldwater Coalition: Republican Strategies in 1964* (Indianapolis: Bobbs-Merrill Co., 1968). A useful compilation of material on techniques of campaign management and communication and their effects on the electorate can be found in Dan Nimmo, *The Political Persuaders: The Techniques of Modern Election Campaigns* (Englewood Cliffs, N.J.: Prentice-Hall, 1970); and for a collection of articles on new campaign methods, see Robert Agranoff, ed., *The New Style in Election Campaigns* (Boston: Holbrook Press, 1972). A study of the use of electoral propaganda in Great Britain, whose practices are compared with those of the United States, is given in Richard Rose, *Influencing Voters: A Study of Campaign Rationality* (London: Faber & Faber, 1967).

Presidential Campaigns and Voting Behavior

If there is anything that has emerged from research on electoral behavior over the last thirty years, it is that most people make up their minds about whom they will vote for in a presidential election well before the onset of the campaign—at least the final campaign between the two major-party nominees that traditionally commences at the beginning of September in a presidential election year. Yet, while the campaign changes few minds, it does serve the important function of reinforcing choices already made, as many studies have documented.

On the other hand, for the typically 20–40 per cent of the electorate who are normally undecided about their choice of a candidate at the start of a presidential campaign,[9] the campaign will not only be decisive to their individual voting decisions but also will often prove decisive to the choice of a candidate by a majority or a plurality of the electorate. This 20–40 per cent minority of the electorate is usually more than sufficient to change the outcome of almost all elections, which is why most campaigns are waged to make only marginal changes in the distribution of voter preferences. Indeed, when a presidential candidate does succeed in capturing as much as 55 per cent or more of the popular vote, his victory is considered a landslide.

If presidential campaigns are decisive principally for the minority of undecided or uncommitted voters who will be crucial to determining the election outcome, then a candidate's ability to project favorably his personality and positions on issues during the campaign assume great importance. Recent research in voting behavior has suggested the importance of issue-oriented aspects of elections first surfaced by V. O. Key, Jr. some years ago.[10] Moreover, since the pioneering work of Anthony Downs the development of spatial models of party competition, which also make the positions that candidates and parties take on issues the focal point of the analysis, has proceeded apace.[11]

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[5] A recent summary of different proposals and a biased assessment (in favor of the present Electoral College) of their likely impact, considered especially in light of the three-way presidential contest in 1968, is given in Wallace S. Sayre and Judith H. Parris, *Voting for President: The Electoral College and the American Political System* (Washington, D.C.: Brookings Institution, 1970). We find it difficult to accept Sayre and Parris's belief (p. 43) that "the political effects of the electoral college system are as clear as any in the nonexact science of American politics," their "nonexact science" qualification notwithstanding. Other summaries that reflect a similar bias in favor of the Electoral College include Bickel, *op. cit.*, pp. 4–36; and Nelson W. Polsby and Aaron Wildavsky, *Presidential Elections: Strategies of American Electoral Politics*, 3d ed. (New York: Charles Scribner's Sons, 1971), pp. 258–271. On the other side, a report by the American Bar Association's Commission on Electoral College Reform has called the popular-vote plan "the most direct and democratic way of electing a President." See American Bar Association (ABA), *Electing the President: A Report of the Commission of Electoral College Reform* (Chicago: ABA, 1967). Also supportive of the direct-vote plan is Neal R. Pierce, *The People's President: The Electoral College in American History and the Direct-Vote Alternative* (New York: Simon and Schuster, 1968); Lawrence D. Longley and Alan G. Braun, *The Politics of Electoral College Reform* (New Haven, Conn.: Yale University Press, 1972); and Harvey Zeidenstein, *Direct Election of the President* (Lexington, Mass.: Lexington Books, 1973). See also John H. Yunker and Lawrence D. Longley, "The Biases of the Electoral College: Who Is Really Advantaged?" and Max S. Power, "Logic and Legitimacy: On Understanding the Electoral College Controversy," both in *Perspectives on Presidential Selection*, ed. Donald R. Matthews (Washington, D.C.: Brookings Institution, 1973).

[6] *Congressional Record*, September 18, 1969, pp. 26007–26008.

[7] *Congressional Quarterly Almanac*, Vol. 26, 91st Congress, 2nd Session (Washington, D.C.: Congressional Quarterly, 1971), p. 840. For further details, see Allan P. Sindler, "Basic Change Aborted: The Failure to Secure Direct Popular Election of the President, 1969–1970" in *Policy and Politics in America: Six Case Studies*, ed. Alan P. Sindler (Boston: Little, Brown, and Co., 1973), pp. 30–80.

[8] Sayre and Parris, p. 15.

[9] William H. Flanigan, *Political Behavior of the American Electorate*, 2d ed. (Boston: Allyn and Bacon, 1972), p. 109.

[10] V. O. Key, Jr., with the assistance of Milton C. Cummings, Jr., *The Responsible Electorate: Rationality in Presidential Voting, 1936–1960* (Cambridge, Mass.: Belknap Press, 1966). For a general discussion of the role of issues in presidential elections, see the articles, comments, and rejoinders by Gerald M. Pomper, Richard W. Boyd, Richard A. Brody, Benjamin I. Page, and John H. Kessel, *American Political Science Review*, 66 (June, 1972), 415–470. The voting decisions of ticket-splitters, in particular, who comprised 54 percent of the American electorate in the 1968 election, are influenced primarily by the candidates and issues, and only secondarily by party identification and other group affiliations; see Walter De Vries and Lance Tarrance,

The campaign models that we shall describe in the sections that follow have much in common with the logical structure, though not the substantive assumptions, of the party-competition models. As in these models, for example, we define optimal strategies to be those strategies that maximize some objective function. Unlike these models, however, we ignore in our models the positions that candidates adopt on issues.

While these positions are central to the determination of optimal strategies in party-competition models, we instead take the positions (and personality) of a presidential candidate as given and ask how he should allocate his total resources among the fifty states and the District of Columbia in order to convey as favorable an image to the voters as possible. An optimal strategy in our models is thus a set of resource allocations to each state rather than a specification of issue positions of candidates. We shall show later how these allocations can be derived from the maximization of different objective functions and discuss the different goals that these functions embody.

Although in some campaigns expenditures beyond a certain point may become counterproductive, we assume that in general a positive correlation exists between the amount of resources that a candidate spends in a state—in relationship to that spent by his opponent—and the favorableness of the image he projects to voters in that state. We further assume that the more favorable a candidate's image, the more likely previously uncommitted voters will vote for him.

Given this connection between campaign spending and voting behavior, the major strategic problem a candidate faces is how best to allocate his total resources among the states to win over that portion of the electorate (i.e., the uncommitted voters) who will prove decisive in most states—without in the process alienating those supporters already predisposed to his candidacy. His problem is rendered even more difficult by the fact that his opponent(s) will tend to allocate his (their) resources in such a way as to exploit any mistakes he might make in his allocations. It is this competitive aspect of presidential campaigns that the mathematics of game theory will prove helpful in illuminating.[12]

We shall not consider here second-order strategic and tactical questions relating to how a candidate should spend his campaign resources *within* each state (e.g., on mass media advertising versus canvassing). Neither will we consider the question of what portion of a candidate's resources should be devoted to *nonstate*-oriented campaign activities (e.g., nationwide TV broadcasting).

The Goals of Candidates

The basic assumption of our analysis is that both voters and candidates are rational individuals who seek to maximize the attainment of certain goals. To be sure, these goals may be the products of sociological, psychological, and other conditioning forces in their lives, but this does not invalidate the assumption that, whatever their goals, candidates and voters seek out the most rational means to achieve them. In the 1964 presidential election, for example, there is strong evidence to support the contention that the Republican nominee, Senator Barry Goldwater, did not so much desire to win as to present voters with "a choice, not an echo." If this is true, then his apparently aberrant campaign behavior, at least as measured against the normal canons of presidential campaigning, may have been quite rational, given his principal goal of espousal of a conservative ideology rather than winning.[13]

Provided one can impute plausible—if not

Jr., *The Ticket-Splitter: A New Force in American Politics* (Grand Rapids, Michigan: William B. Eerdmans Publishing Co., 1972).

[11] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957). For a review of the more recent literature on party-competition models, see William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, N.J.: Prentice-Hall, 1973), chaps. 11 and 12; and Kenneth A. Shepsle, "Theories of Collective Choice," in *Political Science Annual V: An International Review*, ed. Cornelius P. Cotter (Indianapolis: Bobbs-Merrill Co., forthcoming, 1974).

[12] For a related application of a game-theoretical model to the analysis and evaluation of election procedures in the American Political Science Association, see Steven J. Brams, "The APSA and Minority Representation," *PS*, 3 (Summer 1970), 321–335.

[13] As another possible goal, Stanley Kelley, Jr. has suggested that ". . . at least some of the Goldwater inner circle set control of the Republican party—not winning the Presidency—as their principal objective in 1964. That is the implication, certainly, of Senator Goldwater's statement that the conservative cause would be strengthened if he could win as much as 45 percent of the vote." Stanley Kelley, Jr., "The Presidential Campaign," in *The National Election of 1964*, ed. Milton C. Cummings, Jr. (Washington, D.C.: Brookings Institution, 1966), p. 58. Further, there is evidence that Goldwater, who was well aware of his impending defeat from polls commissioned by the Republican National Committee, did little to try to stem its magnitude in the latter half of his campaign (which would be consistent with the goal of winning) but instead tried to rationalize the conduct of his campaign and the anticipated action of the voters. See Stephen C. Shadegg, *What Happened to Goldwater?* (New York: Holt, Rinehart and Winston, 1965), p. 241.

totally realistic—goals to the actors in question, the test of rationality involves determining whether the actors behave *as if* they order their alternative courses of action and choose that which is most preferred, consistent with one or more postulated goals. If a person does not actually make, or is not able to understand, the rationalistic calculations that the theorist imputes to him, the rationality assumption may still satisfactorily account for his behavior by providing a parsimonious explanation from which predictions can be made.

To illustrate, consider the assumption that voters in each state vote for a presidential candidate solely on the basis of how much time (and, in principle, other resources) he spends in each state as compared with that spent by his opponent. Clearly, this assumption is wildly unrealistic for the majority of voters already predisposed to one or the other of the candidates before the start of a campaign.

The assumption is even a radical simplification for the uncommitted voters, whom we argued earlier are usually decisive to the outcome of most presidential elections. Yet this assumption, which we *shall* apply to uncommitted voters in our models, does offer a means for capturing one salient aspect of the campaign—how candidates view the relationship between their expenditures and the potential voting behavior of uncommitted voters in each state—from which we can derive nonobvious prescriptions of how much time the candidates should allocate to each state. Whether in fact this assumption is plausible is an empirical proposition we shall try to test not directly but rather indirectly through corroboration of the consequence deducible from it of how much time the candidates would spend in each state if their goals were to maximize their expected electoral or popular vote.

The goal of maximizing one's expected electoral vote under the present system, and one's expected popular vote under a system allowing for popular-vote election of the president, seem plausible goals to ascribe to most presidential candidates, the case of Senator Goldwater notwithstanding. Both goals, based on probabilistic calculations, incorporate the idea that presidential campaigning is shot full of uncertainties and that there is no surefire campaign strategy that can guarantee victory. For the models we shall develop, the goal of maximizing one's expected popular vote will always be synonymous with maximizing one's probability of winning under the popular-vote system, though under the Electoral-College system the maximization of one's expected electoral vote may under certain circumstances be inconsistent with maximizing one's probability of winning. We shall point out some implications of these different maximization goals for the Electoral College in our later analysis.

The Popular-Vote Model

To motivate the need for models of the campaign process, compare Richard Goodwin's testimony before the Judiciary Committee of the Senate that

> Today, nearly every State has a swing vote which, even though very small, might win that State's electoral vote. Thus, nearly every State is worth some attention. If the focus shifts to numbers alone [under a system of direct popular vote], then the candidate will have to concentrate almost exclusively on the larger States. That is where the people are, and where the most volative [*sic*] vote is to be found. . . . What does this mean? It means that direct election would greatly intensify the attention given to the largest States.[14]

and Senator Bayh's response that

> The record will show that the major party candidates spend considerably more time [under the Electoral-College system] in States that have large blocks of electoral votes. . . . It seems to me you have to be rather naive to overlook the fact that today the whole emphasis of the campaign is in . . . major states.[15]

If these contrary assertions leave one perplexed, what is one to say after several further pages of testimony, interspersed with inconclusive evidence presumably supporting each of these diametrically opposed viewpoints, about which system will force candidates to spend a disproportionately large portion of their time in the largest states? A recognition that the rules of politics are not neutral does not necessarily produce an immediate understanding of what bias they create and what contestants they favor.

To develop the kinds of models that might help to resolve such a question as that discussed above, we shall consider first the popular-vote plan. We shall begin by assuming that the probability that a randomly-selected *uncommitted voter* in state i votes for the Republican candidate is

$$p_i = \frac{r_i}{r_i + d_i},$$

where r_i is the amount (of time, money, or other resources) spent by the Republican candidate in state i and d_i is the amount spent by the Democratic candidate over the course of the campaign. This is the same assumption that we shall make later in the case of our Electoral-College model.

If n_i is the number of uncommitted voters in state i, then to maximize his *expected popular vote* among the uncommitted voters in all fifty states, the Republican candidate should maximize the quantity W_p, which is defined below:[16]

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[14] Senate Hearings on Electoral College Reform (1970), p. 82.

[15] Senate Hearings (1970), pp. 82–83.

$$W_p = \sum_{i=1}^{50} n_i p_i \qquad r_i, d_i, n_i > 0,$$

where

$$\sum_{i=1}^{50} r_i = R, \quad \sum_{i=1}^{50} d_i = D, \quad \text{and} \quad \sum_{i=1}^{50} n_i = N.$$

The term "expected" is used to signify the fact that W_p, the sum of the number of uncommitted voters in each state times the probability of their voting Republican, is not a certain quantity but instead the *average* Republican share of the total uncommitted vote for given allocations r_i and d_i by both candidates in all states.

Given that neither candidate has any information about the allocations made by his opponent(s), it is not difficult to show that the optimal strategy for each candidate consists in allocating funds in proportion to the number of uncommitted voters in each state. That is,

$$r_i = \left(\frac{n_i}{N}\right) R \tag{1}$$

and

$$d_i = \left(\frac{n_i}{N}\right) D \tag{2}$$

for all states i. With these values, the expected number of uncommitted voters that the Republican candidate can pick up from the entire pool of uncommitted voters is

$$W_p = \left(\frac{R}{R + D}\right) N.$$

If $R = D$ (i.e., the total resources of the Republican and Democratic candidates are equal), then

$$W_p = \frac{N}{2}.$$

That is, the two candidates would split the total uncommitted vote.

To this vote, of course, must be added the votes of previously committed Republican and Democratic voters (on whom we assume the campaign has no effect) to get the total number of votes that each candidate receives. We shall show later how these committed voters can be incorporated directly into the resource-allocation calculations. For now, it will be convenient to assume that there are only two candidates (and parties) in each state, though this assumption will later be dropped. Also, we shall assume that the committed voters are split evenly between the parties in each state so that the winner of the uncommitted vote in each state captures a majority of votes in that state.

The allocations given above are "optimal" in the sense that if either candidate adopts the proportionate-allocation strategy, the other cannot gain by deviating from such a strategy. This sense of optimality is game-theoretic in nature: neither candidate has an incentive to depart from this strategy because he might fare worse if he did. Because these strategies minimize a candidate's maximum losses, they are called *minimax* strategies, and the set of such strategies is said to be *in equilibrium.*

Any departure from a minimax strategy by one candidate can be exploited by his opponent. If the Republican candidate, for example, were able to obtain information about deviations by the Democratic candidate from his minimax strategy, he could act on this information (i.e., the nonoptimal allocations of the Democratic candidate) in distributing his own resources so as to capitalize on his opponent's weaknesses. It can be readily demonstrated that, knowing the d_i, the Republican candidate can maximize his expected popular vote among the uncommitted voters by allocating his own resources in the following way:

$$r_i = \frac{\sqrt{n_i d_i}}{\sum_{i=1}^{50} \sqrt{n_i d_i}} (R + D) - d_i. \tag{3}$$

Similarly for the Democratic candidate,

$$d_i = \frac{\sqrt{n_i d_i}}{\sum_{i=1}^{50} \sqrt{n_i r_i}} (R + D) - r_i. \tag{4}$$

If the Democratic candidate pursues a minimax strategy of proportionate allocations according to Eq. (2), then for the Republican candidate Eq. (3)

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[16] This model leads to the same results as one in which the quantity maximized is the *expected plurality* of uncommitted voters:

$$\sum_{i=1}^{50} n_i \left(\frac{r_i - d_i}{r_i + d_i}\right) = \sum_{i=1}^{50} n_i \left(\frac{2r_i}{r_i + d_i} - 1\right)$$
$$= 2\sum_{i=1}^{50} n_i p_i - N.$$

See Richard A. Epstein, *The Theory of Gambling and Statistical Logic* (New York: Academic Press, 1967), pp. 121–123. Note that the summations which follow range only over the fifty states, though in presidential elections beginning in 1964 the District of Columbia must also be included.

Table 1. Allocation of Resources

State i	Number of Voters n_i	Nonoptimal Democratic Allocations d_i	Optimal Republican Allocations r_i	Expected Vote W_{p_i}	Minimax Allocations, r_i and d_i
1	20	100	143	11.8	200
2	30	200	247	16.6	300
3	40	400	404	20.1	400
4	50	600	553	23.4	500
5	50	600	553	23.4	500
Sum	190	1900	1900	95.3	1900

reduces to Eq. (1)—that is, his best response is to allocate his resources proportionately, too, which is the minimax solution in pure strategies for both candidates in this two-person zero-sum infinite game. The game is *zero-sum*, because what uncommitted votes one candidate wins the other candidate necessarily loses, and it is *infinite* because each player has a choice among infinitely many possible expenditure levels in each state. The outcome is a *saddlepoint* when the *pure strategies* (sets of resource allocations to the states) prescribed by Eqs. (1) and (2) are played, for if either candidate plays his prescribed strategy, his opponent can do no better than play his own.

To illustrate how the Republican candidate could do better than his minimax strategy, given nonoptimal allocations by his Democratic opponent, consider the nonoptimal allocations d_i of the Democratic candidate given in Table 1 (which we assume for convenience total ten times the total number of voters). Note that the Democrat's allocations are less than the (proportionate) minimax allocations for the two smallest states (states 1 and 2) and greater than these minimax allocations for the two largest states (states 4 and 5). Assuming that the Republican candidate is privy in advance to these (planned) allocations by his Democratic opponent, his best response according to Eq. (3) is to outspend his opponent in the three smallest states and underspend him in the two largest states, as shown in Table 1.

It can be observed that the Republican candidate, knowing the Democratic candidate's allocations, usually responds with an allocation somewhere between his opponent's allocations and the minimax allocations. Yet, out of 190 votes this best response garners the Republican candidate only 0.6 votes (95.3 for Republican to 94.7 for Democrat) more than his opponent when their total resources are equal, illustrating the relative insensitivity of the popular-vote model to nonoptimal allocations. In sharp contrast, it can be shown that allocations under the Electoral-College system are extremely sensitive in the range where the two candidates about match each other's expenditures in states (i.e., $p_i \simeq 0.50$ for voters in state i).

The Electoral-Vote Model

Under the present Electoral College system, the geographic origin of the vote is salient. Because all the electoral votes of each state—equal to the total number of its Senators plus Representatives—are awarded to the majority (or plurality) winner in that state, such a winner-take-all decision rule is often referred to as "unit rule." In this section we shall outline some of the difficulties connected with the concept and interpretation of "optimal strategy" under this system, and in the next section we shall develop an intriguing, but somewhat fragile, solution to the resource-allocation problem for this system.

For purposes of comparison with the popular-vote model, assume that v_i is the number of electoral votes of state i, where

$$\sum_{i=1}^{50} v_i = V.$$

Then, to maximize his *expected electoral vote* among the uncommitted voters in all fifty states, the Republican candidate should maximize the quantity W_e:

$$W_e = \sum_{i=1}^{50} v_i \pi_i, \tag{5}$$

where, assuming for convenience an even number of voters, n_i,

$$\pi_i = \sum_{k=(n_i/2)+1}^{n_i} \binom{n_i}{k} p_i^k (1 - p_i)^{n_i - k} \tag{6}$$

is the probability that the Republican candidate obtains *more* than 50 per cent of the uncommitted voters in state i—that is, the probability that state i goes Republican if the committed voters are split 50-50. We assume here that the voting of uncommitted voters *within* each state is statistically independent.

Unfortunately, unlike the maximization of W_p in the popular vote model, the maximization of W_e in the electoral-vote model does not yield a closed-form solution for the optimal values of $r_i(d_i)$ when the $d_i(r_i)$ are known. This makes problematic comparisons with the explicit solution to the popular-vote model [Eqs. (3) and (4)].

When the allocations of an opponent are not known, the solution to the optimization problem in general does not take the form of pure strategies in a two-person zero-sum infinite game, where the players are the two candidates. Like the popular-vote model, the contest is a game because it depends on the strategy choice of the other player (i.e., an interdependent decision situation exists). Unlike the popular-vote model, this game has no saddlepoint, which means that the choice by a candidate of an optimal strategy will depend on his opponent's choice. To keep his opponent from discovering his choice, each candidate will leave his choice to chance by choosing from a set of pure strategies at random, with only the probabilities determined. That is, he will use a *mixed strategy*, which is a probability distribution over a set of pure strategies, but we have not been able to establish what the minimax mixed-strategy solution to the game we have described is.[17]

Only under special circumstances does such a game have a pure-strategy solution. We shall not offer any details here except to note that if

$$p_i = \frac{r_i}{r_i + d_i}$$

is *defined* to be the probability that a majority of uncommitted voters (rather than a randomly-selected uncommitted voter) in state i votes Republican—i.e., if $\pi_i = p_i$ instead of the quantity defined in Eq. (6)—then the game will have a solution in pure strategies for both candidates. Analogous to the solution in the popular-vote model (with v_i and V now substituted for n_i and N, respectively), the (pure) minimax strategy of each candidate is to allocate resources in proportion to the number of electoral votes of each state:

$$r_i = \left(\frac{v_i}{V}\right) R; \quad \text{and} \quad d_i = \left(\frac{v_i}{V}\right) D.$$

Because this consequence is derived from the assumption that resource expenditures under the Electoral College system affect directly a majority, and not single individuals, in each state, it is not directly comparable to the proportionate-allocation rule in the popular-vote model, which is derived from the individualistic assumption. While it is hard for us to entertain the belief that aggregates, rather than individuals, respond to the actions of the candidates, we shall nonetheless test the truth of the proportionate-allocation strategy for the Electoral College in our later empirical analysis.

Whatever its exact form, a mixed-strategy solution to the electoral-vote model based on the individualistic assumption will be difficult to interpret, let alone to verify empirically. To exorcise this strategy of its random element, we shall show in the next section how an additional assumption can be made that invests optimal strategies under the Electoral College system with greater determinateness.

The 3/2's Allocation Rule

The assumption we shall make is that the two presidential candidates match each other's resource expenditures in each state. This assumption seems reasonable in light of the fact that the candidates tend to agree on what states (usually large and heterogeneous) are the most attractive campaign targets. Even Senator Goldwater decided to abandon early plans in 1964 to write off the big industrial northeastern states and instead "go shooting where the ducks are." Of the seven states he ended up spending the most time in, which collectively claimed more than half of his time, three (New Jersey, New York, and Pennsylvania) were in the Northeast.[18]

As further support for the matching assumption, the product-moment correlation coefficients between the combined appearances (to be defined later) of the Republican and Democratic presidential and vice-presidential candidates in all fifty states are 0.92 in the 1960 campaign, 0.83 in the 1964 campaign, 0.90 in the 1968 campaign, and 0.74 in the 1972 campaign. Such strong empirical relationships between appearances of the two slates lend support to the proposition that the goal of maximizing one's expected electoral vote that we postulated for the candidates is generally consistent, at least in the four most recent presidential campaigns, with concentrating one's time in the same set of states.

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[17] In Colonel Blotto games—where the candidate who outspends his opponent in a state by whatever amount wins that state with certainty, in contrast to the probabilistic relationship that we assume between expenditures and winnings—a minimax solution in mixed strategies has been found when all states have the same number of electoral votes. See David Sankoff and Koula Mellos, "The Swing Ratio and Game Theory," *American Political Science Review*, 56 (June, 1972), 551–554; and for a discussion of related Colonel Blotto games, see Lawrence Friedman, "Game Theory Models in the Allocation of Advertising Expenditures," *Operations Research*, 6 (Sept.-Oct., 1958), 699–709. For a non-mathematical description of pure and mixed strategies, see Morton D. Davis, *Game Theory: A Nontechnical Introduction* (New York: Basic Books, 1970), pp. 19–48.

[18] Kelley, in Cummings, *The National Election of 1964*, pp. 50–51, 75.

In the calculations of our Electoral College model, we can incorporate this matching assumption by assuming that $r_i = d_i$ (and necessarily $R = D$) for all states i in solving Eq. (5) for a maximum. This assumption enables us to obtain an explicit expression for r_i that maximizes the Republican's expected electoral vote:

$$r_i = \left[\frac{v_i\sqrt{n_i}}{\sum_{i=1}^{50} v_i\sqrt{n_i}}\right] R. \qquad (7)$$

Since we assumed that $d_i = r_i$ (and $D = R$) for all i, the Democrat's optimal resource allocation,

$$d_i = \left[\frac{v_i\sqrt{n_i}}{\sum_{i=1}^{50} v_i\sqrt{n_i}}\right] D, \qquad (8)$$

is the same as his Republican opponent's.

It can be shown that these allocations are in equilibrium: any *arbitrarily small* (but finite) deviation from these strategies on the part of either candidate will be nonoptimal. A *sufficiently large* deviation by one candidate could, however, prove profitable. If, for example, the Republican candidate reallocated all the resources he spends in one state, as prescribed by Eq. (7), to a larger state, he would lose the smaller state with certainty but almost surely win a majority of popular votes (and all the electoral votes) in the larger state. Thus, instead of exactly splitting his expected electoral vote with his opponent—by matching expenditures with him in all states according to Eqs. (7) and (8)—he could, by such a unilateral deviation, easily win a majority of the electoral votes, holding constant his total expenditures.

We cannot easily specify *how small* the deviation must be so as to be nonoptimal for a candidate. As we have shown by the above example, however, the equilibrium is only *local* and not a minimax solution, for *some* unilateral deviation can secure for a candidate a more-preferred outcome (i.e., a higher expected electoral vote). The matching strategies prescribed by Eqs. (7) and (8) are therefore not optimal in a *global* sense.

To be optimal in this sense, the strategies would have to be such that, whenever the candidates spend the same amount in all states, there would be an incentive for both to move toward the particular matching expenditures defined by Eqs. (7) and (8). There is, however, no such incentive. Only when the candidates' expenditures are near or at this point is there an incentive *not to deviate* from these expenditures by a small amount.

Because the allocations defined by Eqs. (7) and (8) constitute only a local equilibrium point, it is unstable.[19] Yet, despite its instability, it is suggestive of a possible reference point in the calculations of candidates that does have an interesting interpretation.

This interpretation can most easily be grasped if we make the simplifying assumption that the electoral votes of state i, v_i, are proportional to the number of uncommitted voters, n_i, in that state. Given this assumption, the terms in the first factor of the numerator, and in the summation of the denominator, of Eqs. (7) and (8), $v_i\sqrt{n_i}$, will be proportional to $v_i\sqrt{v_i} = v_i^{3/2}$ The (local) equilibrium strategies of the Republican and Democratic candidates who match each other's resource expenditures in each state will then be, respectively,

$$r_i = \left[\frac{v_i^{3/2}}{\sum_{i=1}^{50} v_i^{3/2}}\right] R$$

and

$$d_i = \left[\frac{v_i^{3/2}}{\sum_{i=1}^{50} v_i^{3/2}}\right] D.$$

In other words, each candidate should allocate his total resources, which we assumed are the same for the Republican and Democratic candidates ($R = D$) in making the matching assumption ($r_i = d_i$ for all i), in proportion to the 3/2's power of the number of electoral votes of each state.

This is what is meant by the "3/2's rule" in the title of this article. It implies that both candidates, in matching each other's resource expenditures, should not simply allocate on the basis of the electoral votes of each state but rather should allocate decidedly more than proportionately to large states than to small states. For example, if one state has 4 electoral votes, and another state has 16 electoral votes, even though they differ in size only by a factor of four, the candidates should allocate eight times as much in resources to the larger state because

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[19] Whenever we use the term "unstable," we mean "globally unstable," for this point is stable locally (i.e., is impervious to small deviations).

$$\frac{16^{\frac{3}{2}}}{4^{\frac{3}{2}}} = \frac{64}{8} = 8.$$

This allocation rule thus favors large states with more electoral votes, even out of proportion to their size. It is a strikingly simple and nonobvious consequence of the postulated goal that candidates seek to maximize their expected electoral vote, given that they match each other's resource expenditures.[20] While simple in form, however, it is not immediately obvious why large states are so advantaged, apart from the commonsensical observation that voters in large states have greater potential influence over the disposition of large blocs of electoral votes.

Why the Large States Are Favored

To give greater insight into the quantitative dimensions of this "potential influence," it is useful to calculate the "expected minimum number of voters sufficient to change the outcome" in a state. This quantity is simply the sum of the probability of an exactly even split (i.e., a tie, assuming an even number of voters) times 1 (one "swing voter" is minimally sufficient to change the outcome—a deadlock—by changing his vote), the probability of a 2-vote victory by either candidate ($k=+1$ or $k=-1$ below) times 2 (two "swing voters" are minimally sufficient to change the outcome by changing their votes), etc. If we assume for convenience, that there are $2n$ voters (i.e., an even number) in a state, then this expected minimum number, which we shall refer to as the *expected number of decisive voters*, is

$$N(D) = \sum_{k=-n}^{n} \binom{2n}{n+k} p^{n+k}(1-p)^{n-k}(|k|+1),$$

where p is equal to the probability that a randomly selected uncommitted voter in the state votes for one candidate and $(1-p)$ the complementary probability that such an uncommitted voter votes for the other candidate. When $p=(1-p)=\frac{1}{2}$, it can be shown that

$$N(D) = \left(\frac{1}{2}\right)^{2n}\left[n\binom{2n}{n} + 2^{2n}\right].$$

Since

$$\left(\frac{1}{2}\right)^{2n}\binom{2n}{n} \simeq \sqrt{\frac{1}{\pi n}},$$

the central term of the binomial distribution,[21]

$$N(D) \simeq \sqrt{\frac{n}{\pi}} + 1. \tag{9}$$

That is, when the probability is 50–50 that the voters will vote for one candidate or the other—and thereby enhance the likelihood of a close outcome—the expected number of decisive voters in a state varies with the square root of its size, the first term on the right-hand side of Eq. (9).

The number of decisive voters *per voter* in a state, or what might be called the *decisiveness* of an average voter, is found by dividing $N(D)$ by $2n$:

$$D(2n) = \frac{1}{2\sqrt{\pi n}} + \frac{1}{2n}.$$

To illustrate this measure of decisiveness, in a state with $2n=100$ voters,

$$D(100) = 0.040 + 0.010 = 0.050,$$

while in a state with $2n=400$ voters,

$$D(400) = 0.020 + 0.003 = 0.023.$$

Thus, although the larger state has four times as many voters as the smaller state, the decisiveness of an individual voter in the larger state decreases only by a factor of about two. In other words, while an individual voter has a reduced chance of influencing the outcome in a large state because of the greater number of people voting, this reduction is more than offset by the larger number of electoral votes he can potentially influence. Hence, despite the apparent dilution of his vote under a winner-take-all system like the Electoral College, he has on balance greater potential voting power to affect the outcome of a presidential election than a voter in a small state.[22]

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[20] If the total resources of the candidates are not equal (i.e., R ≠ D), it is not difficult to show that a "proportionate matching" of expenditures, wherein the candidates spend the same proportion (or percentage) of their total resources in each state, yields a somewhat more complicated expression for a local maximum.

[21] James S. Coleman has developed a measure of power based on this term, which here gives the probability of an exactly even split of the uncommitted voters in a state. (Note that "π" in the expressions in this section refers to the number 3.14159, not the probability we defined earlier.) See James S. Coleman, "Loss of Power," *American Sociological Review*, 38 (February 1973), 1–17.

[22] Cf. the similar findings in John P. Banzhaf III, "One Man, 3.312 Votes: A Mathematical Analysis of the Electoral College," *Villanova Law Review*, 13 (Winter 1968), 304–332. Banzhaf's analysis of power is similar to that of Coleman, *op. cit.*, but the correctness of Banzhaf's numerical calculations of voting power in the Electoral College has recently been contested in Lawrence D. Longley and John H. Yunker, "The Changing Biases of the Electoral College" (Paper prepared for delivery at the 1973 Annual Meeting of the American Political Science Association, New Orleans, Louisiana, September 4–8, 1973). While Banzhaf bases his analysis on the concept of a "critical vote," which can occur only if there is a 50–50 split, our concept of "decisiveness" takes into account other possible divisions of the vote.

It is precisely this greater potential voting power of voters in large states that makes them more attractive as campaign targets to the candidates. Is it any wonder why, then, the candidates view the large states as more deserving of their attention, even on a per-capita basis, than small states?

This result, though not directly tied to our earlier resource-allocation models, sheds considerable light on the strategic advantage of voters in large states. Yet, because the 3/2's rule that favors large states is only a local equilibrium point when candidates match each other's resource allocations, it is highly vulnerable when the matching assumption is discarded. In fact, it can be shown that when a candidate knows the allocations of his opponent under the Electoral College system, the most devastating strategy that he can generally use against him is to spend nothing in the smallest state and instead use these extra resources (assuming the total resources of both candidates are equal) to outspend his opponent by a slight amount in each of the other states.

In general, adaptive strategies which exploit the commitments of an opponent demand a flexibility in responding to an opponent's allocations and recommitting one's own resources that does not seem to mirror the campaign realities of advance scheduling, purchase of future broadcast time, etc. It is for this reason that the 3/2's rule, despite its instability, may better reflect a fixed, if intuitive and not well understood, point of reference for the major candidates than do allocation strategies directed only to responding to an opponent's commitments. While admitting that most candidates, and especially successful ones, are probably incrementalists in the way they define and quickly respond to issues in a campaign,[23] it nevertheless appears that decisions about resource allocations are inherently less fluid.

Having already described and justified the assumptions and logic that generate the 3/2's rule, and posted warnings about its fragile nature, we turn in the next section to testing its empirical validity. Since by election time the candidates necessarily have made a set of choices that fixes their allocations, we can check these against the 3/2's and other rules without inquiring into what determines each and every choice. In this analysis, we shall refine the 3/2's rule by assuming that the number of uncommitted voters in each state is directly proportional to the population, and not to the number of electoral votes, of that state. Especially in small states, where the two-senator bonus in the Electoral College greatly magnifies their per-capita electoral-vote representation, there is no reason to assume that the proportion of uncommitted voters will be so magnified. It seems far more reasonable to tie estimates of uncommitted voters directly to the population, thus retaining the original form of the 3/2's rule, $v_i\sqrt{n_i}$, where n_i is assumed proportional to the population.[24]

Testing the Models

Political campaigns in the United States have grown enormously expensive in recent years. Herbert E. Alexander, director of the Citizen's Research Foundation, estimated that political spending on all levels during the 1968 campaign ran to $300 million—up 50 per cent from the total cost of campaigns in 1964—and that it cost $100 million to elect a president that year.[25] *The New York Times* estimated that a total of $400 million was spent on political campaigns in 1972,[26] though a final accounting of expenditures at the presidential level at the time of this writing (July 1973) seems likely to be prolonged by the Watergate scandal.

The per-capita costs of presidential campaigns have also soared, with expenditures per potential voter increasing by 71 per cent between 1964 and 1968.[27] Only recently enacted legislation promises some relief from these spiraling costs by limiting the campaign spending of presidential candidates for radio, television, and other media advertising, as well as paid telephone solicitation, to ten cents per voter, with no more than 60 per cent to be spent on broadcast advertising.[28]

Unfortunately, reliable data on the financial expenditures of the presidential candidates in each state are not generally available. Although the Federal Communications Commission has published data on the campaign expenditures of the major political parties for radio and television advertising in recent presidential election years,[29] these figures are not disaggregated for the presidential races. Despite the wealth of data that has been collected on the financial contributions and expenditures of candidates,[30] its completeness and reliability have been hampered by unsystematic reporting and the lack of effective governmental controls on contributions and expenditures. The Federal Election Campaign Act of 1971, which went into effect on April 7, 1972, tightens up reporting procedures that should improve this situation.

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[23] See Karl A. Lamb and Paul A. Smith, *Campaign Decision-Making: The Presidential Election of 1964* (Belmont, Calif.: Wadsworth Publishing Co., 1968).

[24] The population of a state, of course, is not an exact reflection of the proportion of the voting-age population who are registered and actually do vote in a presidential election. Since it is not at all clear whether and how the proportion of uncommitted voters in a state is related to differences in registration and turnout among the states, we have taken the simplest course of using population as a first-approximation estimate of the proportion of voters likely to be uncommitted at the start of a campaign. This assumption can, of course, be modified at a later time.

[25] *The New York Times*, January 31, 1972, p. 48.

[26] *The New York Times*, November 19, 1972, p. 1.

[27] Computed from figures given in Delmer D. Dunn, *Financing Presidential Campaigns* (Washington, D.C.: Brookings Institution, 1972), p. 33.

[28] *The New York Times*, April 9, 1972, p. 44.

[29] Federal Communications Commission (FCC), *Survey of Political Broadcasting: 1960, 1964, 1968* (Washington, D.C.: FCC, April 1961, July 1965, August 1969), which include data on both primary and general election campaigns.

In the absence of reliable state-by-state data on financial expenditures of presidential candidates, we have turned to the one resource which imposes the same implacable restraints on the campaign behavior of all candidates—time. With a finite amount of it to spend in a campaign, the crucial question for a candidate becomes how to apportion it most wisely so as to gain favorable and far-reaching exposure.

In our models we have assumed that the favorableness of a candidate's image in a state will depend on his resource expenditures in that state as compared to those of his opponent. We shall measure expenditures of time in terms of the total number of *campaign appearances* that a candidate makes in a state. Campaign appearances are defined to be events at which a candidate either makes some public address to an audience (whether the address takes the form of a major speech or brief remarks, but excluding news conferences) or participates in some public activity like a parade, motorcade, or fair. To be sure, our counting of all campaign appearances in a state as equivalent ignores important differences among them (e.g., size of audience, extent of news coverage, etc.), but the distribution of both politically "important" and "unimportant" appearances in each state visited probably makes the aggregated data roughly comparable for the purposes of our analysis.

Our data are based on news coverage of the 1960, 1964, 1968, and 1972 presidential campaigns in *The New York Times* from September 1 to the day before Election Day in November in each year, supplemented by such other sources as congressional reports, *Congressional Quarterly Weekly Reports*, and *Facts on File*.[31] While these data on campaign appearances are only as accurate as coverage by the media, they are generally highly correlated with data collected by Stanley Kelley, Jr. on the number of hours spent by the presidential and vice-presidential candidates in each state in the 1960 and 1964 elections, which is based in part on the candidates' own personal schedules.[32] In 1960, the correlation between number of appearances (our data) and number of hours (Kelley's data) for the Republican slate is 0.88, for the Democratic slate, 0.94; in 1964, the comparable figures for the Republican and Democratic slates are 0.92 and 0.70, respectively.

For each party, we have combined the number of appearances made by both the presidential and vice-presidential candidates and used this total as an indicator of the candidates' resource allocations to each state in the four campaigns studied. Combining the appearances of each party's two nominees seemed preferable to singling out only the time expenditures of the presidential candidate, since often the two candidates adopt complementary strategies. For example, in the 1960 race, Lyndon Johnson, the Democratic vice-presidential candidate, was assigned the task of holding the South for the Democrats, and he devoted more than twice as much time to this region as his running mate, John Kennedy, or either of the Republican candidates, Richard Nixon and Henry Cabot Lodge.[33] If we did not add Johnson's campaign appearances to Kennedy's, it would appear that the Democrats did not consider the southern states to be strategically important to their fashioning a victory, which was manifestly not the case.

As an aid to summarizing the Republican and Democratic candidates' time allocations in the last four campaigns, we have categorized the fifty states on the basis of number of Electoral College votes as small (3–9 electoral votes), medium (10–19 electoral votes), and large (more than 20 electoral votes). Comparing the percentage of electoral votes of these three groups of states in 1960, 1964 and 1968 (after the 1960 reapportionment), and 1972 (after the 1970 reapportionment) with the actual percentage allocations of campaign appearances by candidates to these groups given in Table 2, we see that, with the exception of the 1964 campaign, the candidates of both parties made substantially more appearances in the large states, and substantially less in the medium and small states, than would be proportionate to the elec-

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[30] The most comprehensive source of this information for the 1968 election is Herbert E. Alexander, *Financing the 1968 Election* (Lexington, Mass.: D. C. Heath and Co., 1971).

[31] After collecting our data we learned of the data on the itineraries of presidential candidates published in John H. Runyon, Jennefer Verdini, and Sally S. Runyon, eds., *Source Book of American Presidential Campaign and Election Statistics: 1948–1968* (New York: Frederick Ungar Publishing Co., 1971), pp. 139–173. This work contains no data on the itineraries of vice-presidential candidates, however, whose campaign appearances we have combined with the appearances of presidential candidates in the data analyzed below.

[32] We are grateful to Stanley Kelley, Jr. for furnishing us with his data, which he has analyzed in his articles entitled "The Presidential Campaign," in *The Presidential Election and Transition*, ed. Paul T. David (Washington, D.C.: Brookings Institution, 1961), pp. 57–87; and in Cummings, *The National Election of 1964*, pp. 42–81.

[33] Computed from data given in Kelley, in David, *The Presidential Election and Transition*, p. 72.

Table 2. Percentages of Campaign Appearances (CA) and Electoral Votes (EV) in Different-Size States

Year	Slate	CA Allocations and EV Representation for Groups of States					
		Large		Medium		Small	
		CA	EV	CA	EV	CA	EV
1960	Republican	66	38	21	34	13	28
	Democrat	56		25		19	
1964	Republican	45		24		31	
	Democrat	38	39	29	32	32	29
1968	Republican	61		26		12	
	Democrat	62		21		16	
1972	Republican	50	39	24	30	26	30
	Democrat	63		17		20	

toral votes held by these groups of states.

While overspending in the large states provides presumptive evidence for the 3/2's rule, it is useful to check this presumption against the fit of the 3/2's rule to the distribution of campaign appearances in the three groups of states. Allocations for the 3/2's rule comparable to those given in Table 2 for the proportionate rule in 1960, 1964 and 1968, and 1972 are, respectively, 56, 57, and 56 per cent to the large states, 29, 27, and 25 per cent to the medium states, and 15, 15 and 16 per cent to the small states (rounded off to the nearest percentage point).

Comparing this fit with the proportionate rule, we can determine which of the allocation rules is more congruent with the appearance data. In Table 3 we have calculated the distribution of the data on candidates' appearances in large, medium, and small states as a percentage of the allocations prescribed by each rule. The figures greater than 100 per cent represent overexpenditures, under 100 per cent underexpenditures, according to each rule.

We have summarized these data for each group of states and each allocation rule by computing the *average* percentage overexpenditure (or underexpenditure) across the four years. In the case of the largest states, the 3/2's rule far surpasses the proportionate rule with an average correspondence of 97 (versus 141) per cent. It is also better than the proportionate rule in the case of the medium states, achieving an average correspondence of 85 (versus 74) per cent. But in the case of the small states, the 3/2's rule fits the expenditure data less well than the proportionate rule (139 versus 74 per cent correspondence).

One reason the 3/2's rule fares badly for the small states is because of the huge overexpenditures made to these states (more than 200 per cent greater than prescribed by this rule) by both sets of candidates in the 1964 race. Why the candidates' allocations in this campaign differed so dramatically from those in the other campaigns is not at all apparent. We indicated earlier that Senator Goldwater may never have earnestly campaigned to win, which the awesome magnitude of President Johnson's landslide (61 per cent of the popular vote, 90 per cent of the electoral vote)

Table 3. Campaign Appearances as Percentage of Proportionate and 3/2's Allocation Rules

Year	Slate	Percentage Overexpenditures (>100)/Underexpenditures (<100) for Groups of States					
		Large		Medium		Small	
		Prop.	3/2's	Prop.	3/2's	Prop.	3/2's
1960	Republican	172	118	62	71	48	91
	Democrat	148	99	74	86	66	126
1964	Republican	115	79	74	86	118	208
	Democrat	97	67	93	107	111	214
1968	Republican	151	107	83	96	43	82
	Democrat	158	108	67	78	57	109
1972	Republican	127	87	57	64	64	123
	Democrat	161	109	80	91	82	162
Average		141	97	74	85	74	139

suggests may simply not have been in the cards for him no matter how he campaigned. By the same reasoning, President Johnson may not have felt compelled to follow a "normal" optimal strategy—at least not the 3/2's rule. Whatever the peculiarities of this campaign, it is noteworthy that Goldwater's strategy adheres closer to the 3/2's rule than the proportionate rule in large and medium states, while for Johnson the 3/2's rule works as well as the proportionate rule in the medium states.

President Nixon's margin of victory in the 1972 election (61 per cent of the popular vote, 97 per cent of the electoral vote) matched President Johnson's in 1964, but unlike previous campaigns the candidates of each party in 1972 made very different decisions about the proportion of time they would spend on the campaign trail versus other activities. In this campaign, the Democratic slate made a total of 189 campaign appearances, the Republican slate only 54, which would appear to violate our matching assumption (i.e., $r_i = d_i$ for each state i and $R = D$ for all states) that generated the 3/2's rule. However, insofar as campaign appearances can be regarded as a surrogate variable for other forms of attention paid to states, and all forms taken together can be assumed to be the same for each slate, then the $r_i = d_i$ assumption for resources (i.e., attention) expended in states will be met if the *proportionate* (or percentage) allocations of campaign appearances made in each state are the same for both slates, even if the total numbers of appearances are not. The correlation coefficients given earlier relating the number of campaign appearances of the Democratic and Republican slates in all fifty states for each of the four campaigns show that the proportionate allocations for both slates match each other quite closely.

A rather convincing case can be made for the general applicability of the 3/2's rule from the correlation coefficients relating number of campaign appearances to proportionate and 3/2's allocations for all fifty states (see Table 4). The proportionate coefficients exceed the 3/2's coefficients only for the Democratic candidates in the 1964 campaign, though the 3/2's rule correlations in the remaining seven cases are not significantly higher. What we may infer is that the 3/2's rule fits the actual campaign allocations at least as well, and probably slightly better, than the proportionate rule.[34] Because the proportionate and 3/2's allocations are in fact very highly correlated (0.98), however, it is not surprising that the differences in fit of the two rules to the campaign-appearance data are small.

Table 4. Correlation Coefficients Between Campaign Appearances and Proportionate and 3/2's Allocation Rules

Year	Republican Slate		Democratic Slate	
	Prop.	3/2's	Prop.	3/2's
1960	0.91	0.95	0.94	0.96
1964	0.86	0.87	0.84	0.83
1968	0.94	0.94	0.90	0.93
1972	0.79	0.80	0.85	0.86

We wish to emphasize that while the 3/2's rule fits the appearance data somewhat better overall than the proportionate rule does, we do not promulgate it as an immutable law. Like any theoretical consequence of a set of assumptions, its applicability will be limited to those situations that can be reasonably well characterized by these assumptions—particularly the postulated goal of maximizing one's expected electoral vote and the assumption that candidates match each other's campaign expenditures in each state—which are not easy to verify. Further, the instability of the 3/2's rule as an equilibrium point, which makes it vulnerable to only small deviations, may also limit its applicability, especially when candidates resort to adaptive strategies in response to each other's allocations. Finally, another potential source of slippage between the theoretical allocations and the actual campaign behavior of candidates occurs in the reconstruction of campaign itineraries, which is a task fraught with difficulties that certainly contributes to unreliability in the data.

Despite all these warnings, our data do make clear that the candidates generally make disproportionately large expenditures of time in the largest states. While one could always find a better-fitting function than the 3/2's rule for any particular campaign, it would not constitute an explanatory model unless one could derive it from assumptions that are both interpretable and plausible. Fitting a curve to empirical data may help one summarize repeated instances of a phenomenon, but in itself it does not impart a logic to the curve that we consider the hallmark of scientific explanation.

Having shown that the 3/2's rule mirrors quite well actual campaign allocations of time, we shall show in the next section what the allocations prescribed by this rule are for each state from 1972 through 1980, when the next decennial census will be taken and serve as the basis of a reapportionment of electoral votes among the states in the 1984 presidential election. (Our analysis assumes that Maine operates under the Electoral College system, although starting with the 1972 presidential election system it adopted a "district plan," whereby its two electoral votes based on population are decided on a district-by-district basis and its two senatorial electoral votes are decided on a statewide basis.) Our calculations will also reveal the extent to which the 3/2's rule favors the largest states, even on a per-capita basis, despite the two-senator bonus that favors the smallest states.

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[34] We must also note that the 3/2's rule works less well than the proportionate rule (correlation coefficients of 0.84 and 0.87, respectively) in the case of the only major third-party presidential bid since 1948, that by George Wallace's American Independence Party in 1968, which captured 14 per cent of the popular vote and 9 percent of the electoral vote. However, Wallace and his running mate, General Curtis LeMay, did not campaign in nearly one-third of the fifty states, which throws doubt on the substantive significance of correlations based on all fifty states.

Campaign Allocations and Biases through 1980

Based on the 1970 census and the electoral votes of each state through 1980, we have indicated in Table 5 the percentage of time a candidate should spend in each state and the District of Columbia if he allocates his resources proportionately or allocates them according to the 3/2's rule. For example, a candidate's proportionate allocation to California on the basis of its forty-five electoral votes represents about an 8-per cent commitment of his resources, but the 3/2's rule nearly doubles this commitment to 15 per cent. On the other hand, the 3/2's rule would slash a 0.56 per cent commitment to Alaska by nearly a factor of five. According to this rule, then, California should receive about twice as much, and Alaska about one-fifth as much, resources *per electoral vote* as would be commensurate with their respective forty-five and three electoral votes.

This is what we call the *electoral bias* (EB) of the present system, which is simply the ratio of 3/2's allocations to proportionate allocations for all states. These ratios are given in Table 5 and show that the nine largest states, with 52 per cent of the population, are advantaged by the 3/2's rule ($EB > 1.00$), the remaining forty-one and the District of Columbia disadvantaged ($EB < 1.00$).

The *individual bias* (IB) of the Electoral College is the concept most relevant to assessing the degree to which the 3/2's rule engenders campaign allocations in states that are inconsistent with the egalitarian principle of "one man, one vote." This bias is the ratio of each state's 3/2's percentage allocation to its percentage share of the total population.

In substantive terms, IB represents the relative proportion of resources that a candidate should commit *per person* to each state, given that he makes his allocations according to the 3/2's rule. These per-capita allocation ratios are given in Table 5, along with the ranks (in parentheses) of these ratios from the highest to the lowest for the fifty states and the District of Columbia.

The ranking of states on the basis of their IB values, unlike their ranking on EB values, does not correspond perfectly to their ranking in terms of electoral votes. Thus, for example, while an individual voter in the largest state, California, ranks as the most attractive target for a candidate who allocates according to the 3/2's rule—receiving 50 per cent more attention ($IB = 1.50$) than he would get if the candidate allocated his time strictly according to the population of each state—a voter in the smallest state, Alaska, with an IB of 0.81, is still comparatively well off based on the sixteenth position of Alaska among the fifty states and the District of Columbia. His IB is below the proportionate norm of 1.00 but far from the bottom rung of the ladder that citizens who live in Washington, D. C. have the dubious distinction of occupying with IB's equal to 0.51.

There are two reasons why the IB scores of states are only an imperfect reflection of their electoral votes. First, when two states have the same number of electoral votes, like Texas and Illinois with twenty-six each, citizens of the larger state (i.e., Texas) will be slightly disadvantaged, because the attention that they receive from the candidates according to the 3/2's rule must be divided among more people.

The two-senator bonus accorded to all states more seriously upsets the generally positive relationship between IB scores and the electoral votes of a state. This bonus, naturally, is much more significant to a state like Alaska with one representative than a state like California with forty-three representatives: percentagewise, the two-senator bonus inflates Alaska's per-capita representation by 200 per cent, California's by only about 5 per cent.

Some critics of the Electoral College have charged that this bonus favors small states, which it obviously does on a proportional basis. On the other hand, proponents of the Electoral College have responded that this favoritism is counteracted by the fact that the large blocs of votes cast by large states in the Electoral College have a greater chance of being decisive, especially in close elections.[35] To what extent do these opposing forces cancel each other out?

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[35] In the context of voting in the Electoral College, Mann and Shapley demonstrated that the Shapley-Shubik index of pivotalness for each state produces only a slight bias in favor of the largest states. See Irwin Mann and L. S. Shapley, "The A Priori Voting Strength of the Electoral College," in *Game Theory and Related Approaches to Social Behavior,* ed. Martin Shubik (New York: John Wiley & Sons, 1964), pp. 151–164. For an elementary discussion of the Shapley value on which the Shapley-Shubik index is based, see Davis,

Table 5. Electoral and Individual Biases of 3/2's Campaign Allocations in Fifty States and District of Columbia

State	Electoral Votes	Proportionate Allocation (%)	3/2's Allocation (%)	Electoral Bias (EB): 3/2's / Proportionate	Individual Bias (IB): 3/2's / Population
1. Calif.	45	8.36	14.69	1.76	1.50 (1)
2. N.Y.	41	7.62	12.78	1.68	1.43 (2)
3. Pa.	27	5.02	6.78	1.35	1.17 (3)
4. Texas	26	4.83	6.36	1.32	1.15 (5)
5. Ill.	26	4.83	6.34	1.31	1.16 (4)
6. Ohio	25	4.65	5.96	1.28	1.14 (6)
7. Mich.	21	3.90	4.57	1.17	1.05 (7)
Lge. States	211	39.22	57.48	1.47	1.27
8. N.J.	17	3.16	3.33	1.05	0.94 (9)
9. Fla.	17	3.16	3.24	1.02	0.97 (8)
10. Mass.	14	2.60	2.44	0.94	0.87 (10)
11. Ind.	13	2.42	2.17	0.90	0.85 (12)
12. N.C.	13	2.42	2.14	0.89	0.86 (11)
13. Mo.	12	2.23	1.90	0.85	0.82 (15)
14. Va.	12	2.23	1.89	0.85	0.83 (14)
15. Ga.	12	2.23	1.88	0.84	0.83 (13)
16. Wis.	11	2.04	1.69	0.83	0.78 (18)
17. Tenn.	10	1.86	1.45	0.78	0.75 (22)
18. Md.	10	1.86	1.45	0.78	0.75 (21)
19. Minn.	10	1.86	1.43	0.77	0.76 (20)
20. La.	10	1.86	1.40	0.75	0.78 (17)
Med. States	161	29.93	26.41	0.88	0.84 (31.28)
21. Ala.	9	1.67	1.22	0.73	0.72 (28)
22. Wash.	9	1.67	1.21	0.73	0.72 (27)
23. Ky.	9	1.67	1.18	0.71	0.75 (23)
24. Conn.	8	1.49	1.02	0.68	0.68 (36)
25. Iowa	8	1.49	0.98	0.66	0.71 (30)
26. S.C.	8	1.49	0.94	0.63	0.74 (25)
27. Okla.	8	1.49	0.94	0.63	0.74 (24)
28. Kans.	7	1.30	0.77	0.59	0.69 (34)
29. Miss.	7	1.30	0.76	0.59	0.70 (33)
30. Colo.	7	1.30	0.76	0.58	0.70 (32)
31. Ore.	6	1.12	0.63	0.57	0.62 (43)
32. Ark.	6	1.12	0.61	0.55	0.64 (41)
33. Ariz.	6	1.12	0.58	0.52	0.67 (39)
34. W.Va.	6	1.12	0.58	0.52	0.67 (38)
35. Neb.	5	0.93	0.45	0.48	0.61 (44)
36. Utah	4	0.74	0.30	0.40	0.58 (49)
37. N.M.	4	0.74	0.29	0.40	0.59 (48)
38. Me.	4	0.74	0.29	0.39	0.60 (47)
39. R.I.	4	0.74	0.29	0.38	0.61 (45)
40. Haw.	4	0.74	0.26	0.35	0.68 (37)
41. N.H.	4	0.74	0.25	0.34	0.69 (35)
42. Idaho	4	0.74	0.25	0.33	0.70 (31)
43. Mont.	4	0.74	0.24	0.33	0.71 (29)
44. S.D.	4	0.74	0.24	0.32	0.73 (26)
45. D.C.	3	0.56	0.19	0.34	0.51 (51)
46. N.D.	3	0.56	0.17	0.31	0.57 (50)
47. Del.	3	0.56	0.16	0.29	0.60 (46)
48. Nev.	3	0.56	0.15	0.28	0.64 (42)
49. Vt.	3	0.56	0.15	0.26	0.67 (40)
50. Wyo.	3	0.56	0.13	0.23	0.77 (19)
51. Alas.	3	0.56	0.12	0.22	0.81 (16)
Sm. States	166	30.86	16.11	0.52	0.68

The balance between these forces, it turns out, is very one-sided: the large-state bias created by the 3/2's rule swamps the small-state bias resulting from the two-senator bonus, giving citizens of the most populous fifteen states the highest IB scores. This is not unexpected, since the 3/2's rule compels the candidates to make inordinately large expenditures of resources in the largest states that are well out of proportion to their populations.

Yet not all the citizens of even these fifteen large states are favored by the 3/2's rule. Only the citizens of the seven very largest states, which comprise less than a majority (45 per cent) of the population, receive representation greater than in proportion to their numbers (IB > 1.00). Citizens in the remaining forty-three states, plus the District of Columbia (whose sad plight will be recounted below), receive attention below the one-man, one-vote standard from candidates who adhere to the 3/2's allocation rule.

Only with Alaska's entry into the sixteenth position of the IB ranking does the two-senator bonus begin to help the very small states. While Wyoming breaks into the IB ranking at the nineteenth position, none of the four other very small states with three electoral votes is successful in outdistancing the 3/2's rule by an amount sufficient to rank it above the fortieth position. In the case of Alaska and Wyoming, the two-senator bonus gives each a per-capita electoral vote representation more than four times greater than that of California, which accounts for their ability to overcome to some degree the large-state bias of the 3/2's rule.

Although the states that the two-senator bonus helps the most are among the smallest, small size alone is not the only factor that tends to counteract the 3/2's rule. The ability of a state just to meet the "quota" for a certain number of electoral votes is also important, for it gives the state higher per-capita representation than that of larger states with the same number of electoral votes. Thus, the three smallest states with four electoral votes—Idaho, Montana, and South Dakota—succeed in raising their population rankings of forty-second, forty-third, and forty-fourth by eleven, fourteen, and seventeen notches (one more notch if the District of Columbia is included), respectively, on the IB scale. No other states besides these and Alaska and Wyoming are able to better their population rankings by as much as ten positions.

From the perspective of per-capita representation, the most unfortunate citizens are those who live in Washington, D. C. The Twenty-third Amendment limits the District of Columbia to electoral representation no greater than the least populous state (Alaska), which has three electoral votes. This limitation is strangely inconsistent with the fact that the District's population is greater than that of four states that each have four electoral votes. With an IB of 0.51, its citizens rank below those of all fifty states in per-capita representation.

Using Washington, D. C. as a basis for comparison, we have ranked in Table 6 the ratios of the IB's of all states to the District's IB. Similarly, taking the lowest-ranking state on the EB scale (Alaska), we have given the ratios of all states' EB scores to that of Alaska. These comparisons reveal that the largest state, California, has 2.92 times as great individual representation as Washington, D.C., and 8.13 times as great electoral representation as Alaska. This means that a candidate who campaigns according to the 3/2's rule should allocate to California more than eight times as many resources per electoral vote as he does to Alaska, and almost three times as many resources per person as he does to Washington, D.C. Though Washington, D.C. has 1.76 times as many electoral votes per person as California, which is why its individual bias of 0.51 is greater than (i.e., deviates less from the standard of 1.00) than its electoral bias of 0.34, it would still need about twice as many electoral votes as it has (i.e., six), holding all other states constant, to wipe out its individual bias (i.e., make IB $\simeq$ 1.00).

To be sure, Washington, D.C. is the nation's capital, and its residents probably do not suffer from any lack of exposure to presidential candidates. Yet for Alaska, whose electoral votes have only 12 per cent of the drawing power of California's (EB of 0.22 for Alaska versus 1.76 for California), it may be more than distance that has kept away all presidential and vice-presidential candidates except Richard Nixon in his 1960 campaign. And for the citizens who live in the small and medium states that together comprise 55 per cent of the population of the United States, the average IB and EB scores given in Table 5 for these groups of states indicate that they are gen-

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Game Theory, pp. 172–176. We find the Mann-Shapley result interesting but of limited substantive significance since the real contest for the presidency does not occur *within* the Electoral College, which is largely a ratifying institution. Rather, it is the effects of unit rule *outside* the Electoral College (e.g., on presidential campaigning, the two-party system, etc.) that make this institution loom so large; it is precisely some of these effects that we have tried to capture in our models. For an attempt to explain the allocation data reported in this article using alternative models, see Claude S. Calantoni, Terrence J. Levesque, and Peter C. Ordeshook, "Campaign Resource Allocations under the Electoral College" (Paper, Carnegie-Mellon University, May, 1973). For a generalization of the Shapley value and its application to the Electoral College, see Guillermo Owen, "Multilinear Extensions of Games," *Management Science*, 18 (Jan., Part 2, 1972), pp. P-64—P-79.

Table 6. Rankings of EB and IB Ratios for Fifty States and District of Columbia

Rank	EB Ratio: EB (State *i*)	IB Ratio: IB (State *i*)	Rank	EB Ratio: EB (State *i*)	IB Ratio: IB (State *i*)
	EB (Alaska)	IB (D.C.)		EB (Alaska)	IB (D.C.)
1	Calif. (8.13)	Calif. (2.92)	27	Okla. (2.91)	Wash. (1.41)
2	N.Y. (7.76)	N.Y. (2.79)	28	Kans. (2.73)	Ala. (1.41)
3	Pa. (6.25)	Pa. (2.28)	29	Miss. (2.71)	Mont. (1.39)
4	Texas (6.09)	Ill. (2.26)	30	Colo. (2.70)	Iowa (1.38)
5	Ill. (6.07)	Texas (2.25)	31	Ore (2.63)	Idaho (1.37)
6	Ohio (5.94)	Ohio (2.22)	32	Ark. (2.52)	Colo. (1.37)
7	Mich. (5.42)	Mich. (2.04)	33	Ariz. (2.42)	Miss. (1.36)
8	N.J. (4.87)	Fla. (1.89)	34	W.Va. (2.40)	Kans. (1.35)
9	Fla. (4.74)	N.J. (1.84)	35	Neb. (2.22)	N.H. (1.35)
10	Mass. (4.34)	Mass. (1.70)	36	Utah (1.87)	Conn. (1.33)
11	Ind. (4.15)	N.C. (1.67)	37	N.M. (1.83)	Haw. (1.32)
12	N.C. (4.10)	Ind. (1.65)	38	Me. (1.81)	W.Va. (1.32)
13	Mo. (3.94)	Ga. (1.62)	39	R.I. (1.77)	Ariz. (1.31)
14	Va. (3.92)	Va. (1.61)	40	Haw. (1.60)	Vt. (1.30)
15	Ga. (3.90)	Mo. (1.61)	41	D.C. (1.58)	Ark. (1.25)
16	Wis. (3.82)	Alas. (1.58)	42	N.H. (1.56)	Nev. (1.24)
17	Tenn. (3.60)	La. (1.52)	43	Idaho (1.54)	Ore. (1.20)
18	Md. (3.60)	Wis. (1.52)	44	Mont. (1.52)	Neb. (1.19)
19	Minn. (3.55)	Wyo. (1.51)	45	S.D. (1.49)	R.I. (1.19)
20	La. (3.47)	Minn. (1.49)	46	N.D. (1.43)	Del. (1.18)
21	Ala. (3.38)	Md. (1.46)	47	Del. (1.35)	Me. (1.16)
22	Wash. (3.36)	Tenn. (1.46)	48	Nev. (1.27)	N.M. (1.15)
23	Ky. (3.26)	Ky. (1.45)	49	Vt. (1.21)	Utah (1.13)
24	Conn. (3.17)	Okla. (1.45)	50	Wyo. (1.05)	N.D. (1.11)
25	Iowa (3.06)	S.C. (1.44)	51	Alas. (1.00)	D.C. (1.00)
26	S.C. (2.93)	S.D. (1.42)			

erally disadvantaged, even if the inequities of the electoral system visited upon them do not match the injustice done to the citizens of Alaska (by the EB measure) and the District of Columbia (by the IB measure).

So far we have shown that the 3/2's rule greatly favors the large states and generally overwhelms what per-capita electoral-vote advantage the small states do have. Of course, the relative neglect of some states by presidential candidates is not only a function of their electoral representation but also the degree to which candidates estimate that their campaigning or other expenditures of resources can make a difference in the probable outcome of the election. If no feasible allocations of time, money, or other resources can possibly change the probable outcome, then a candidate would be foolish to give a state anything more than token attention. Richard Nixon, who placed great stress on the symbolic value of visiting all fifty states in his 1960 presidential bid, despite an injury at the beginning of the campaign that kept him sidelined for several days, dumped this strategy in his 1968 and 1972 campaigns. There is no evidence that such "tokenism" brings any payoff in extra votes.

In an actual campaign a candidate and his advisors would normally pare down the fifty states and the District of Columbia (for which 3/2's allocations are given in Table 5) to a smaller list of states in which uncommitted voters are likely to have a decisive impact on the election outcome. The states that a candidate should ignore would include not only those in which his opponent is heavily favored but also those in which he has piled up big early leads prior to the campaign, for it makes as little sense to expend resources in states that seem invulnerable to one's opponent as it does to conduct forays into states that an opponent has virtually locked up. Candidates and their advisors seem to make precisely these calculations,[36] which V. O. Key, Jr. reports also characterize the effects of unit rule in Georgia where, in party nominations for statewide races, the candidate who wins a plurality in each of Georgia's 159 counties receives all the unit votes of that county

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[36] The Democrats reportedly wrote off twenty-four states in 1968, though our data indicate that one or both of the Democratic candidates made appearances in more than half of these states. See Joseph Napolitan, *The Election Game and How to Win It* (Garden City, N.Y.: Doubleday and Co., 1970), p. 62.

(one, two, or three, depending on its population):

> . . . [a practical politician] classifies the counties into three groups: those in which he is sure of a plurality; those in which he has no chance of a plurality; those which are doubtful. He forgets about the first two groups except for routine campaign coverage. He concentrates his resources in the third group: expenditures, appearances by the candidate, negotiations, all the tricks of county politicking.[37]

The widespread use of public opinion polls today makes it easier than ever before to weed out those states "securely" in the camp of one candidate and pinpoint the "toss-up" states likely to swing either way. Insofar as polls indicate the largest states to be the toss-up states, candidates who act on this information and concentrate almost all their resources in these states will magnify even the large-state bias of the 3/2's rule. If we eliminate the anomalous allocations made by the candidates of both parties in 1964, our data in Table 5 reveal that in four of the six remaining cases the candidates' actual allocations did in fact exceed the 3/2's allocations in the large states.

Limitations and Extensions of the Models

The rationale of the 3/2's rule rests on the idea that states can be pinpointed like military targets and, independent of nearby and faraway other targets, captured with a high probability by a concentration of forces superior to that of one's opponent (given that his allocations are known or can be estimated). As Richard M. Scammon and Ben J. Wattenberg warn, however, this logic may be seriously flawed:

> It is extremely difficult, and probably impossible, to move 32,000 votes in a New Jersey Presidential election without moving thousands and tens of thousands of votes in each of the other forty-nine states. The day of the pinpoint sectional or statewide campaign is gone—if it ever existed—and the fact that votes cannot be garnered in bushels on specific street corners is of crucial significance when one looks at the arithmetic of the future.[38]

Scammon and Wattenberg go on to point out that political rallies usually draw mostly partisans already committed to a candidate. For this reason, relatively few uncommitted voters are likely to observe, much less be persuaded by, the speech a candidate makes at a rally in their state.[39] Their choices, Scammon and Wattenberg argue, will depend much more on national coverage of the campaign, particularly by television.[40]

> Yet, these analysts do not dismiss campaigns as insignificant: . . . The campaign is important in providing the candidate with *something to do* that can be televised, photographed, and written about for national consumption . . . . Further, if a candidate does *not* campaign at all, his opposition will criticize him for "not taking his case to the people."
>
> Beyond all that, however, is a certain extremely valuable democratic symbolism that underlies all the flesh pressing, baby kissing, hurly-burly of a campaign. There is great value in a system that somehow demands that a candidate get sweaty and dirty and exhausted, his hands bleeding, his hair messed by the masses of people whom he wants to represent. The successful candidate in America must *touch* the people, figuratively and literally.[41]

How a candidate does this is not well understood, even if we do know that campaigns often decide the outcomes of elections.[42]

Our interest in this analysis, however, is not in showing how the conduct of campaigns affects election outcomes.[43] Rather, it is to establish that candidates act *as if* they believe their conduct matters when it comes to making campaign allocations. Perhaps "the spectacle of seeing one's opponent run around the country at a furious pace without following suit is too nerve-racking to contemplate,"[44] but it also appears that the as-if assumption is rooted in the actual beliefs of candidates, who, at least at the state and local level, think that their campaigns have an impact on the outcome.[45] As evidence for our postulated goal that presidential candidates campaign as if they seek to maximize their expected electoral vote, we have already shown that the 3/2's rule that follows from this goal reflects quite well the actual campaign allocations of candidates.

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[37] V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), p. 122. We are indebted for this citation to Melvin J. Hinich and Peter C. Ordeshook, "The Electoral College: A Spatial Analysis" (Paper, Carnegie-Mellon University, February, 1973), in which the effect of the Electoral College on equilibrium strategies, given different distributions of policy preferences of the electorate, are examined.

[38] Richard M. Scammon and Ben J. Wattenberg, *The Real Majority: An Extraordinary Examination of the American Electorate* (New York: Coward-McCann, 1970), p. 213.

[39] According to a series of Gallup polls, less than 6 per cent of all people saw any one of the presidential candidates in person in 1968. See Sara Davidson, "Advancing Backward with Muskie," *Harper's*, June 1972, p. 61.

[40] Scammon and Wattenberg, *The Real Majority*, pp. 214–216. Moreover, television news shows, documentaries, and other specials, which are generally beyond the direct control of candidates, rank far above television advertising as the most important media influences, at least for the split-ticket voter. See De Vries and Tarrance, *The Ticket-Splitter*, p. 78.

[41] Scammon and Wattenberg, *The Real Majority*, p. 217. Italics in original.

[42] At the local level, party organization activities do appear to influence election results favorably, including the vote for president. See William J. Crotty, "Party Effort and Its Impact on the Vote," *American Political Science Review*, 65 (June 1971), 439–450, and references cited therein; also, Robert J. Huckshorn and Robert C. Spencer, in *The Politics of Defeat: Campaigning for Congress* (University of Massachusetts Press, 1971), stress the importance of campaign organization on election results. At the national level, on the other hand, there is no clear-cut relationship between the campaign spending of each party and the outcome of presidential elections. See Twentieth Century Fund, *Voters' Time: Report of the Twentieth Century Fund Commission on Campaign Costs in the Electronic Era* (New York: Twentieth Century Fund, 1969), pp. 11–13; Congressional Quarterly, *Dollar Politics: The Issue of Campaign Spending* (Washington, D.C.: Congressional Quarterly, 1971), pp. 14, 19–31; and Dunn, *Financing Presidential Campaigns*, p. 9. More opaque, still, is the relationship between major strategic decisions made by candidates, especially during the heat of a close race, and their effect on the vote. See Polsby and Wildavsky, *Presidential Elections*, pp. 199–206.

The increasing nationalization of presidential campaigns obviously limits the applicability of any resource-allocation model that makes states the units of analysis. It is worth noting, though, that in postulating that candidates maximize their expected electoral vote, we do *not* assume that voting by individuals in *different* states is statistically independent. If we had postulated for candidates the goal of maximizing their probability of winning a majority of electoral votes, the statistical-independence assumption would be convenient, and probably necessary, to obtain tractable results.

The goal of maximizing one's expected electoral vote may not be equivalent to maximizing one's probability of winning the election. To illustrate, a candidate who concentrates his resources in states with a bare majority of electoral votes may win these states, and the election, with a high probability, but his expected number of electoral votes will be relatively small. On the other hand, if he spreads his resources somewhat thinner over more states, he may increase his expected number of electoral votes, but at the price of lowering the probability that he will win a majority of electoral votes. Thus, the two goals may be logically inconsistent—that is, they may lead to contradictory strategic choices on the part of rational candidates. We do not think that the implications of these different goals will be seriously contradictory in most cases, at least in two-candidate races, but this question needs to be explored further.

Finally, we wish to point out that several restrictive assumptions we made earlier in the development of our models can be relaxed. The minimax solution of the popular-vote model, and the (unstable) equilibrium solution of the electoral-vote model, hold *mutatis mutandis* when three or more candidates compete in an election. Further, it is not necessary to assume, as we did initially, that the committed voters in each state are evenly divided at the outset of a campaign. If a candidate's polls indicate, for example, that he is ahead by particular margins in some states, and behind by particular margins in others, he can use this information to determine his optimal allocations to each state, given that he can estimate the probable allocations of his opponent.

This problem can be conceptualized as one in which a candidate tries to reduce the effect of an opponent's supporters in a state by matching them against his own. If at the start of a campaign one candidate has more supporters in a state than the other, the lesser candidate faces the problem of winning over a sufficient number of uncommitted voters to neutralize both his opponent's extra supporters and those uncommitted voters his opponent is likely to pick up, given that commitments once made are not broken.

Given the allocations of the candidates for all states i that define p_i, and the distribution of supporters committed to each candidate in each state, the probability of neutralizing an opponent's support and then going on to win is given by Lanchester's Linear Law, for which Richard H. Brown has provided a useful approximate solution.[46] Optimal allocations can be obtained by maximizing the expected electoral vote based on these probabilities for all states. Without having made these kinds of calculations, we would suspect that, holding size constant, the most attractive targets by far for campaign allocations in the electoral-vote model will be those states in which the committed voters are split roughly 50–50.

Finally, it seems possible that the models we have developed may be applicable to the analysis of resource-allocation decisions in other political arenas, particularly where the distribution of payoffs to members of a coalition is involved.[47] For example, the apportionment of foreign aid, the commitment of military forces, the granting of patronage and other favors, and even the assignment of priorities all involve decisions about how to allocate resources among actors (or programs) of different weight or importance. Moreover, these decisions are often influenced by the actions or probable responses of an opponent in an essentially zero-sum environment. Presumably, the extent to which decision making in such competitive situations is conditioned by either of the goals we have postulated could be determined by comparing the resource expenditures actually made with the theoretical implications these goals entail (i.e., the proportionate or 3/2's rules).

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[43] That the organization and planning of presidential campaigns is critical to a candidate's eventual success is stressed in Jerry Bruno and Jeff Greenfield, *The Advance Man* (New York: William Morrow & Co., 1971).

[44] Polsby and Wildavsky, *Presidential Elections*, p. 183. If the steeliness of a candidate's nerves can be judged by this standard, Richard Nixon's nerves progressively hardened in his successive bids for the presidency: as an incumbent president in the 1972 campaign, he made only 18 campaign appearances, while as a nonincumbent in 1968 and 1960 he made 140 and 228 appearances, respectively. Yet, though he cut down his number of appearances in successive campaigns, in all three races Nixon made disproportionately greater allocations to the large states than to the medium and small states, as did the entire Republican state in each of these campaigns (see Table 3).

[45] John W. Kingdon, *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1968), pp. 109–114. See also Robert A. Schoenberger, "Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-Making in the 1964 Congressional Elections," *American Political Science Review*, 63 (June, 1969), 515–520.

[46] Richard H. Brown, "Theory of Combat: The Probability of Winning," *Operations Research*, 11 (May-June 1963), 418–425.

Summary and Conclusion

We began our analysis on a critical note by contending that many of the arguments for and against the Electoral College suffer from a primarily descriptivist and nontheoretical orientation. They have been responsible for more obfuscation than clarification of the possible consequences that would flow from the abolition of the Electoral College and the substitution of direct popular-vote election of the president in its place.

As a way of making the comparison of alternative electoral systems more rigorous, we assumed at the outset that different electoral systems encourage political actors to seek different goals. By deducing the theoretical consequences that follow from the rational pursuit of these goals, we suggested that the effects of different electoral arrangements—specifically, different vote aggregation procedures—on political behavior could be illuminated.

This approach to the analysis of political behavior in general, and electoral behavior in particular, is still unorthodox in political science, though the recent development of spatial models of party competition provides one example of the power of this approach. Unlike party-competition models, however, which take the positions of candidates on issues as their focus for analysis, we took these positions as given and instead asked how a presidential candidate should allocate his resources to convey as favorable an impression of his positions and personality as possible. We assumed that the favorableness of his image to uncommitted voters in each state would be a function of the amount of resources he allocated to each state, as compared with the amount allocated by his opponent.

Under a system of direct popular-vote election of a president, we postulated that a candidate would seek to maximize his expected popular vote. If this were a candidate's goal, we showed that his optimal minimax strategy would be to allocate his resources in proportion to the number of uncommitted voters in each state. If one candidate deviated from this strategy, and these deviations were known to his opponent, we indicated how this information could be used by the opponent to capitalize on the weaknesses of the candidate who departed from his minimax strategy.

Under the present Electoral-College system, we demonstrated that there is no minimax solution to the resource-allocation problem in pure strategies. We showed, however, that if candidates matched each other's resource expenditures, and if their total resources were the same, then their optimal allocations would be roughly proportionate to the 3/2's power of the electoral votes of each state (subsequently refined to include population). This rule encourages disproportionately large expenditures in the largest states, whose plausibility we demonstrated by showing how the potential voting power of a voter, derived from a measure of his decisiveness, is greater for voters in large states than in small states. We also indicated that the 3/2's allocations are inherently unstable, and we described the most devastating strategy that could be used against them by a candidate who exploits information about his opponent's allocations.

As a test of the models, we compared the 3/2's allocation rule to a proportionate allocation rule based on the electoral votes of each state. Using time as a resource, we operationalized its allocation in terms of the number of campaign appearances of presidential and vice-presidential candidates in each state in the 1960, 1964, 1968, and 1972 presidential election campaigns.

The 3/2's rule proved to be generally superior to the proportionate rule in explaining the time allocations of the candidates. For the upcoming presidential elections in 1976 and 1980, we indicated the 3/2's allocations for all states and electoral and individual biases that these allocations engender in each state. We concluded our analysis with a discussion of both theoretical and empirical limitations and extensions of the models.

We think that probably the most compelling conclusion that emerges from our analysis, at least for noncandidates, does not concern campaign strategy but rather concerns the severely unrepresentative character of presidential campaigns under the Electoral College. By forcing candidates—and after the election, incumbent presidents with an eye on the next election—to pay much greater attention in terms of their allocations of time, money, and other resources to the largest states, the Electoral College gives disproportionate weight to attitudes and opinions of voters in these states. On a per-capita basis, voters in California are 2.92 times as attractive campaign targets as voters in Washington, D.C.; even greater than this ratio of the most extreme individual biases is the most extreme electoral-bias ratio that makes California 8.13 times as attractive per electoral vote as Alaska.

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[47] For a discussion of literature in this area, see Steven J. Brams, "Positive Coalition Theory: The Relationship between Postulated Goals and Derived Behavior," in *Political Science Annual IV: An International Review*, ed. Cornelius P. Cotter (Indianapolis: Bobbs-Merrill Co., 1973), pp. 3–40.

Although no voter today is actually deprived of a vote by the Electoral College, we feel that an aggregation procedure that works to deflate the weight of some votes and inflate the weight of others is clearly in violation of the egalitarian principle of one man, one vote. The reapportionment decisions of the courts since 1962 have upheld this principle, with reapportionment itself obviating the need for an institution with a large-state "urban bias" like the Electoral College to offset the rapidly fading "rural bias" of the House of Representatives. Further, as a procedure that places a premium on gaining information and responding to an opponent's allocations in each state, the Electoral College tends to encourage manipulative strategic calculations for outmaneuvering the opposition in each state that may divert a candidate from seeking broad-based nationwide support.

This is the goal that we believe the electoral system should promote. The Electoral College subverts this goal by giving special dispensation to particular states and, additionally, fostering manipulative strategies in them. As an alternative, we think that direct popular-vote election of the president, which would render state boundaries irrelevant, would encourage candidates to maximize their nationwide appeal by tying their support directly to potential votes everywhere on a proportionate basis.

To close this analysis on a methodological note, we hardly need indicate that our analytic approach as well as our conclusions are not the usual stuff of conventional democratic theory, whose study is rooted mainly in the traditions of historical inquiry and speculative philosophy. Political theorists have by and large ignored all that smacks of formal analysis and quantitative empirical research. We think this is unfortunate, because questions that relate to such qualities as the representativeness and equality of political institutions, which we have explored in one context, are fundamental to almost all political theory.

We have tried to show that such questions can be fruitfully attacked not just by making endless observations of the workings of political institutions but by assuming that they generate particular forms of political behavior. The theoretical explication of this behavior, we have argued, requires the postulation of goals that motivate rational individuals to act in particular ways as they try to cope with, and benefit from, these institutions. To make sense of their actions in an ever-more complex world necessitates the construction of theoretical models that can help to transform vague insights into coherent and logical explanations that clarify the insights and relate them to each other.

Formal analysis will help to make these explanations more rigorous, but this analysis must be coupled with empirical research that can substantiate the truth of the logical assertions. The most relevant and powerful theory depends on the presence of both ingredients. We hope that our analysis helps to underscore the need for cooperation among theorists and empiricists of different persuasions in the investigation of important theoretical questions in concrete empirical settings.

Election Goals and Strategies: Equivalent and Nonequivalent Candidate Objectives*

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I. Introduction

This study examines the relationship between the goals an election candidate pursues and the campaign strategies he adopts. Specifically, we wish to know if different goals lead candidates to adopt the same or different strategies. While recognizing the importance of other objectives (e.g., the ideological desires that candidates hold), we limit the discussion to those objectives that are functions of the votes that candidates expect to receive.

The importance of ascertaining equivalence and nonequivalence among election goals arises in several different contexts. It is reasonable to postulate at the outset, for example, that a candidate's election goals affect the nature of the campaign strategies he adopts. If these strategies include the advocacy of a set of public policies, then election goals can influence the nature of the public policy that office-holders eventually enact. But, the selection of particular election goals themselves might be the result of different election environments, and descriptions of these environments include variations in election laws, procedures, competition, and information. To predict the enactment of a public policy, therefore, requires the construction of a reliable set of theoretical relationships from election environment to campaign objectives, from campaign objectives to election strategy, from election strategy to election outcome, and from election outcome to public policy.

Suppose, for example, that we postulate that candidates in proportional representation constituencies adopt strategies to maximize their respective proportions of the expected vote, while candidates in single-member district constituencies adopt strategies to maximize their respective expected pluralities. If these two objectives are equivalent, then *ceteris paribus*, we can expect to find no strategic variations that we can attribute to variations in the election system. If these two objectives are not equivalent, then the opposite is true. Certainly in either instance to ascertain equivalence or nonequivalence between maximizing expected plurality and maximizing proportion of the expected vote is a requirement for judging the policy consequences of the two election systems.[1]

A second example shows, moreover, that the postulation of election objectives within a given election system may not be simple. Kramer offers a pertinent example for plurality systems.

> With the usual electoral arrangements, winning is normally closely related to the size of the candidate's plurality. However, these two formulations of the candidate's goal, though related, may nevertheless lead to differing recommendations when used to assess the value of alternative campaign tactics [strategies]. This is particularly likely if one of the tactics is very risky, but also potentially very productive. For example, suppose the choice facing the candidate is between adopting such a tactic (t_1) versus continuing present tactics (t_2); moreover, suppose the candidate now has 55 per cent of the vote and will maintain this lead for sure with t_2. Tactic t_1, on the other hand, will either gain another 40 per cent or lose 10 per cent of the vote, each with a probability .5. If his goal is to maximize his expected plurality, the proper choice is to adopt t_1, since his expected plurality is 40 per cent then versus 10 per cent with t_2. On the other hand, choosing t_1 over t_2 reduces the probability of winning from 1.0 to .5, and therefore if the goal is to maximize his probability of winning, exactly the opposite recommendation is in order.[2]

This example is interesting for three reasons. First, it shows that even in plurality systems, candidates can pursue diverse objectives with varying strategic (and, hence, policy) consequences. Second, it raises a question that is implicit in Kramer's discussion: he treats expected plurality of vote pro-

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[1] The previous *ceteris paribus* condition is quite important. We may find differences in public policy that, for example, we can attribute to *legislative* (e.g., coalition) differences, but not to electoral differences.

[2] Gerald H. Kramer, "A Decision-Theoretic Analysis of a Problem in Political Campaigning," in *Mathematical Applications in Political Science,* II, Joseph L. Berndt, ed. (Dallas: Southern Methodist University Press, 1966), pp. 140–141.

portion and probability of winning as nonequivalent goals; as we show later, however, this is an assumption that may not be justified. Third, considering vote proportion apart, the example suggests that both proportional representation and plurality systems can generate identical election objectives.

Sections III and IV contain the major findings of our study concerning these examples. It is sufficient here to observe that a discussion of equivalence and nonequivalence of election objectives is a prerequisite for understanding the effects of election laws, procedures, reforms, and other characteristics of the electoral environment. Section II begins this discussion with some notation and definitions, as well as an enumeration of six alternative objective functions. Section III offers some conditions for equivalence and nonequivalence among these alternative objectives. The definition of equivalence we use in Section III posits either that all candidates adopt electoral equilibria or that a given candidate believes that all of his opponents shall adopt equilibria. Additionally, this discussion posits some strong conditions about a candidate's information and his attitude toward risk. Section IV weakens these conditions and examines the problem of variations in risk. Finally, Section V describes several determinants of election goals, and suggests some implications of the preceding discussion for election theory.

II. Notation, Definitions and Objective Functions

A. Notation

1. *Strategies*. An election consists of $n \geq 2$ candidates (parties, lists, slates, etc.), indexed $i=1, 2, \cdots, n$. Further, we let θ_i denote candidate i's strategy and we denote the strategies of all n candidates by the vector $(\theta_1, \theta_2, \cdots, \theta_n)$. Although this notation is consistent with that of present theories of electoral competition, it has some wider interpretations; hence, a brief discussion is in order.

Traditional spatial theory posits that θ_i is the vector $(\theta_{i1}, \theta_{i2}, \cdots)$, such that θ_{ij} is candidate i's position on issue j. Clearly, though, if we define an election strategy as anything a contestant might vary in pursuit of his election goals, then issue position considerations alone do not exhaust the possible strategies.

Although a complete enumeration of possible components of a candidate's strategy is not our central concern, these components can include: (1) issue positions—policy advocacy—like integration vs. segregation, escalation vs. status quo vs. de-escalation of a war, higher vs. lower welfare expenditures, and more vs. less governmental pollution control; (2) the relative emphasis the candidate places on each issue position during his campaign; (3) the certainty or uncertainty he generates about his issue positions; (4) the relative emphasis the candidate places on personal characteristics over which he has little or no immediate control—like age, sex, party identification, religion, and prior incumbency; (5) variations in campaign allocations of money, time, etc., in identifiable sections of the candidate's constituency as well as among different media of campaigning. Since the subsequent analysis is general, the components of θ_i may include any or all of these possibilities as well as others that we do not list.

2. *Expressions for votes*. $V_i(\theta)$ denotes the proportion of citizens voting for candidate i. This notation posits that each citizen's decision to vote for one of the candidates or to abstain is a function of the strategies of all n candidates. Similarly, $v_i(\theta)$ denotes the expected value of $V_i(\theta)$. This convention introduces risk by assuming that $V_i(\theta)$ can be a random variable with mean value of $v_i(\theta)$.

3. *Objective functions*. The function $\phi_i(\theta)$ denotes candidate i's objective function, or payoff. Naturally, ϕ_i depends on θ, the strategies the candidates select. We assume, further, that ϕ_i is a formal specification of the candidate's election goal, and that he prefers θ' to θ'' if $\phi_i(\theta') > \phi_i(\theta'')$. That is, the candidate's election goal is a standard against which he can evaluate alternative strategy sets. The function $\phi_i(\theta)$ measures this evaluation.

B. Definitions

1. *Equilibrium*. Since the candidate's payoff, $\phi_i(\theta)$, depends not only on the strategy he employs, θ_i, but also on the strategies his opponents employ, an election is an n-person game. We assume that this game is non-cooperative: each candidate seeks independently to adopt a strategy that is best for him and candidates do not coordinate their strategies by forming election coalitions. A liberal and a conservative, for example, initially might coordinate their strategies to defeat a moderate, with the understanding between them that their coalition dissolves once the moderate candidate loses. We ignore such complications here.

Given the concept of an election as a non-cooperative game, $\theta^* = (\theta_1^*, \theta_2^*, \cdots, \theta_n^*)$ is an equilibrium strategy set with respect to the set of objectives $(\phi_1, \phi_2, \cdots, \phi_n)$ if,

$$\phi_i(\overset{*}{\theta}) \geq \phi_i(\overset{*}{\theta_1}, \overset{*}{\theta_2}, \cdots, \theta_i, \cdots, \overset{*}{\theta_n}), \quad (1)$$

for all i and $\theta_i \neq \theta^*$. That is, given each candidate's objective function, θ^* is an equilibrium strategy set if no candidate has an incentive to alter his strategy unilaterally once all n candidates arrive at their respective equilibrium strategies.

Although candidates do not necessarily adopt equilibrium strategies, these strategies are powerfully attractive. If the election is zero-sum, for example, or if θ^* is unique, then equilibrium strategies are reasonable definitions of solutions to the election game.[3] The correspondence between spatial and game theoretic formulations illustrates this solution concept. The spatial theory of election competition ascertains the existence and location of θ^*.[4] Typically, spatial analysis might ask: should two plurality maximizing candidates converge to the electorate's median preference? The game theoretic restatement of the question is thus: if $n=2$, and ϕ_i denotes candidate i's plurality, $i=1, 2$, then does a unique strategy pair (θ_1^*, θ_2^*) exist that satisfies (1) and for which $\theta_1^*=\theta_2^*=$ the median preference?

2. *Equivalent objective functions.* The first definition of equivalent objective functions follows the sense of equilibrium just stated.[5]

Definition 1: *ϕ' and ϕ'' are equivalent objective functions for candidate i if, given his opponents' objective functions, θ^* is an equilibrium strategy set under $\phi_i' \Leftrightarrow \theta^*$ is an equilibrium strategy set under ϕ_i''.*[6]

Two objective functions are equivalent, then, if a candidate can substitute one for the other with no incentive to disrupt the electoral equilibrium. If, for example, two elections are identical except that in one a candidate maximizes expected plurality, while in the other he maximizes his probability of winning, then the equilibrium strategy sets are identical if the two objectives satisfy Definition 1.

To illustrate further the meaning of Definition 1, we state without proof the following lemma.

Lemma: *If ϕ' and ϕ'' are equivalent for candidates i and k, and if θ^* is an equilibrium for any of,*

$$(\phi_1, \cdots, \phi_i', \cdots, \phi_k', \cdots \quad \phi_n),$$
$$(\phi_1, \cdot \quad \cdot, \phi_i'', \cdots, \phi_k', \cdots \quad \phi_n),$$
$$(\phi_1, \cdots, \phi_i', \cdots, \phi_k'', \cdots, \phi_n),$$
$$(\phi_1, \cdots, \phi_i'', \cdots, \phi_k'', \cdots, \phi_n),$$

then θ^* is an equilibrium for all four.

Suppose, for example, that $\theta^*=(\theta_1^*, \theta_2^*)$ is an equilibrium pair for two candidates if each maximizes his probability of winning. To demonstrate that θ^* is also an equilibrium if each candidate maximizes his plurality, or if one candidate maximizes plurality while the other maximizes probability of winning, the lemma states that it is sufficient to show that maximizing plurality and maximizing probability of winning satisfy Definition 1 for each candidate.

Some elections, of course, may not have equilibrium strategy sets. And, even if an election has an equilibrium, it may be unreasonable to suppose that it is a solution to the election game (e.g., as in the two-person, nonzero-sum, non-cooperative game of the Battle of the Sexes). Definition 1, however, incorporates the concept of equilibrium strategy sets to introduce one notion of equivalence. There may be other useful definitions of equivalence, so that two or more objective functions are equivalent under one definition, but not under another.[7] Section IV considers such an alternative definition, which is particularly useful if one or more candidates do not adopt equilibrium strategies.

3. *Symmetric elections.* Our final two definitions concern the nature of electoral competition. An

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[3] If only two candidates compete ($n=2$), and if $\phi_i(\theta)$ denotes candidate i's expected plurality, the game is zero-sum and $\theta^*=(\theta_1^*, \theta_2^*)$ is a pure strategy minimax solution. If the game is nonzero-sum, but θ^* is unique, then θ^* is a Nash solution to the election game. See, for example, the discussion in R. Duncan Luce and Howard Raiffa, *Games and Decisions: Introduciton and Critical Survey* (New York: Wiley, 1957).

[4] For the correspondence between spatial analysis and the theory of games, see James L. Barr and Otto A. Davis, "An Elementary Political and Economic Theory of the Expenditures of Local Governments," *The Southern Economic Journal,* 33 (October, 1966), 149–165; Martin Shubik, "A Two-Party System, General Equilibrium, and the Voters' Paradox, "*Zeitschrift für Nationalökonomie,* 28 (1968), 341–354; Melvin J. Hinich, John O. Ledyard, and Peter C. Ordeshook, "A Theory of Electoral Equilibrium: A Spatial Analysis Based on the Theory of Games," *The Journal of Politics,* 35 (February, 1973), 154–193 and Peter C. Ordeshook, "Pareto Optimality and Election Competition," *American Political Science Review,* 65 (December, 1971), 1141–1145. See also John M. Kessel, "A Game Theoretic Analysis of Campaign Strategies," in *The Electoral Process,* ed. M. Kent Jennings and L. Harmon Zeigler (Englewood Cliffs, New Jersey: Prentice-Hall, 1966), pp. 290–304.

[5] Later in Section IV we offer a second definition of equivalence that does not require candidates to adopt equilibrium strategies. To avoid confusion, we ignore the second definition until that section.

[6] A useful restatement of Definition 1 is: if θ^* satisfies (1) for all $j \neq i$, ϕ_i' and ϕ_i'' are equivalent if $\phi_i'(\theta^*) \geq \phi_i'(\theta_1^*, \cdots, \theta_i, \cdots, \theta_n^*) \Leftrightarrow \phi_i''(\theta^*) \geq \phi_i''(\theta^*, \cdots, \theta_i, \cdots, \theta_n^*)$. In the proofs of the theorems we show that two objective functions are equivalent by assuming that θ^* satisfies (1) for all candidates except i, and by demonstrating that if θ^* satisfies (1) with ϕ_i', it satisfies (1) with ϕ_i'', and vice-versa.

[7] Here we note simply that the condition of equilibrium we incorporate in the definition of equivalence has an internal prescriptive logic. That is, even if a candidate has no knowledge of his opponent's(') likely strategies, he might make a minimax-like calculation, especially in a competitive context, and assume that his opponents will "do their damnedest" by adopting their respective equilibria.

election is *symmetric* with respect to $(\phi_1, \phi_2, \cdots, \phi_n)$ if, for all i and j, $v_i(\theta^*)=v_j(\theta^*)$, such that θ^* is an equilibrium strategy set under $(\phi_1, \phi_2, \cdots, \phi_n)$.[8] That is, at equilibrium a symmetric election is one in which each candidate has an equal number of expected votes. Consequently, a two-candidate election is symmetric if: (1) both candidates employ the same objective function; (2) the candidates choose from identical strategy sets (e.g., we do not restrict one candidate to "liberal" positions and his opponent to "conservative" positions); and (3) citizens are not predisposed to vote for one candidate or the other (i.e., the utility the citizen associates with a candidate derives solely from θ_i and the citizen is as likely to vote for one candidate as the other if he is indifferent between them with respect to θ).

Consider three examples of nonsymmetric elections. First, if a bimodal density describes the electorate's preferences, if three candidates compete, and if an equilibrium strategy set entails a candidate at each mode and a candidate at the mean, then the election is not symmetric because the candidate at the mean receives, generally, a different number of votes than each of his opponents receives. Second, if an electorate is biased in favor of one of the candidates in a two-candidate election, so that even at equilibrium his opponent must lose, then the election is not symmetric because $v_i(\theta^*)\neq v_j(\theta^*)$. Third, suppose that the candidates choose from identical strategy sets and the electorate has no nonstrategic biases; if $n-1$ candidates maximize their respective proportions of the expected vote, and if the nth candidate maximizes his expected vote, then the election is not symmetric if there is no reason to suppose that all candidates receive the same expected vote in equilibrium.

4. *Strongly symmetric elections.* An election is *strongly symmetric* with respect to candidate i if it is symmetric and if

$$v_j(\overset{*}{\theta_1}, \cdots, \theta_i, \cdots, \overset{*}{\theta_n}) = v_k(\overset{*}{\theta_1}, \cdots, \theta_i, \cdots, \overset{*}{\theta_n})$$

for all j, $k\neq i$. Hence, a strongly symmetric election yields an n-way tie if all candidates adopt equilibrium strategies; it yields an $(n-1)$-way tie if one of the candidates adopts some other strategy. All two-candidate symmetric elections, then, are strongly symmetric, since there is only one opponent. Similarly, if all candidates converge in equilibrium for a multi-candidate symmetric election, then it is strongly symmetric also, since if one candidate moves to another strategy, he affects his opponents' expected votes equally.[9]

A symmetric election might not be strongly symmetric. Suppose, for example, that the electorate's preference density is bivariate normal, that θ represents spatial strategies, and that three candidates are in equilibrium if they are located symmetrically about the mean. This election is not strongly symmetric because if one candidate shifts toward one opponent and away from the other, then he cannot affect them equally.

The reader should interpret these definitions of symmetry and strong symmetry with care. We do not use them as assumptions about electoral competition, nor do we believe that they are often satisfied, as conditions, in real elections. Indeed, many of our results do not employ them. We can establish certain equivalences, though, if (and in one instance only if) we assume that elections are either symmetric or strongly symmetric. We discover in this way not only some conditions for which certain goals are equivalent, but also some equally important conditions for which these goals are *not equivalent.*

C. Alternative Candidate Goals (Objective Functions)

We specify here six alternative definitions of $\phi_i(\theta)$. To anticipate the discussion in Section V, we believe there are several reasons for a formal treatment, beyond a preference for rigor and a desire to investigate links between campaign goals and strategies. First, even writers who argue that "votes" are a basic election goal treat these goals-as-functions-of-votes with remarkable ambiguity and looseness. Indeed, one gathers from several places the notion that, *ceteris paribus*, all candidates who maximize votes, plurality, or proportion of the vote, and sometimes probability of winning, always operate under the same strategic incentives.[10] Several examples in Section III disprove this notion. Second, the goal of "winning" may take many forms as a function of vote, depending on the election environment and even on postelection coalition formation processes. To maximize vote proportion, even in a proportional representation system, might be desirable in one

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[8] This is a property of most spatial analyses. The exceptions to this are in: Hinich, Ledyard, and Ordeshook; and David Chapman, "Models of the Working of a Two-Party Electoral System," *Public Choice*, 3 (Fall, 1967), 19–37, and 5 (Fall, 1968), 19–37.

[9] For an example of such an equilibrium, see Gordon Tullock, *Toward a Mathematics of Politics* (Ann Arbor: The University of Michigan Press, 1967), pp. 53–55.

[10] See Melvin J. Hinich and Peter C. Ordeshook, "Plurality Maximization vs Vote Maximization: A Spatial Analysis with Variable Participation," *American Political Science Review*, 64 (September, 1970), 772–791, which cites examples of this ambiguity. Specifically, many political scientists treat vote, plurality, and proportion maximization as if they were equivalent.

Table 1. Candidate Objective Functions

		Interpretation; Maximizing:
O_1	$\phi_i = v_i(\theta) - \max_{j \neq i} \{v_j(\theta)\}$	Expected Plurality
O_2	$\phi_i = v_i(\theta) \Big/ \sum_{j=1}^{n} v_j(\theta)$	Proportion of the Expected Vote
O_3	$\phi_i = v_i(\theta)$	Expected Vote
O_4	$\phi_i = \Pr\left[V_i(\theta) - \max_{j \neq i} \{V_j(\theta)\} \geq \lambda_i\right]$	Probability that Plurality Exceeds Some Level
O_5	$\phi_i = \Pr\left[V_i(\theta) \Big/ \sum_{j=1}^{n} V_j(\theta) \geq \lambda_i\right]$	Probability that Proportion Exceeds Some Level
O_6	$\phi_i = \Pr[V_i(\theta) \geq \lambda_i]$	Probability that Vote Exceeds Some Level

election, for example, and disastrous in another.[11] Third, even for a particular set of election rules, candidates might adopt different objective functions, depending on their information, their expectations and their hopes for future elections.

Table 1 lists the six objective functions we consider here, in terms of their formal statements and verbal interpretations. The following discussion details briefly the nature and possible sources of these objective functions. We relegate to Section V a more nearly complete discussion of the conditions that generate these goals, however, so that we may have the benefit of the findings of equivalence and nonequivalence among them.

1. *Maximizing expected plurality*, O_1. It is reasonable to suppose that for two-candidate, plurality system elections, each candidate seeks strategies that maximize his expected plurality, $\phi_i(\theta) = v_i(\theta) - v_j(\theta)$. Since this analysis includes n-candidate elections, we offer the generalization that $\phi_i(\theta) = v_i(\theta) - \max_{j \neq i}\{v_j(\theta)\}$, such that "$\max_{j \neq i}\{v_j(\theta)\}$" reads, "the maximum $v_j(\theta), j \neq i$." Thus, O_1 dictates that the candidate seeks strategies that maximize his expected plurality in relation to his strongest opponent. O_1 includes two-candidate elections as a special case, since if $n=2$, $\phi_i(\theta) = v_i(\theta) - v_j(\theta)$.

2. *Maximizing proportion of the expected vote*, O_2. While O_1 may be a reasonable representation of the candidate's objective function for plurality election systems, O_2, maximizing proportion of the expected vote, is an intuitively plausible goal for proportional representation system candidates.[12] Thus,

$$\phi_i(\theta) = v_i(\theta) \Big/ \sum_{j=1}^{n} v_j(\theta).$$

By positing O_1 and O_2, therefore, it is possible to compare the candidate's strategic incentives in two distinct electoral systems.

3. *Maximizing expected vote*, O_3. Frequently, a candidate can estimate only the citizens' responses to his own campaign. That is, he does not know how his opponent is doing. Spatial theory often employs a useful representation for this situation as an alternative to O_1 and O_2, namely O_3: $\phi_i(\theta) = v_i(\theta)$. If the candidate uses O_3 as his objective function, he seeks strategies that maximize his expected vote.

4. *Maximizing the probability that plurality exceeds some level*, O_4. Objective O_4 is $\phi_i(\theta) = \Pr[V_i(\theta) - \max_{j \neq 1}\{V_j(\theta)\} \geq \lambda_i]$, such that "Pr" reads, "the probability that." To illustrate O_4, assume that $\lambda_i = 0$ and that two candidates, i and j, compete. Thus, O_4 reduces to $\Pr[V_i(\theta) \geq V_j(\theta)]$, which is the probability that candidate i does no worse than tie his opponent. If ties are unlikely

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[11] There are, of course, other alternative goals for candidates in proportional representation systems. Moreover, these goals are dependent upon postelection coalition rules. For a discussion of these dependencies, see Peter H. Aranson, "A Theory of the Calculus of Voting for Alternative Three-Contestant Election Systems" (Ph.D. dissertation, The University of Rochester, Rochester, New York, 1972), chap. iii.

[12] It might seem that maximizing expected proportion of the vote is an alternative to maximizing proportion of the expected vote. While the relationship between these two objectives depends on the specific stochastic properties of $V_i(\theta)$, we show in the proof of Theorem 5 (Appendix) that with certain stochastic assumptions they approximate each other. We note also that another possible objective for candidates is maximizing plurality of vote proportion. For two candidate contests, however, this objective is equivalent to O_2. Specifically,

$$\frac{v_1 - v_2}{v_1 + v_2} = \frac{2v_1}{v_1 + v_2} - 1$$

(since $v_2/(v_1+v_2) = 1 - v_1/(v_1+v_2)$), which is linearly monotonic with $v_1/(v_1+v_2)$.

(i.e., if the number of citizens is sufficiently large), then this expression is, simply, the probability that the candidate beats his opponent. Similarly, if $\lambda_i = 0$ and $n > 2$, then O_4 is the probability that no opponent receives more votes than candidate *i*. Thus, if $\lambda_i = 0$, O_4 is the formal statement of the assumption that the candidate adopts election strategies that maximize his probability of winning in plurality election systems.

We permit λ_i to assume other values, though, to ascertain the effects of plurality quotas on the candidates' strategies. As we describe this possibility later, such quotas can arise from a desire to "do well enough" to win other nominations or offices, as well as from the imperatives party leaders set for ward, precinct, and district chairmen, in relation to mandatory pluralities.

5. *Maximizing the probability that proportion of the vote exceeds some level*, O_5. Analogous to O_4, objective O_5 is

$$\phi_i(\theta) = \Pr\left[V_i(\theta) \bigg/ \sum_{j=1}^{n} V_j(\theta) \geq \lambda_i\right].$$

This analogy pertains specifically to the correspondence between O_1 and O_4 for plurality election systems; hence, O_2 and O_5 might offer alternative proportional representation election system goals. Objective O_5 also might represent a candidate's primary election strategy, should a minimum proportion insure the viability of his candidacy over a series of sequential (e.g., presidential) primary contests.

6. *Maximizing the probability that vote exceeds some level*, O_6. Paralleling the discussions of O_4 and O_5, an alternative to O_3, maximizing expected vote, is O_6, $\phi_i(\theta) = \Pr[V_i(\theta) \geq \lambda_i]$. As with O_4 and O_5, so with O_6: we do not constrain λ_i to a particular value. Again, precinct and ward leader vote quotas may be the source of O_6.

7. *Some omissions.* This completes the initial specification of candidate objective functions. We do not suppose that the list is exhaustive, since we ignore: (1) Riker's suggestion that candidates and party leaders seek minimum-winning coalitions of voters; (2) the hypothesis that candidates pursue ideological goals and want their campaigns to focus on certain public policy demands, as well as (or instead of) pursuing election victory; and (3) the possibility of cooperative (e.g., coordinate) election strategies.[13]

Each of these omissions (and others) reduces the generality of the analysis. The objective functions included, though, warrant analysis not only because some of them are central assumptions of spatial theory, but also because they are sufficiently general to provide a base for a rigorous analysis of more complex goals. We turn, then, to the central issue of this essay: the identification of equivalent and nonequivalent candidate objective functions.

III. Equivalence and Nonequivalence

A. Equivalence and Nonequivalence among O_1, O_2, and O_3

We present here two theorems that give sufficient conditions for certain equivalences among O_1, O_2, and O_3, and a third theorem that gives necessary and sufficient conditions for equivalence between O_1 and O_2. The Appendix contains the proofs of these and all other theorems.

1. *Statement and discussion of Theorem 1.*

THEOREM 1: *Let* $\theta' = (\theta_1, \theta_2, \cdots, \theta_i', \cdots, \theta_n)$, *and* $\theta'' = (\theta_1, \theta_2, \cdots, \theta_i'', \cdots, \theta_n)$; *if for all* $j \neq i$ *and for all* θ,

(*a*) $v_i(\theta') < v_i(\theta'') \Rightarrow v_j(\theta') \geq v_j(\theta'')$,

(*b*) $v_i(\theta') = v_i(\theta'') \Rightarrow v_j(\theta') = v_j(\theta'')$,

(*c*) $v_i(\theta') > v_i(\theta'') \Leftarrow v_j(\theta') < v_j(\theta'')$,

then O_1, O_2, *and* O_3 *are equivalent for candidate i.*

The operant conditions of Theorem 1—the conditions (together) that insure equivalence among O_1, O_2, and O_3—are quite simple: (a) if candidate *i* changes his strategy from θ_i' to θ_i'', intending to increase his expected vote, then his opponent's(') expected vote(s) must either decrease or remain unchanged; (b) if he changes his strategy from θ_i to θ_i'', and his expected vote remains unchanged, then his opponent's(') expected vote(s) must remain unchanged; (c) if he adopts a strategy to increase his opponent's(') expected vote(s), then his expected vote must decrease. Thus, Theorem 1 requires that, in terms of expected vote, the election is much like a zero-sum game.

Two examples for which Theorem 1 holds demonstrate the logic of the theorem. First, consider a simple two-candidate plurality system election with constant turnout: $v_i(\theta) + v_j(\theta) = c$. This election satisfies all three conditions of Theorem 1: (a) if $v_i(\theta)$ increases, then $v_j(\theta)$ must decrease; (b) if a change in θ_i produces no change in $v_i(\theta)$, then it produces no change in $v_j(\theta)$; (c) if candidate *i* adopts a strategy that increases $v_j(\theta)$, then $v_i(\theta)$ must decrease. Additionally, the structure of the example is sufficiently clear to demonstrate equivalence among O_1, O_2, and O_3. If candidate *i* adopts an expected vote maximizing strategy, then necessarily his additional votes are at candidate *j*'s expense; hence, such a strategy also maximizes expected plurality and proportion of the expected vote. Surely, most two-candidate elections with universal or nearly universal turnout meet the conditions of Theorem 1.

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[13] William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962).

Second, consider an election like the previous one, except that turnout is a variable and that $v_j(\theta)$ is insensitive to variations in θ_i. This election satisfies two conditions of Theorem 1: (a) if $v_i(\theta)$ increases, then $v_j(\theta)$ remains unchanged; (b) if a change in θ_i does not change $v_i(\theta)$, then it does not change $v_j(\theta)$; condition (c) is irrelevant, as the implication is unnecessary for the example. The logic of this second example is that if candidate *i* adopts a strategy to maximize expected plurality, O_1, then it must be a vote maximizing strategy, O_3, since $v_j(\theta)$ remains invariant. Similarly, candidate *i* maximizes his proportion of the expected vote, O_2, with O_3 (assuming $v_j(\theta) > 0$). While the first example is applicable to two-candidate elections with universal turnout, this example is applicable to two-candidate elections with variable turnout. An example of such an election is one in which two non-overlapping preference distributions characterize the electorate (the distribution is strongly bimodal), where if the citizen at one of the modes votes, he votes for a candidate associated with that mode; and where alienation induces abstention.

To illustrate further the three conditions of Theorem 1, consider two elections that fail to satisfy these conditions, and for which equivalence does not hold. These examples are interesting, moreover, because they refute the intuitive supposition that, at least for two-candidate elections, O_1, maximizing expected plurality, and O_2, maximizing proportion of the expected vote, are always equivalent.

Assume for the first example that one candidate's expected vote is a constant proportion of the other candidate's expected vote; that is, $v_j(\theta) = cv_i(\theta)$, $c \geq 0$. This example violates conditions (a) and (c) of Theorem 1: (a) if $c > 0$, then an increase in $v_i(\theta)$ increases (rather than decreases or leaves unchanged) $v_j(\theta)$; (c) if $c \geq 0$, then a decrease in $v_j(\theta)$ at best leaves $v_i(\theta)$ unchanged (rather than increasing it). To show nonequivalence formally, observe that candidate i's expected vote is simply $v_i(\theta)$; his expected plurality is

$$v_i(\theta) - v_j(\theta) = (1 - c)v_i(\theta);$$

his proportion of the expected vote is

$$v_i(\theta)/[v_i(\theta) + v_j(\theta)] = v_i(\theta)/[v_i(\theta) + cv_i(\theta)] = 1/[1 + c].$$

Clearly, proportion of the expected vote is constant for all θ, but expected vote and expected plurality are not constant for all θ (except for the case in which $v_i(\theta)$ is constant for all θ and $c = 1$). Thus, the candidate is indifferent among all θ for O_2; he is not indifferent among all θ for O_1 and O_3. The implication of this result is that all θ can be equilibria for O_2, but not for O_1 and O_3, which is to say that O_2 is equivalent neither to O_1 nor to O_3. Comparing O_1 and O_3, if $c = 1$, then expected plurality is a constant but expected vote is not; hence O_1 and O_3 are not equivalent. If $c > 1$, then an increase in $v_i(\theta)$ decreases candidate *i*'s expected plurality ($1 - c < 0$); hence candidate *i*'s expected plurality becomes more negative as $v_i(\theta)$, his expected vote, increases. Again, therefore, O_1 and O_3 are not equivalent.[14]

Assume for the second example that expected plurality is a constant. That is, $v_i(\theta) - v_j(\theta) = -c$, $c > 0$. This example violates conditions (a) and (c) of Theorem 1: (a) since $v_i(\theta) = v_j(\theta) - c$, an increase in $v_i(\theta)$ increases (rather than decreases or leaves unchanged) $v_j(\theta)$; (c) a decrease in $v_i(\theta)$ decreases (rather than increases) $v_j(\theta)$. To show nonequivalence formally, observe that candidate *i*'s proportion of the expected vote is

$$v_i(\theta)/[v_i(\theta) + v_j(\theta)] = v_i(\theta)/[2v_i(\theta) + c],$$

which increases monotonically with $v_i(\theta)$. But O_1, expected plurality, is constant for all θ, while O_2, proportion of the expected vote, and O_3, expected vote, are not constant for all θ (except for the e-d generate case of $v_i(\theta)$ constant for all θ). Paralleling the previous example, then, all strategies can be equilibria for O_1, but not for O_2 or O_3. Hence, O_1 is equivalent neither to O_2 nor to O_3.[15]

Statement and discussion of Theorem 2. Recalling the definition of strong symmetry, a sufficient condition for equivalence between O_1 and O_2 is,

> THEOREM 2: *If an election is strongly symmetric with respect to candidate i, then O_1 and O_2 are equivalent for candidate i.*

Two examples for which Theorem 2 holds demonstrate its logic. First, suppose that a symmetric unimodal density describes the electorate's preferences in a two-candidate election with neither electorate biases nor spatial strategy constraints. We know that for such conditions plurality maximizing candidates converge to the electorate's median preference. Since this election conforms to one of the previous examples of strong symmetry, it follows from the lemma and Theorem 2, that two candidates who maximize proportion of the expected vote, O_2, converge also to the median preference. Similarly, if two expected plurality maximizing candidates fail to converge because preferences are distributed bimodally, then two candidates who maximize their respective proportions of the expected vote fail to converge also.

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[14] Observe that if $c < 1$, candidate *i*'s plurality is a monotonically increasing function of $v_i(\theta)$, but candidate *j*'s plurality is a monotonically decreasing function of $v_j(\theta)$. This example is interesting, therefore, because with it if O_1 and O_3 are equivalent for candidate *i*, they cannot be equivalent for candidate *j*, and vice versa.

[15] We observe, though, that O_2 and O_3 can be equivalent. But, this emphasizes that Theorem 1 presents a sufficient condition for equivalence, and not a necessary one.

Second, suppose that an election is symmetric, that more than two candidates vie, and that if each candidate maximizes his proportion of the expected vote, then all candidates converge to the median preference; it follows that plurality maximizing candidates converge to the median: the election is strongly symmetric, since if one candidate moves off the median, he affects the expected votes of all of the other candidates equally.

To illustrate further the implications of Theorem 2, recall the two examples we cite earlier that violate the conditions of Theorem 1, and that do not yield equivalence among O_1, O_2, and O_3. Since Theorem 2 presents a sufficient condition for equivalence, then it must be the case that these examples fail also to satisfy the symmetry condition. First, with respect to the example for which $v_j(\theta)=cv_i(\theta)$, $c\geq 0$, $v_i(\theta^*)\neq v_j(\theta^*)$ (except for the cases of $c=1$ or $v_i(\theta^*)=0$ and $c=0$). Hence, the election is not symmetric. (If $c=1$, of course, O_1 and O_2 are equivalent, since expected plurality and proportion of the expected vote are constant for all θ.) Second, with respect to the example for which $v_i(\theta)-v_j(\theta)=-c$, $c>0$, since $v_j(\theta)=v_i(\theta)+c$, if $c\neq 0$, $v_j(\theta^*)\neq v_i(\theta^*)$. Hence, again the election is not symmetric.

3. *Statement and discussion of Theorem 3.* It is apparent from the discussion in Section II that symmetry and strong symmetry are restrictive conditions. Furthermore, if these were necessary conditions for the equivalence of O_1 and O_2, then it would be reasonable to conclude that O_1 and O_2 are rarely equivalent in real elections. But, perhaps unfortunately, Theorem 2 establishes strong symmetry only as a sufficient condition for equivalence, and not as a necessary one. Hence, it is reasonable to ask if O_1 and O_2 are likely to be equivalent if an election is neither strongly symmetric nor even symmetric. We cannot now answer this question. But, we can offer one circumstance in which symmetry (the weaker condition) is necessary for equivalence. Specifically,

THEOREM 3: *If $n=2$, and if $v_i(\theta)$, $i=1, 2$, is everywhere differentiable in θ_i, then O_1 and O_2 are equivalent for candidate i if and only if (i) the election is symmetric, or (ii) O_1 and O_2 are equivalent to O_3 for candidate i.*

This result does not prove that O_1 and O_2 are equivalent in all circumstances only if (i) or (ii) holds. More than two candidates may compete or the objective function may not be everywhere differentiable in θ. We offer the conjecture, nevertheless, that except for unusual circumstances, this result can be generalized. Consequently, since symmetry and strong symmetry are restrictive conditions, and since it appears that O_1 and O_3, and O_2 and O_3 are equivalent only under equally restrictive conditions (e.g., turnout is constant), we offer the additional conjecture that O_1 and O_2 are not equivalent in most real elections.

B. Equivalence and Nonequivalence for O_4, O_5, and O_6.

The analysis in Section III, Part A, rests on the assumption that the candidate acts as if he knows only the expected value, $v_i(\theta)$, of each $V_i(\theta)$. To analyze a candidate's strategic incentives from O_4, O_5, and O_6, though, requires some additional assumptions about the candidate's forecast error—assumptions about the distribution of $V_i(\theta)$. Suppose, then, that candidates estimate $V_i(\theta)$ in accordance with the usual stochastic assumptions of linear models. That is, let

$$V_i(\theta) = v_i(\theta) + \epsilon_i, \qquad i = 1, 2, \cdots, n$$

and,

ASSUMPTION A1: *$E(\epsilon_i)=0$, and ϵ_i is distributed independently of θ.*[16]

*A*1 requires, for example, that the variance of ϵ_1 does not depend on $v_i(\theta)$.

The following theorem is a straightforward implication of this assumption about forecast errors.

THEOREM 4: *If A1 holds, O_1 is equivalent to O_4 and O_3 is equivalent to O_6 for candidate i.*

An important corollary to Theorem 4 is,

COROLLARY: *If A1 holds, maximizing expected plurality and maximizing the probability of winning in a plurality system are equivalent.*

Maximizing expected plurality, O_1, and maximizing the probability of winning ($\lambda_i=0$ for O_4) or the probability that plurality exceeds some other level appear to be intuitively appealing and seemingly distinct candidate goals; our earlier reference to Kramer is not unusual; if A1 holds, though, they are equivalent goals. Theorem 4 and its Corollary impose no conditions, moreover, on the context of election competition. Unlike the equivalence among O_1, O_2, and O_3 that Theorem 1 identifies, Theorem 4 and its Corollary are independent of the zero-sum-like conditions in expected vote. Unlike the equivalence between O_1 and O_2 that Theorems 2 and 3 identify, Theorem 4 and its Corollary are independent of the strong symmetry and symmetry conditions. Theorem 4 and its Corollary depend solely on the properties of the candidate's forecast error.

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[16] One technical difficulty arises with this assumption because the dependent variables are proportions (of citizens) and, therefore, are bounded between 0 and 1. And, for values of $v_i(\theta)$, etc., near 0 and 1, the error distribution must change so as not to exceed the bounds. A simple resolution of this problem is to ignore candidates whose expected vote is small and to assume that candidates cannot adopt strategies that guarantee them nearly all of the vote. In nearly all elections, of course, the set of conceivable outcomes is considerably smaller than the set of theoretically possible outcomes. In our theoretical models, then, we can simply restrict the candidates to this set and assume that Assumption 1 is satisfied over it.

But, Theorem 4 does not identify an equivalence between objectives O_2 and O_5: that is, A1 is not sufficient to guarantee equivalence between maximizing proportion of the expected vote and maximizing the probability that proportion of the vote exceeds some level. The reasons are instructive.

The logic of Theorem 4 is that with A1, the distributions (but not the means) of $V_i(\theta)$ and $V_i(\theta)-\max_{j\neq i}\{V_j(\theta)\}$ do not depend on θ. Hence, the distributions of these random variables shift like rigid bodies as their means shift. A strategy that maximizes the means, therefore, also maximizes the probability that vote or plurality exceeds some level (e.g., there is no heteroscedasticity). Section IV offers a theorem (Theorem 5) whose proof demonstrates, however, that the variance and covariance of the distribution of

$$V_i(\theta) \Big/ \sum_{j=1}^{n} V_j(\theta)$$

are functions of θ, even with A1. This can preclude equivalence between O_2 and O_5.

It is possible, however, to establish equivalence between O_2 and O_5 with a different formulation of the candidate's forecast error. That is, assume that the candidate does not estimate each $V_i(\theta)$, and does not use these estimates to calculate either proportion or probability that proportion exceeds some level. Assume, rather, that the candidate estimates proportion directly. That is, instead of assuming that

$$V_i(\theta) \Big/ \sum_{j=1}^{n} V_j(\theta) = [v_i(\theta) + \epsilon_i] \Big/ \sum_{j=1}^{n} [v_j(\theta) + \epsilon_j],$$

assume, rather, that

$$V_i(\theta) \Big/ \sum_{j=1}^{n} V_j(\theta) = \left[v_i(\theta) \Big/ \sum_{j=1}^{n} v_j(\theta) \right] + \epsilon_i.$$

If the error term in this second expression, ϵ_i, satisfies A1, then as in the proof of Theorem 4, the distribution of a candidate's proportion of the vote shifts with its mean like a rigid body, and O_2 and O_5 are equivalent.

This discussion not only establishes a sufficient condition for equivalence, but also reveals that for certain objectives though not for others, the candidate's strategies are sensitive to how he estimates the election outcome. Whether a candidate estimates $V_1(\theta), \cdots, V_n(\theta)$ to calculate plurality, for example, or estimates $V_i(\theta)-\max_{j\neq i}\{V_j(\theta)\}$ directly, if he assumes that his error is distributed independently of θ, then O_1 and O_4 are equivalent. Thus, the candidate's model for plurality (and, of course, for vote) does not affect the location of equilibrium strategies. If the candidate's objective is to maximize the probability that his proportion of the vote exceeds some level, then the model that he adopts can affect the location of equilibrium strategies.

IV. Weakening A1, an Alternative Definition of Equivalence, and the Effect of Attitudes Toward Risk

A. Weakening Assumption A1

That maximizing expected plurality can be equivalent strategically to maximizing the probability of winning (Corollary to Theorem 4) may cause some surprise: as we show in the Introduction, objectives O_1 and O_4 often are thought to be *different* assumptions about candidates and their election goals. Perhaps this intuitive supposition arises because A1 appears to assume away the very reason for differentiating between O_1 and O_4; namely, as in Kramer's example, if ϵ_i is not distributed independently of θ, candidates might have to choose between "conservative" strategies that yield reliable pluralities, and "risky" strategies that offer the promise of a great plurality along with the danger of profound failure. Note that it is this possibility—that the degree of risk depends on the candidate's proportion of expected vote—that precludes extending Theorem 4 directly to include O_2 and O_5.

To illustrate and explore these possibilities further, suppose that two candidates compete in a plurality system election, and assume that the variance of their estimates of $V_1(\theta)-V_2(\theta)$ violates A1: it increases as the absolute value of $v_1(\theta)-v_2(\theta)$ increases. Letting f denote the density of $V_1(\theta)-V_2(\theta)$, we see that Figure 1 is a possible illustration of this example.

It appears, then, that with this dependence of the variance on the mean (in violation of A1), maximizing expected plurality and maximizing the probability of winning are not equivalent objectives. That is, using O_1 candidate 1 prefers θ'' to θ'; using O_4, he prefers θ' to θ''.

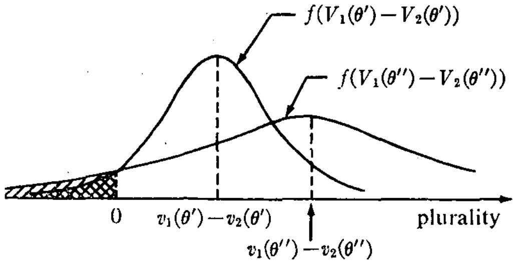

Figure 1

The shaded region of Figure 1 denotes the probability that candidate 1 loses the election given θ'' (i.e., $\Pr[V_1(\theta'')-V_2(\theta'')<0]$); the hatched region denotes the probability that candidate 1 loses given θ' (i.e., $\Pr[V_1(\theta')-V_2(\theta')<0]$). Clearly, even though θ'' gives candidate 1 a higher expected plurality than does θ', his probability of losing is greater with θ'' than with θ', i.e.,

$$\Pr[V_1(\theta'') - V_2(\theta'') < 0] > \Pr[V_1(\theta') - V_2(\theta') < 0].$$

But, the conclusion that *any* violation of A1 results in non-equivalence is unwarranted: it is possible to demonstrate that these two objectives can remain equivalent under a different set of assumptions, namely, $f(V_1(\theta)-V_2(\theta))$ is symmetric about its mean, $\lambda_i=0$, and the election is symmetric. From the definition of equilibrium, equivalence requires that

$$\begin{aligned} v_1(\theta^*) - v_2(\theta^*) &\geq v_1(\theta) - v_2(\theta) \\ &\Leftrightarrow \Pr[V_1(\theta^*) - V_2(\theta^*) \geq 0] \qquad (2) \\ &\geq \Pr[V_1(\theta) - V_2(\theta) \geq 0], \end{aligned}$$

for all $\theta=(\theta_1, \theta_2^*)$. Suppose the election is symmetric, in which case, $v_1(\theta^*)-v_2(\theta^*)=0$, and, from the symmetry of f, $\Pr[V_1(\theta^*)-V_2(\theta^*)\geq 0]=1/2$. Thus, (2) becomes,

$$\begin{aligned} 0 &\geq v_1(\theta) - v_2(\theta) \\ &\Leftrightarrow 1/2 \geq \Pr[V_1(\theta) - V_2(\theta) \geq 0], \end{aligned} \qquad (3)$$

which is simply an essential property of symmetric densities. Therefore, if the forecast error is distributed symmetrically, and if the election is symmetric, then (2) is satisfied and the conclusion of the Corollary to Theorem 4 holds, even with the violation of A1.

This example shows that A1 is not a necessary condition for equivalence between the pairs of objectives O_1 and O_4, O_2 and O_5, and O_3 and O_6. Indeed we can offer an alternative to Theorem 4. Let λ_i^* denote the minimax value of the election game to candidate i if he adopts objectives O_1, O_2, or O_3 (in the previous example, $\lambda_i^*=0$). Then,

Theorem 5: *If $f(V_1(\theta), \cdots, V_n(\theta))$ is a multivariate normal density with mean $(v_1(\theta), \cdots, v_n(\theta))$, then for $\lambda_i=\lambda_i^*$,*

(1) if $n=2$, O_1 is equivalent to O_4;

(2) O_2 is equivalent to O_5 in the special case where forecast errors are small;

(3) O_3 is equivalent to O_6.

Theorem 5 establishes its equivalences even if the variance/covariance matrix of f is a function of θ. The condition that f is a multivariate normal density guarantees that $V_i(\theta)$, $V_i(\theta)-V_j(\theta)$, and

$$V_i(\theta) \Big/ \sum_{j=1}^{n} V_j(\theta)$$

are distributed symmetrically about $v_i(\theta)$, $v_i(\theta) - v_j(\theta)$, and

$$v_i(\theta) \Big/ \sum_{j=1}^{n} v_j(\theta)$$

respectively. As the proof of Theorem 5 demonstrates, though, we can substitute for this assumption any alternative condition that yields this symmetry property.

Yet, Theorem 5 establishes equivalence between objectives O_1 and O_4 only for the two-candidate case. This is because generally $V_i(\theta) - \max_{j\neq i}\{V_j(\theta)\}$ is not symmetric for $n>2$, even if for all $j\neq i$, $V_i(\theta)-V_j(\theta)$ is symmetric. Indeed, for most reasonable assumptions about $V_i(\theta) - V_j(\theta)$, the distribution of plurality for $n>2$ does not yield an equivalence between objectives O_1 and O_4. A restrictive exception to this conjecture occurs if candidate i knows with certainty his strongest competitor, say candidate k. If this condition holds, then plurality reduces to $V_i(\theta) - V_k(\theta)$, and O_1 and O_4 are equivalent (for $\lambda_i=\lambda_i^*$) if this plurality is distributed symmetrically.

B. An Alternative Definition of Equivalence

The reader might have believed that Figure 1 illustrates a counter-example to *any* equivalence between objectives O_1 and O_4 if A1 is not satisfied. And, since the generalization is intuitively plausible, he might wonder where his intuition erred. At least one possible source of this error is that the reader had in mind a notion of equivalence unlike that of Definition 1. Specifically, consider this alternative definition of equivalence:

Definition 2: *ϕ' and ϕ'' are equivalent objective functions for candidate i if when he prefers θ_i' to θ_i'' with ϕ', he prefers θ_i' to θ_i'' with ϕ'' and, if when he prefers θ_i' to θ_i'' with ϕ'', he prefers θ_i' to θ_i'' with ϕ''.*

To illustrate the distinction between Definitions 1 and 2, suppose the electorate's preferences are distributed symmetrically and unimodally, so that if the candidates maximize expected plurality, then the mean is the spatial equilibrium. If the conditions of Theorems 1, 2, 4, or 5 are satisfied, and if we change the objective functions either to O_2 or O_4, then the mean remains the equilibrium. But, the theorems do *not* imply that if we fix one candidate's strategy at some point other than the mean, then an opponent's strategy that best exploits this vulnerability is the same for all objective functions. Such an implication is correct only if the analysis holds for the *second* definition of equivalence.

Clearly, Definition 2 implies Definition 1, but the weaker definition (1) does not imply the stronger definition (2). Figure 1 illustrates that Definition 2 *is* stronger than 1: that example shows that objectives O_1 and O_4 are equivalent under Definition 1, but not under the stronger definition, Definition 2. This raises the possibility that those who believe that the equivalence of O_1 and O_4 is a counter-intuitive proposition have in mind, probably, a definition of equivalence more nearly corresponding to Definition 2 than to Definition 1. And, as the example demonstrates, this intuitive belief is correct if the variance of f is a function of the mean of f.

Our substantial use of Definition 1 of equivalence, however, should not indicate that we consider alternative definitions uninteresting. We place an initial heavy reliance on Definition 1 because spatial theories of electoral competition concern primarily the existence and location of equilibrium strategies. And, this study adumbrates several concerns of spatial theory. As we suggest earlier, however, such equilibria may not exist; the candidate may not expect his opponents to adopt equilibria that do exist; or, in reality, the opponents may fail to adopt their equilibrium strategies. Since these eventualities are possible, equivalences and nonequivalences established under Definition 2 are more appropriate for ascertaining the strategic implications of alternative objective functions. To begin inquiry in this area, we note simply that the proof of Theorem 4 demonstrates that A1 is sufficient to render the appropriate objectives equivalent even for the stronger definition, Definition 2.

C. Attitudes Toward Risk

The preceding analysis represents elections as *n*-person, non-cooperative games and, in accordance with the usual definitions, defines the players' payoffs in terms of some measure of expected vote. Yet, the theory of games does not define the players' payoffs in terms of the physical commodities that they win, lose, or exchange (e.g., money, votes, etc.), but in terms of the players' utilities for these commodities. Hence, a consistent interpretation of the definition of candidate payoffs in terms of expected votes is: a candidate's utility is linear in votes, plurality, or proportion of the vote.[17]

That utility is linear in these measures is the simplest assumption: typically, investigators use the linear utility function if the actual functional form of a person's utility for a commodity (e.g., expected votes) is unknown. The players in most experimental games, for example, bargain for money, and a coalition's payoff *is* the money that it can secure for its members.[18] Similarly, classical microeconomic theory postulates that firms maximize profit, which implies that a firm's utility is linear with money. It seems reasonable, consequently, to impose the linearity assumption in formal models of electoral processes: any science in its infancy should ascertain first the logical consequences of simple assumptions before it examines more complex ones, especially if there is no a priori reason for complexity.

We can ascertain the sensitivity of certain equivalence findings to the assumption of linear utility, nevertheless, by examining a particular generalization. Specifically, consider the assumption that a candidate's utility is any monotonically increasing function of vote, plurality, or proportion of the vote. That is, for example, instead of assuming that a candidate's utility increases linearly with plurality, assume only that it increases: instead of assuming that O_1 is the maximization of expected plurality, assume that it is the maximization of the expected utility of plurality,

$$E[U(V_i(\theta) - \max_{j \neq i} \{V_j(\theta)\})].$$

It is simple to show that if the variances of a candidate's estimates of vote, plurality, or proportion of the vote do not depend on θ, then the equivalences that Theorems 1 and 2 establish continue to hold. But, this independence condition is restrictive (especially if it involves O_2). Therefore, in the likely event that the estimates are not independent of θ, then the theorems do not hold generally. A brief example demonstrates this possibility.

Suppose $V_1(\theta) = v_1(\theta) + \epsilon_1$, $V_2(\theta) = v_2(\theta) + \epsilon_2$, and ϵ_1 is independent of θ but ϵ_2 is not. Clearly, then, the two objectives—maximization of expected vote, O_3, and maximization of expected utility of votes, O_3'—are equivalent for candidate 1. But, consider O_1, the maximization of expected plurality, and O_1', the maximization of expected utility of plurality. Referring to Figure 2, suppose, first, that $\theta^* = (\theta_1^*, \theta_2^*)$ is an equilibrium under O_1; suppose, second, that $v_1(\theta^*) = v_2(\theta^*) = 0.5$, $\epsilon_1 \equiv 0$, and $\epsilon_2(\theta^*)$ equals $+0.1$ with probability 1/2 and -0.1 with probability 1/2 (thus, θ^* yields candidate 1 a plurality of $+0.1$ with probability 1/2, and of -0.1 with probability 1/2); and suppose, third, that for $\theta = (\theta_1, \theta_2^*)$, $v_1(\theta) = 0.49$, $v_2(\theta) = 0.51$, $\epsilon_1 \equiv 0$, and $\epsilon_2(\theta)$ equals $+0.03$ with probability 1/2 and -0.03 with probability 1/2 (thus, θ yields candidate 1 a plurality of $+0.01$ with probability 1/2 and of -0.05 with probability 1/2). The variance of plurality is not a constant for this example. The variance for θ^* is $[(-0.1)^2 + (0.1)^2]/2 = 0.01$; the variance for θ is $[(-0.03)^2 + (0.03)^2]/2 = 0.0009$.

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[17] A utility function, U, is linear in x if $U(x) = ax + b$, such that a and b are constants and a is positive. Note, then, that $E[U(x)] = aE[x] + b$, in which case if we let $a = 1$ and $b = 0$, a person's payoff, $E[U(x)]$, equals $E(x)$. Hence, the most general justification for denoting a person's payoff by $E(x)$ is to say that utility is linear in x.

[18] See, for example, William H. Riker and William James Zavoina, "Rational Behavior in Politics: Evidence from a Three Person Game," *American Political Science Review*, 64 (March, 1970), 48–60.

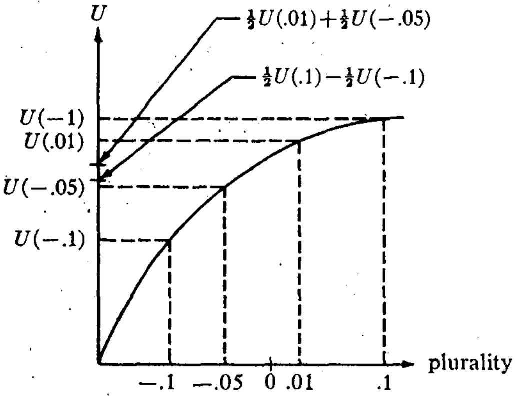

Figure 2.

Observe that as Figure 2 illustrates,

$$0 = v_1(\theta^*) - v_2(\vartheta^*) \geq v_1(\theta) - v_2(\theta) = -0.02,$$

but that

$$\begin{aligned} &E[U(V_1(\theta^*) - V_2(\theta^*))] \\ &\quad = \tfrac{1}{2}U(-0.1) + \tfrac{1}{2}U(0.1) \\ &\quad \leq E[U(V_1(\theta) - V_2(\theta))] \\ &\quad = \tfrac{1}{2}U(-0.05) + \tfrac{1}{2}U(0.01). \end{aligned}$$

If we calculate expected payoffs with linear utility functions for this example, then θ^* satisfies the equilibrium condition; if we calculate expected payoffs with a nonlinear utility function, however, then θ^* fails to satisfy the equilibrium condition. Hence, O_1 is not equivalent to O_1'. Yet, since O_3 *is* equivalent to O_3', the conditions of Theorem 1 are not sufficient to establish equivalence between O_1' and O_3'.

The logic of our example is thus: since the variance of f is a function of the mean of f, the candidate associates different degrees of risk with different expected outcomes; by assuming that the candidate's utility over the outcomes is concave (as in Figure 2), we assume that he is risk averse; therefore, he trades the expectation of a greater plurality (with high risk) for the expectation of a lesser plurality (with low risk).[19]

The implication of our example is that if we violate either the assumption of constant variance or the assumption of linear utility, then we cannot guarantee that objectives are equivalent, without the imposition of other constraints. Here, though, we can do no more than identify this problem as an opportunity—an opportunity to explain differences in the strategic choices candidates make.

V. Implications and Conclusions

We suggest briefly, throughout the preceding discussion, that the election goals a candidate adopts may have many sources: e.g., the candidate's information, his competitive environment, and formal election law. We elaborate here on these possibilities by organizing our discussion around the appropriate election system and observing variations of information, competitive environment, and custom within each system. We conclude with brief discussions of the implications of our analysis for election theory.

A. Goal Sources within Plurality Election Systems

1. *Information.* Consider, for purposes of simplicity, a two-candidate, winner-take-all election. As we suggest earlier, O_1, maximizing expected plurality, is a reasonable candidate objective for such an election. Assume, though, that the candidate's information about the effects of his strategy does not reveal completely the relationships among his strategy, his opponent's strategy, and the votes each candidate can expect to receive.

First, the candidate's information might include only reaction to his strategy, which might take the form of the amount of campaign contributions he receives, the number of people who attend his public appearances, and even the number of people who recognize him. Such a candidate, with little or no information about his opponent's fortunes, might use O_3, expected vote maximization, as a surrogate for expected plurality maximization.

Second, the candidate's information might be "better" in the sense that newspaper polls indicate partially the electorate's response to both candidates' strategies. Yet, this information reports only the respective proportions of the expected

[19] For a general discussion of risk acceptance and risk aversion in a different context, as well as for a spatial analysis of citizens' attitudes toward risk, see Kenneth A. Shepsle, "The Strategy of Ambiguity: Uncertainty and Electoral Competition," *American Political Science Review*, 66 (June, 1972), 555–568.

vote the candidates can expect to receive. Hence, O_2, maximizing proportion of the expected vote, is a reasonable surrogate for maximizing expected plurality.

Finally, the candidate might use sophisticated polling techniques to measure in depth the electorate's response to his and his opponent's strategies. Here O_1, plurality maximization itself, is a reasonable goal to postulate.

Clearly, then, the form a candidate's information assumes can be an important determinant of his election goal. Should we inquire about the source of information, though, we cannot be as sanguine. We offer the conjecture, nevertheless, that constituency characteristics might influence the nature of the candidate's information: some research indicates that candidates for more "important" offices use polls more heavily than others. "Importance" in this sense, appears to be a function of constituency size (e.g., Senate v. House) and urbanization; additionally, incumbents from "safe seats" use polls less frequently than others. Similarly, newspapers conduct polls for some (e.g., important) elections and not for others.[20] Thus, we can imagine some candidates for minor offices maximizing expected vote; other candidates for more important offices using newspaper polls and maximizing proportion of the expected vote; and still other candidates for major offices using their own complete polls and maximizing expected plurality.

Even if all other constituency characteristics (e.g., preferences) are identical, *if information* varies, then goals might vary. If the goals are not equivalent, moreover, we can expect to find variations in strategy (including policy positions) for which we cannot otherwise account.

2. *Sequential elections.* Beyond variations of information, we can postulate a number of variations in the candidate's strategic environment that prompt the adoption of different goals. Specifically, consider a candidate who must win more than one election (e.g., a primary and general election) to win office in a plurality system. We can divide his problem, perhaps artificially, in three sequential time segments.

First, to make a "respectable showing," to "scare off" marginal opposition, and to give his party the impression that his candidacy is viable, the office seeker might devote the *pre-primary* phase of his campaign to maximizing support among a limited group of activists. These people contribute time, money, and endorsements: among these activists, the candidate might adopt strategies that would maximize expected vote.

Second, to continue the respectability and viability of his campaign in the *primary* phase of his candidacy, the office seeker might adopt strategies to maximize his proportion of the expected primary vote. The incentive to adopt O_2 derives here from three sources: first, the presence of many candidates might cloud the meaning of expected plurality; second, and more important, the party leaders seem to regard proportion as more important in contests of several elections with different winners; third, referring to information variations, the primaries are reported in proportion, as are the polls that predict the results.

Third, should information be sufficient, it is reasonable to assume that in the *general election* phase of his campaign, the candidate seeks election strategies to maximize his expected plurality. As our discussion of information suggests, this is true especially of major contests: those that have primary elections to begin with.

Even should the preferences and goals of all three groups coincide—those in the pre-primary, primary, and general election phases of the campaign—*ceteris paribus*, if the contests in each campaign phase are not symmetric, or are not zero-sum-like in expected vote, then the candidate has nonequivalent objectives in different phases of his campaign.[21] Hence, his policy positions can change in a relatively short period of time, independent of the preferences of the electorates.

3. *Quotas.* While information and the sequential nature of many contests may affect the candidate's choice of goals, the presence of some quota-like level can affect this choice also. Unlike information and elections in sequence, however, quotas involve variations in λ.

The first such quota, of course, is O_4, maximizing the probability that plurality exceeds some level, λ. And, usually, we set this level at $\lambda = 0$, as we suggest earlier, to represent winning in plurality system elections. Several other levels of λ for O_5 and O_6 as well as O_4 might emerge, though, and we should consider them.

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[20] For a discussion of information sources and of polling by candidates, see John W. Kingdon, *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1968), pp. 90–93. See also a series of articles devoted to polling in campaigns, in *Public Opinion Quarterly,* 17 (Spring, 1973). They include: Morris Janowitz, "Political Polling," 1–2; Louis Harris, "Polls and Politics in the United States," 3–8; Mark Abrams, "Public Opinion Polls and Political Parties," 9–18; and Martin Meadows, "Public Opinion Polls and the 1961 Philippine Election," 19–27. These references support our conjectures in a limited manner.

[21] For a discussion of the effects on a strategy if party members and the electorate hold dissimilar preferences and goals, see Peter H. Aranson and Peter C. Ordeshook, "Spatial Strategies for Sequential Elections," and James Coleman, "The Positions of Political Parties in Elections," in *Probability Models of Collective Decision Making,* ed. Richard G. Niemi and Herbert F. Weisberg (Columbus, Ohio: Charles E. Merrill, 1972), pp. 298–357.

First, suppose a Republican mayorality candidate in an overwhelmingly Democratic city believes that he has no chance to win the election. But, the candidate's reason for running is not to win the mayorality race: he seeks his party's gubernatorial nomination in the next election. He knows that if he can come closer to his Democratic mayorality opponent than any previous Republican, the nomination is his. Perhaps statewide offices are closely contested; if the candidate can demonstrate to Republican party leadership that he can attract more Democrats than any preceeding Republican nominee, then he wins the gubernatorial nomination. Finally, suppose that the smallest Democratic mayoral plurality is 50,000 votes of an electorate of 1,000,000. Thus, the Republican candidate might seek to maximize,

$$\phi_i(\theta) = \Pr\left[V_i(\theta) - V_j(\theta) \geq -\frac{50{,}000}{1{,}000{,}000}\right].$$

Theorem 3 and its Corollary state that if A1 holds, the Republican adopts identical strategies if he maximizes expected plurality, maximizes the probability of winning ($\lambda_i = 0$), or maximizes the probability of coming within 50,000 votes of his opponent. This equivalence between O_1 and O_4 suggests that candidates who expect to lose can adopt strategies identical to those whose projections are not so dire. Conversely, if A1 fails to hold, or if $\lambda_i \neq \lambda_i^*$, then the quota level can create strategic variation.

Second, we observe that it is the custom of presidential nomination candidates to set some proportion of the vote in primary elections below which their chances for nomination are "derailed," and above which, as we mention earlier, their "showing is respectable." We note, also, that such goals find expression in vote proportion, rather than in vote or plurality, perhaps because people can grasp meaning from vote proportion more easily than from raw vote. We do not know if such figures are meaningful or if they are largely for public consumption. We speculate, nevertheless, that to the extent that these figures have meaning, candidates who publish them operate in accordance with O_5 and λ_i is the candidate's minimum per cent. Stated differently, the candidate who says, "I need 35 per cent of the vote to stay in the race," maximizes the probability that $\lambda_i \geq .35$.[22]

If O_2, maximizing proportion of the expected vote, and O_5, maximizing the probability that proportion exceeds some level, are equivalent—i.e., if the candidate's forecast error in proportion meets the conditions of A1—then the level of λ has no strategic significance. If, however, O_2 and O_5 are not generally equivalent, or if they are equivalent only for a specific value of λ (as in Theorem 5), then the candidate's strategy might vary with the objective he adopts, as well as with the value he assigns to λ.

Third, precinct leaders, ward leaders, and even local office seekers might adopt strategies to maximize O_6, the probability that vote exceeds some level. This goal can have a *raison d'être* from the demands of party leaders who establish vote quotas in each district, if the leaders believe that meeting such quotas can generate levels of success sufficient to vanquish any statewide opposition.

Alternatively, the leader of a slate may set such a level for himself, knowing that the coattails effect is great enough to bring his party widespread victories. And, as with the preceding examples of quotas, the form of the candidate's forecast error and, therefore, the equivalence or nonequivalence of O_3 and O_6 can affect the strategy the candidate adopts.[23]

B. Goal Sources within Proportional Representation Election Systems

We offer the conjecture that *many of the same influences on goal selection that operate in plurality election systems operate also in proportional representation election systems.* First, limitations on a party's information can dictate its election goals. Second, primary contests to "make the party list" can generate different goals for the party's candidates at different phases of their pursuit of office. Third, the exploitation of geographic (regional) strengths and weaknesses might compel the setting of levels of success by vote in identifiable areas. Hence we cannot say, simply, that "plurality election candidates do this and proportional representation election candidates do that." Additionally, we can isolate sources of goal variations in proportional representation systems that are not present unambiguously in plurality systems.

1. *Formal and informal rules.* A minor rule that is present in most proportional representation systems is the exclusion clause (*Sperrklausel*), which sets a proportion of the vote each party must win to receive parliamentary representation. Though the "law of cubic proportions" seems to have a

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[22] There is the additional possibility, given the Democratic party's proportional representation allocations of convention seats, that a Democratic presidential nomination candidate will adjust λ_i depending on the number of his already committed delegates, his expectations for future primaries, and the numerical strength of a state delegation. We thank Edwin Fogelman for pointing out this possibility to us.

[23] Summarily, we observe that candidates who maximize according to O_3, O_4, or O_6 might adopt one or another of these goals because of the information or sequential election effects we discuss previously. Here, we see the acute importance of finding nonequivalences as well as equivalences among election goals.

similar effect in legislatures drawn from single-member districts, the exclusion clause is a formal requirement. Clearly, major parties need not consider the threshold percentage (though they act as if they do if, for example, O_2 and O_5 are equivalent). Minor parties close to the threshold proportion, though, must be cognizant of their precarious situation; if O_2 and O_5 are not equivalent, the level of the exclusion clause creates an incentive, *ceteris paribus*, for minor parties to adopt strategies that differ from those of larger parties that maximize proportion of the expected vote. If A1 holds, or if Theorem 5 applies, then the exclusion clause has no strategic implications and, *ceteris paribus*, we cannot distinguish the parties by virtue of their size.

A second and more important variation in the adoption of goals derives from the nature of extralegal coalition formation rules. If, for example, government coalitions contain traditionally the party with the greatest number of seats (as in Israel or Chile), we can expect the major parties to seek strategies that maximize either their expected plurality, or the probability that their plurality is greater than zero.

2. *Competitive environment*. It is reasonable to suppose that the goals of party members in proportional representation systems focus on proportion rather than plurality. To offer one illustration of this focus, consider such an election and suppose, further, that a government consists of a majority coalition of legislative parties and that an undistorted proportional representation formula operates, mapping directly from proportion of the vote to proportion of the legislative seats. A party, then, adopting O_5, might set $\lambda_i = 1/2$, which is to say that it seeks strategies that maximize the probability that it governs alone.

Hence, O_5 can represent the goal of maximizing the probability of maintaining a parliamentary majority. Similarly, a party whose leaders believe that it has no chance to capture a legislative majority, might satisfice by seeking to maximize simply the probability that it retains as many seats as it holds currently. If s_i equals the number of seats it holds currently, and if S equals the total number of seats in the legislature, then, for the satisficing party, $\lambda_i = s_i/S$. Paralleling our discussion of the strategic effects of the exclusion clause, if O_2 and O_5 are equivalent goals for all values of λ, parties that seek to maximize the probability of a legislative majority and satisficing parties are strategically indistinguishable. If, however, the party leaders' estimate of proportion derives from estimates of each $v_i(\theta)$ (i.e., proportion generally is not distributed independently of θ) and if $\lambda_i \neq \lambda_i^*$, then we can expect to find real strategic differences between the satisficing and maximizing parties.

C. Conclusions

The preceding conjectural exercise reminds us again of Froman's observation that, "campaign strategies . . . vary considerably, depending upon the personal motivations and purposes of the parties and candidates."[24] Yet in spite of the abundance of possible "motivations and purposes" (i.e., objectives), there are at least two important conclusions we can draw from the preceding discussion.

First, we conclude that there is no unambiguous relationship between a given election system on the one hand and campaign objectives and strategies (leading to public policy) on the other hand. Indeed, differences in campaign goals and strategies appear to be as much a product of variations of information and competitive environment as they are a product of the structure of a given election system.

Clearly, for example, if our conjectures about the determinants of candidate objectives are correct, then we can expect frequently to find two candidates in vastly different election systems using *identical* objective functions. That is, for example, we can expect to find any of our six objective functions in use either in proportional representation elections or in plurality elections. And, we can attribute differences in objectives that do occur to varying levels of information, quotas, sequential procedures, and stupidity.

Just as clearly, even if candidates do adopt different objectives for different election systems, this analysis shows that these objectives might be equivalent, in which case we cannot attribute strategic variations to variations in election laws. Rather, the analysis shows that equivalence and nonequivalence among these objectives are the result not of the election system but of such variables as election symmetry, the characteristics of forecast errors, and the levels of expectations and quotas. Again, even if different systems do lead to the adoption of different objective functions, other factors may enter, thus negating any variations in strategic incentive (e.g., policy advocacy) we might otherwise attribute to the differences in election systems.

This conclusion is not as depressing as it may seem. We do not argue that the strategic (and hence, policy) implications of a given election system or rule are indeterminate. Rather, we argue that political scientists have just *begun* to understand these implications. Such an understanding, furthermore, requires a knowledge of many other things besides the abstract election system considered *in vacuo*.

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[24] See Lewis A. Froman, Jr., "A Realistic Approach to Campaign Strategies and Tactics," in *The Electoral Process*, ed. Jennings and Ziegler, p. 2.

Second, we conclude that the analysis of elections *per se* (e.g., spatial theory), as well as the study of constituency influence on legislators may use assumptions that do not permit simple inferences to real world choices. This study shows for example, that the results of earlier formal models of electoral competition are more nearly general than we might have supposed if certain (restrictive) assumptions are satisfied. It shows, also, that nonequivalence among campaign objectives may limit generality considerably. Hence, further analysis must proceed with care in treating these objectives and the relevant equivalences. Similarly, the analysis of constituency influences on a legislator assumes implicitly that the legislator seeks to maximize some electoral outcome. But, the functional form of that outcome may affect profoundly the legislator's voting choices in the legislature. Hence, some care in specifying the legislator's objective is necessary.

Appendix

Proof of Theorem 1: Let $\theta^* = (\theta_1^*, \theta_2^*, \cdots, \theta_n^*)$ and $\theta = (\theta_1, \theta_2^*, \cdots, \theta_n^*)$. In accordance with footnote 6, it is sufficient to show that, regardless of the objectives of other candidates: (1) if θ^* is an equilibrium for candidate 1 under O_1, it is an equilibrium under O_2 and O_3, (2) if θ^* is an equilibrium under O_2, it is an equilibrium under O_1 and O_3, and (3) if θ^* is an equilibrium under O_3, it is an equilibrium under O_1 and O_2. Stated differently, we must show that

$$\begin{aligned} &(1) \quad (A1) \Rightarrow (A2), (A3) \\ &(2) \quad (A2) \Rightarrow (A1), (A3) \\ &(3) \quad (A3) \Rightarrow (A1), (A2) \end{aligned}$$

where

$$v_1(\theta^*) \geq v_1(\theta) \qquad (A1)$$

$$v_1(\theta^*) - \max_{j \neq 1} v_j(\theta^*) \geq v_1(\theta) - \max_{j \neq 1} v_j(\theta) \qquad (A2)$$

$$v_1(\theta^*) \Big/ \sum_{i=1}^{n} v_i(\theta^*) \geq v_1(\theta) \Big/ \sum_{i=1}^{n} v_i(\theta). \qquad (A3)$$

First, observe that,

$$v_1 \Big/ \sum_{i=1}^{n} v_i = \frac{1}{1 + \sum_{j \neq 1} v_j / v_1}$$

so that proportion of the vote is monotonically increasing with

$$v_1 \Big/ \sum_{j \neq 1} v_j.$$

That is, proportion and

$$v_1 \Big/ \sum_{j \neq 1} v_j$$

are equivalent—(A4) is equivalent to (A3) where

$$v_1(\theta^*) \Big/ \sum_{j \neq 1} v_j(\theta^*) \geq v_1(\theta) \Big/ \sum_{j \neq 1} v_j(\theta) \qquad (A4)$$

(1) From (A1) and from the conditions (a) and (b) of T1, $v_j(\theta^*) \leq v_j(\theta)$, $j \neq i$, Thus,

$$\max_{j \neq 1} v_j(\theta^*) \leq \max_{j \neq 1} v_j(\theta)$$

and

$$\sum_{j \neq 1} v_j(\theta^*) \leq \sum_{j \neq 1} v_j(\theta),$$

and (A2) and (A3) clearly hold.

(2) Suppose that $(A2) \nRightarrow (A1)$, i.e., suppose that $v_1(\theta^*) < v_1(\theta)$. Then for (A2) to hold, it must be the case that $\max_{j \neq 1} v_j(\theta^*) < \max_{j \neq 1} v_j(\theta)$ which means that for at least one j, $v_j(\theta^*) < v_j(\theta)$. From condition (c) of the theorem, however, this implies that $v_1(\theta^*) > v_1(\theta)$ which contradicts our supposition. Thus, $v_1(\theta^*) \geq v_1(\theta)$, i.e., $(A2) \Rightarrow (A1)$. That $(A2) \Rightarrow (A3)$ follows now from the fact that $(A2) \Rightarrow (A1) \Rightarrow (A3)$.

(3) Suppose that $(A3) \nRightarrow (A1)$, i.e., suppose that $v_1(\theta^*) < v_1(\theta)$. Then for (A3) to hold, it must be the case that

$$\sum_{j \neq 1} v_j(\theta^*) < \sum_{j \neq 1} v_j(\theta)$$

which means that for at least one j, $v_j(\theta^*) < v_j(\theta)$. But again, from condition (c) this implies that $v_1(\theta^*) > v_1(\theta)$ which contradicts our supposition. Thus, $v_1(\theta^*) \geq v_1(\theta)$, i.e., $(A3) \Rightarrow (A1)$. That $(A3) \Rightarrow (A2)$ follows from the fact that $(A3) \Rightarrow (A1) \Rightarrow (A2)$.

Proof of Theorem 2: Considering candidate 1 and assuming that θ^* is an equilibrium strategy, if the election is symmetric, and if $\phi_1 \equiv O_1$,

$$v_1(\theta^*) - v_j(\theta^*) = 0, \qquad j = 2, 3, \cdots, n$$

Thus, for a candidate who maximizes expected plurality, (A2) becomes

$$0 \geq v_1(\theta) - \max_{j \neq 1} \{v_j(\theta)\} \qquad (A5)$$

Similarly, if θ^* is an equilibrium strategy pair with $\phi_1 \equiv O_2$ and if the election is symmetric

$$v_i(\theta^*) \Big/ \sum_{j=1}^{n} v_j(\theta^*) = 1/n \qquad \text{for all } i$$

Thus, for a candidate who maximizes proportion of the expected vote, (A3) becomes

$$1/n \geq v_1(\theta) \Big/ \sum_{j=1}^{n} v_j(\theta). \qquad \text{(A6)}$$

We now show, in accordance with footnote 6, that if θ^* satisfies (A6), it satisfies (A5), i.e., if θ^* is an equilibrium for a candidate who maximizes proportion of the expected vote, it is an equilibrium for a candidate who maximizes expected plurality, *ceteris paribus*. First, dropping the argument θ since it is carried throughout, we can write from (A6)

$$\frac{1}{n} \geq v_1 \Big/ \sum_{j=1}^{n} v_j = v_1 \Big/ \left[v_1 + \sum_{j=2}^{n} v_j \right].$$

But, since

$$v_1 \Big/ \left[v_1 + \sum_{j=2}^{n} v_j \right] \geq v_1/[v_1 + (n-1) \max_{j \neq 1} v_j],$$

then clearly

$$\begin{aligned} \text{(A6)} &\Rightarrow \frac{1}{n} \geq v_1[v_1 + (n-1) \max v_j] \\ &\Rightarrow v_1 + (n-1) \max v_j \geq n v_1 \\ &\Rightarrow -(n-1) v_1 + (n-1) \max v_j \geq 0 \\ &\Rightarrow v_1 - \max v_j \leq 0 \Rightarrow \text{(A5)} \end{aligned}$$

or simply, (A6)⇒(A5). We now show that if the election is strongly symmetric and if θ^* satisfies (A5), θ^* satisfies (A6). Clearly we can reverse our previous steps and show that

$$\text{(A5)} \Rightarrow \frac{1}{n} \geq v_1/[v_1 + (n-1) \max_{j \neq 1} v_j].$$

Note now that if $v_2 = v_3 = \cdots = v_n$—the fundamental property of a strongly symmetric election—then

$$(n-1) \max_{j \neq 1} v_j = \sum_{j=2}^{n} v_j.$$

Thus, (A5)⇒(A6) and our proof is complete.

Proof of Theorem 3: That $\theta^* = (\theta_1^*, \theta_2^*)$ is an equilibrium for O_1, for candidate i requires that

$$\frac{\partial}{\partial \theta_i}[v_i(\theta) - v_j(\theta)] = \frac{\partial v_i(\theta)}{\partial \theta_i} - \frac{\partial v_j(\theta)}{\partial \theta_i} = 0 \quad \text{(A7)}$$

for $\theta = \theta^*$. Similarly, for θ^* to be an equilibrium for O_2 for candidate i requires that

$$\begin{aligned} &\frac{\partial}{\partial \theta_i}\left[\frac{v_i(\theta)}{v_i(\theta) - v_j(\theta)}\right] \\ &\quad = v_j(\theta) \frac{\partial v_i(\theta)}{\partial \theta_i} - v_j(\theta) \frac{\partial v_j(\theta)}{\partial \theta_i} = 0, \end{aligned} \qquad \text{(A8)}$$

for $\theta = \theta^*$. To prove necessity (only if), suppose the election is not symmetric, i.e., $v_i(\theta^*) \neq v_j(\theta^*)$. Then, excluding the degenerate case of $v_j(\theta^*) = v_i(\theta^*) = 0$; (A7) and (A8) are satisfied only if $\partial v_i(\theta)/\partial \theta_i = 0$ for $\theta = \theta^*$ (in which case $\partial v_j(\theta)/\partial \theta_i$, $\theta = \theta^*$ must also equal zero). But this condition implies that O_1 and O_2 are equivalent to O_3 for candidate i. Thus, if the election is not symmetric, O_1 and O_2 are equivalent only if O_1 and O_2 are equivalent to O_3. Suppose now that O_1 and O_2 are not equivalent to O_3 (i.e., $\partial v_i(\theta)/\partial \theta_i \neq 0$ for $\theta = \theta^*$). Then clearly (A7) and (A8) can be satisfied simultaneously at $\theta = \theta^*$ only if $v_i(\theta^*) = v_j(\theta^*)$. Thus, if O_1 and O_2 are not equivalent to O_3, O_1 and O_2 are equivalent only if the election is symmetric.

The sufficiency of (i) follows from Theorem 2 since 2-candidate symmetric elections are also strongly symmetric. The sufficiency of (ii)—assuming that second order conditions are appropriately satisfied—follows simply from the fact that if we substitute $\partial V_i/\partial \theta_i = 0$ in (A7) and (A8), both expressions reduce to the same condition, $\partial V_j(\theta)/\partial \theta_i = 0$, $\theta = \theta^*$.

Proof of Theorem 4: Assume that $X(\theta)$ is a random variable, where $X(\theta) = x(\theta) + \epsilon$ and $E[X(\theta)] = x(\theta)$. Clearly, the theorem follows if, when $x(\theta)$ is either $v_i(\theta)$ or $v_i(\theta) - \max_{j \neq i}\{v_j(\theta)\}$, the following implication is true.

$$x(\theta'') \geq x(\theta') \Leftrightarrow \Pr[X(\theta'') \geq \lambda] \geq \Pr[X(\theta') \geq \lambda]$$

where θ'' and θ' are any two strategy vectors. (Note also that if this implication is true, Theorem 4 holds for both definitions of equivalence since θ'' is not presumed to be an equilibrium.) Note now that if $X(\theta) - x(\theta)$ is distributed independently of θ, i.e., if only the mean of the distribution of $X(\theta)$ varies with θ (assumption A1), an increase in $x(\theta)$ implies and is implied by an increase in the probability that the random variable $X(\theta)$ exceeds some constant value. For $X(\theta) = V_i(\theta)$—for objectives O_3 and O_6—the Theorem is obviously true. To establish equivalence between O_1 and O_4 it is sufficient to point out that

$$E[\max_{j \neq i}\{V_j(\theta)\}] = \max_{j \neq i}\{v_j(\theta)\}$$

so that

$$V_i(\theta) - \max_{j \neq i}\{V_j(\theta)\} - v_i(\theta) - \max_{j \neq i}\{v_j(\theta)\}$$

is distributed independently of θ under A1.

Proof of Theorem 5: Consider the random variable $X(\theta)=x(\theta)+\epsilon(\theta)$, where $E[X(\theta)]=x(\theta)$. What we wish to prove is that

$$x(\theta^*) \geq x(\theta) \Leftrightarrow \Pr[X(\theta^*) \geq x(\theta^*)] \geq \Pr[X(\theta) \geq x(\theta^*)]$$

where $X(\theta)$ is either vote, plurality, or proportion of the vote. Assume first that $X(\theta)$ is distributed symmetrically about its mean, $x(\theta)$. Then clearly, $\Pr[X(\theta^*) \geq x(\theta^*)] = 1/2$. The implication $\Rightarrow$ follows now since, if $x(\theta^*)$ is greater than or equal to the mean of $X(\theta)$, $x(\theta)$, and if the distribution of $X(\theta)$ is symmetric about its mean, $\Pr[X(\theta) \geq x(\theta^*)] \leq 1/2$. The implication $\Leftarrow$ follows with a similar argument.

Clearly, now, if $X(\theta)=V_i(\theta)$, $X(\theta)$ is distributed symmetrically if $f(V_1, \cdots, V_n)$ is multivariate normal. Similarly, $X(\theta)=V_i(\theta)-V_j(\theta)$ is distributed symmetrically since the sum (or difference) of two jointly normally distributed variables is normal. For proportion of the vote, note that

$$\frac{V_i(\theta)}{\sum_{j=1}^{n} V_j(\theta)} = \frac{v_i(\theta)+\epsilon}{\sum_{j=1}^{n} v_j(\theta) + \sum_{j=1}^{n} \epsilon_j}$$

$$= \frac{v_i(\theta)}{\sum_{j=1}^{n} v_j(\theta)} + \frac{\epsilon_i \sum_{j\neq i} v_j(\theta) - v_i(\theta) \sum_{j\neq i} \epsilon_j}{\left[\sum_{j=1}^{n} v_j(\theta)\right]^2} + 0\left[\epsilon_i^2, \left(\sum_{j\neq i} \epsilon_j\right)^2\right]$$

by the Taylor series expansion. If

$$\epsilon_i \text{ and } \sum_{j\neq i} \epsilon_j$$

are jointly normal (the assumption of the theorem), the second term of this expression is normal and hence symmetric. Thus, using the linear approximation, proportion is distributed symmetrically.

With respect to our failure to include O_2 and O_5 in Theorem 4, however, note that even if all ϵ's are independent of θ, the second term of the previous expression is not independent of θ. And this term is the stochastic variable of the linear approximation. Thus, A1 is not in general sufficient to establish equivalence between O_2 and O_5.

Helvétius and the Roots of the "Closed" Society*

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"We have found happiness," say the last men, and they blink thereby. [*Thus Spake Zarathustra*]

Contemporary scholars, alarmed by the erosion of personal liberties in the twentieth century, have turned increasingly to the past, seeking to identify the origins of the manipulative state. The problem is not, of course, susceptible of facile isolation, either geographically or intellectually. All modern nations, including the most liberal, have evinced an attenuation of the belief that the individual is possessed of an inviolable realm of private experience, a dimension of selfhood with which society and the state must not interfere. And the reasons for this decline are far too complex to fall within the competency of a single discipline—indeed, it is a problem that has exercised virtually all the social sciences as well as the humanities. But some students of intellectual history, inspired by Karl Popper and J. L. Talmon, have tended to ignore both the pervasiveness and the complexity of this trend, attributing it almost exclusively to an intellectual style: a specific set of doctrines or a mentality.[1] Instead of viewing the problem of increasing political manipulation as a rampant force, they confine it to one of two polar sociopolitical models. On the one hand, there is the "closed" or "totalitarian" society which embodies all the repugnant qualities of modernity; it is here that the hapless individual has surrendered his liberty, his very identity, to a Big Brother. The other model, the British-inspired "open" or "liberal" society, embodies all those values on behalf of which we protest against Big Brother: a maximum of personal liberty combined with the utmost veneration for individuality, and a pragmatic, humane conception of politics. Since Talmon and Popper view these models as having their source in ideas rather than social forces, their main concern is to isolate the doctrines that have engendered them: to distinguish a totalitarian style of thinking from its liberal counterpart.

Talmon and Popper disagree concerning the details of these two styles of thinking, and thus it is a simple matter to dispatch a number of their claims by a process of mutual cancellation.[2] But such criticism lacks force, for in tacitly accepting their framework, it sidetracks the crucial question: whether speculation about human affairs is governed by an inexorable logic of political consequences; whether there exist certain broad perspectives or orientations —certain 'paradigms'—which impel men willy-nilly to pathological extremes in their political views, apart from individual intention or historical circumstance.

It will be my purpose in this essay to suggest that Talmon and Popper provide an erroneous answer to this question. Helvétius is uniquely appropriate for my task, since he is indubitably one of the most manipulative of thinkers in his conception of politics. Other putative totalitarian ideologists have been vindicated, but Helvétius was so explicit in repudiating the ideal of individual autonomy as to be indefensible. His own contemporaries were as repelled by his views as is Talmon.[3] Furthermore, Helvétius brings a systematic or comprehensive perspective into his political speculation. Thus his example provides us with a common point of departure: we can agree that his ideas constitute a radical denial of personal liberty; the question is whether we can isolate a syndrome of singularly malevolent attitudes or doctrines underlying his thought.

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[1] Karl R. Popper, *The Open Society and Its Enemies*, 2 vols. (New York: Harper & Row, 1963), and J. J. Talmon, *The Origins of Totalitarian Democracy* (New York: Frederick A. Praeger, 1961). Popper and Talmon are by no means unique in their approach. For other studies in the same vein, see T. L. Thorson's *Plato: Totalitarian or Democrat?* (Englewood Cliffs: Prentice-Hall, 1963), John W. Chapman's *Rousseau—Totalitarian or Liberal?* (New York: AMS Press, 1968). Chapman's book includes a résumé of the literature concerning Rousseau's putative totalitarian leanings. Also see Hans Kelsen's "Absolutism and Relativism in Philosophy and Politics," in *What is Justice? And Other Essays* (Berkeley: University of California Press, 1957).

[2] Talmon, for instance, describes the totalitarian mentality as ultra-rationalist (*Origins*, p. 4), whereas for Popper it is irrational (*Open Society*, I, 167). Similarly, Talmon contrasts the empirical method of liberal society with the totalitarian penchant for dealing in abstractions (*Origins*, pp. 2–4). Popper, on the other hand, suggests that the "closed" mentality is quite compatible with scientific empiricism (*Open Society*, I, 162) and attributes abstractness to the open, rather than closed society. The latter is for him "still a concrete group of concrete individuals, related to one another not merely by such abstract social relations as division of labour and exchange of commodities, but by concrete physical relationships such as touch, smell, and sight (*Ibid.*, p. 173). Because the open society has lost its organic character, it gradually becomes an "abstract society": "It may, to a considerable extent, lose the character of a concrete or real group of men, or of a system of such real groups" (*Ibid.*).

[3] See D. W. Smith's *Helvétius: A Study in Persecution* (Clarendon: Oxford University Press, 1965).

My argument is that we cannot. I will attempt to demonstrate that the general perspective which informed Helvétius' thought was simply the framework of early-modern science, as it was understood in France. It was the same philosophy which served his unequivocally humanitarian contemporaries, such as Voltaire and Diderot, as well as their predecessors. His political conclusions resulted not from evil attitudes or doctrines, but rather from his attempt to resolve a problem engendered within the new science, a fundamental dilemma in French scientific thinking concerning the relationship of the individual to society and the state. Here again, Helvétius is in accord with his humanitarian contemporaries, for they were equally concerned with this problem. Moreover, I will demonstrate that the new science which inspired Helvétius and his fellow-philosophes did not constitute a narrow school of elitist intellectuals, severed from all connection with social reality,[4] and fired by a quasi-religious mood.[5] It was an orientation that had deep historic roots in France, both in its norms and its problems. Indeed, the hedonism on which Helvétius based his utilitarianism was—at least in its pristine form—more of the nature of sociological generalization than abstract dogma, for hedonism, as we will see, appeared first as a pervasive cultural ideal and was only subsequently accepted by philosophy and science.

But the Talmon-Popper dichotomy beclouds and overrides so many broad issues concerning the roles of political speculation and history as to preclude challenge until at least some of these issues have been salvaged. Since both authors develop their views about political thinking in historical contexts, we legitimately expect to find carefully reasoned arguments regarding the political significance of thinking and of history. Yet we do not encounter such arguments, or even clear assumptions; instead we find paradoxes. Based on a skeptical attitude concerning the creative function of cognition,[6] their dichotomy nonetheless ascribes greater force to thought than do all but the most extreme of rationalist systems. Ideas cease to be tools, subject to (and thus limited by) the vagaries and ambivalencies of human purpose; they form instead a juggernaut, a compelling presence that sweeps across human affairs on the winds of logic.[7] From this paradox emerges an attitude of unremitting hostility to the speculative impulse in politics: one can legitimately think about the political realm only in the most restricted, *ad hoc* fashion, approaching policy alternatives in the empirical, pragmatic manner of the "piecemeal social engineer."[8] To aspire to more is to lapse into the doctrinaire spirit characteristic of totalitarian styles of thought.

This attitude, reflecting, no doubt, a pervasive modern crisis of confidence regarding political "vision," might serve as a useful corrective were it restricted to the function of chastening intellectual *hubris*. But its prescriptions extend beyond mere retrenchment. It conveys an exhaustive conception of "legitimate" political thinking that precludes a number of important intellectual demands westerners have perennially levied upon their politics. If, for instance, we accept the dictum that the desire to absorb political knowledge into a comprehensive scheme is unique to totalitarian-prone thinkers,[9] then we must dismiss not only a major problem of eighteenth-century thought, but most of our broader tradition of political speculation as well. We must even condemn the very urge to impose intellectual coherence upon politics, since the desire for comprehensiveness has almost always attached to the quest for political knowledge. Plato's analogy between the political realm and the shadowy cave reflects an attitude so deeply entrenched as to remain virtually unchallenged; political knowledge, subject as it is to innumerable vicissitudes that do not assail the more abstract domains of thought, has been viewed as being among the least certain, the most contingent forms of knowledge. Consequently, political thinkers have always sought to ground their inquiry in the more "fundamental" or "real" dimensions of experience, such as metaphysics, religion, or science.[10] Indeed, this tendency provides us with one measure of constancy amid the historical flux of speculation, uniting Plato, Hobbes, and Locke with the most methodologically sophisticated of contemporary behavioralists, thoroughly imbued as the latter are with the norms of modern science. Thus to condemn this systematizing urge is to cut across all distinctions, to set aside all the arguments and theories that have occupied students of politics over the ages.

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[4] This is the thrust of Popper's condemnation of Plato. See *Open Society*, I, 169, for instance.

[5] See Talmon's discussion of "political messianism" in *Origins*, pp. 3ff, as well as my first section, below.

[6] See Popper's discussion of the poetic impulse in political speculation, *Open Society*, I, 165ff.

[7] Although Talmon claims to be treating attitudes, rather than ideas, his discussion centers in fact upon the latter. See below, first section and *passim*. Also see Alfred Cobban's excellent critique of Talmon's notion of "influence" in his *In Search of Humanity: The Role of the Enlightenment in Modern History* (New York: George Braziller, 1960), pp. 183ff.

[8] *Open Society*, I, 1. Popper has discussed his conception of piecemeal engineering in greater detail in his *The Poverty of Historicism* (New York: Harper and Row, 1961), pp. 58–71. Cf. *Origins*, p. 1.

[9] *Origins*, p. 2.

Nor can the Talmon-Popper scheme tolerate the quest for coherence in its ethical form: the attempt on the part of the individual to preserve his ethical integrity (indeed his sanity) amid the chaotic imperatives of the political realm. We are forced to view this quest—obviously an important one for the post-Reformation West—less as an intellectual problem than as a prodrome. In the first place, the questions raised by this search inevitably transcend the *ad hoc*, the strictly political: How is the good (prudent, self-interested) man to hold the state ethically accountable? How is he to circumscribe his obligations to the state? What standards may he invoke? Since these questions rest on a demand for the reconciliation of the various sectors of one's conceptual universe—an insistence that the political be subsumed by the most "real" or "immediate"—they must be rejected as not truly political.

Moreover, in acknowledging the significance of this ethical problem, we are admitting the importance of historical circumstance for political thought, inasmuch as the problem of ethical standards acquires urgency only under certain conditions—only when the regnant certainties, having lost their persuasive force, are not readily succeeded by a new scheme. Of course it would seem to most that in thus acknowledging the importance of historical vicissitude, we are only trumpeting the truistic. Yet within the Talmon-Popper scheme, history occupies only the most dubious status. Here again we encounter the same paradoxical, deprecatory attitude we found underlying the notion of reason. Adduced as a framework for historical analysis, the "open"-"closed" dichotomy is radically unhistorical in implication, for if there exist archetypical "good" and "evil" styles of thinking, then historical circumstance becomes irrelevant. History, indeed, becomes an encumbrance in our effort to capture the real significance of ideas; in immersing us in the contingencies of time and place, it tends to qualify—and ultimately to dissolve—our archetypes, to submerge dichotomies in a factual quagmire.

It is impossible to refute this ahistorical bent, since it resolves itself into an attitude, a predilection for which no probative canons exist. To challenge it with a specific instance of the influence of historical circumstance upon political thinking (seemingly the obvious procedure) is to gainsay it at the outset and, consequently, to embark upon an argument without a common ground. Yet while impossible, refutation is perhaps also unnecessary, for even if we do not impose the requirement of verifiability upon scholarly attitudes, they still are not free of all constraint: we do expect them, at least, to be seasonable. Thus it seems sufficient to observe, by way of tentative justification for my question-begging procedure, that timeless perspectives ill-suit the temper of the times. To identify an antipathy toward Clio underlying a modern approach to intellectual history would appear in itself to effect a telling criticism.

"Political Messianism" and Eighteenth-Century France

The tendency to hypostatize ideas and to ignore history is crucial to Talmon's argument against the philosophes. This is apparent at the beginning, in his discussion of "political messianism." Here he adumbrates an orientation that would seem—at least in comparison with British libertarian views—most undesirable; consequently, to demonstrate that the philosophes were guided by this orientation is, for him, to expose them at the outset, to unmask their quandaries, revealing them as symptoms of a pathological mentality. The main features of this orientation appear to be: a desire to seek out a "new unitary principle of social existence"[11] and to "redefine the guarantees of social cohesion and morality" in conformity with this principle;[12] a penchant for ignoring the different levels of social life, emphasizing man's egoistic or self-interested, rather than his social, dimension;[13] and, finally, an animosity toward the church and "transscendental justice," which resulted in exalting social utility as the only standard of judgment, and the state as the "sole source and sanction of morality."[14]

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[10] There is, of course, one highly-touted exception to this generalization, Machiavelli. But it is unlikely that Talmon or Popper would be prepared to claim him for their side.

[11] *Origins*, p. 18.

[12] *Ibid.*, p. 22.

[13] *Ibid.*, p. 4, 29ff.

[14] *Ibid.*, p. 4.

The crucial assumption on which this indictment rests concerns the underlying rationale for this philosophic armamentarium: the philosophes warrant Talmon's strictures only if we assume that they opted arbitrarily for his "messianic" orientation—that they were entirely impervious to historical circumstances in their political thinking. Yet if we examine the context of eighteenth-century French thought, we discover that such an assumption is unwarranted; it was precisely in historical circumstance, rather than in motive, that the philosophes differed so strongly from the English. And, consequently, the constituents of the messianic mentality begin to lose distinctness. For instance, while it is true that the philosophes were concerned to discover a unitary principle of social existence (as most political thinkers have been), they diverged from English tradition in this respect only in the number of broadly shared principles available to them. The problem of creating a social niche for postmedieval man was more stark in France than it ever was in England, where social life had early reached a high degree of cohesiveness in the face of feudal regionalism. G. M. Trevelyan places the English sense of cultural unity as far back as the fourteenth century; by the time of Edward III, national self-consciousness had already begun to "dissolve the local loyalties and the rigid class divisions which had characterized the cosmopolitan society of the feudal age."[15] And this unity was not left to wither under the ravages of modernity. Buttressed by a fluid class structure, it acquired operative force over the lives of all Englishmen, providing them—through the agencies of common law, custom, precedent, and prejudice—with a set of rules for the conduct of their social and political life. Thus while England underwent its share of strife, it never experienced a social void. National self-consciousness provided a substratum, a firm bank to stem the currents of religious and political conflict.

There was no such unity in France.[16] Here strife was itself a deeply entrenched tradition, and each new conflict, unconfined by any dimension of fundamental consensus, threatened the very foundations of society. If the philosophes tended to emphasize the role of the state, ignoring other associations, they did so because their circumstances afforded them no alternative. "The nation," Turgot reported to the king, "is an aggregate of different and incompatible social groups whose members have so few links between themselves that everyone thinks solely for his own interests; no trace of any feelings for the public weal is anywhere to be found."[17] Previous ethical thought had not succeeded in adducing a unifying civic creed; in the preceding century moral codes had been private, for the most part, of interest only to the concerned individuals.[18] The old Christian social verities—which, however feeble and contradictory, had served to direct and restrain collective life, at least to some extent—had been eroded not by philosophic doctrine, but rather by increasing religious apathy.[19] Thus politics in France, while more fragile than in England, was nonetheless more vital for the maintenance of collective life. England had never been entirely dependent upon the political realm for unity or stability. But in France, the state was "accepted as being the only source of energy for the maintenance of the social system,"[20] and therefore, the only conceivable agent of harmony.

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[15] *English Social History* (London: The Reprint Society, 1948), p. 3.

[16] Medieval France presents a sharp contrast with Trevelyan's England. See, for instance, Marc Bloch's *Feudal Society* (Chicago: University of Chicago Press, 1968).

[17] Quoted in Alexis de Tocqueville's *The Old Régime and the Revolution* (Garden City: Doubleday and Company, 1955), p. 107. Tocqueville has abundantly documented the social and cultural fragmentation which confronted the philosophes, see part II, chaps. 8–12.

[18] Daniel Mornet, *French Thought in the Eighteenth Century*, trans. L. M. Levin (Hamden: Archon Books, 1969), p. 231. Nor, for that matter, had there been a vigorous tradition of moral speculation in the preceding century. Although the sixteenth century had witnessed the apogee of neostoicism, ethics lacked connection with a coherent view of life: ethical rationalism merely coexisted with skepticism in speculative matters. See A. Levi's *French Moralists: The Theory of the Passions (1585–1649)* (Clarendon: Oxford University Press, 1964), p. 2. Albert Desjardins begins his *Les moralistes français du seizième siècle* with the observation: "the sixteenth century produced neither a great school nor even a great thinker" capable of creating a system of morality (Paris: Librairie Académique, 1840), p. 2. And Octave Nadal describes the prevailing code of "*gloire*" as a mere mixture of egoism and fraternalism, "L'ethique de la gloire au dix-septième siècle," *Mercure De France*, 308 (1950), 22–34.

[19] Gustav Lanson, "L'influence de la philosophie cartésienne sur la litérature française," in *Études D'Histoire Littéraire* (Paris: Librairie Ancienne Honoré Champion, 1930), p. 69. See also J. P. Zimmerman's "La morale laique au commencement du XVIII^e siècle," *Revue d'Histoire Littéraire de la France*, 24 (1917), pp. 42–65. However, M. Thamin offers a contrary view, describing a continuous infatuation with religion from the post-Montaigne period to Bossuet. See his "La philosophie morale en France à la fin du XVII^e siècle," *Revue Hebdomadaire*, N°7 (Dec. 1896), p. 291.

[20] Tocqueville, *Old Regime*, p. 68. Also see H. G. Koenigsberger's "The Organization of Revolutionary Parties in France and the Netherlands during the Sixteenth Century," *Journal of Modern History*, 27 (Dec. 1955), pp. 335ff.

Nor can the philosophes' self-interested conception of human nature be understood apart from their historical circumstances, for the problem of achieving unity was compounded by a pervasive egoism which further reinforced the individual's sense of isolation. This psychology of self-interest was much more broadly entrenched in France than it ever was in England.[21] French egoistic formulations reflected not merely the disenchantments of a few isolated realists, but rather a general consensus, uniting several generations of intellectuals. In the previous century egoism had already intruded into most intellectual concerns. In ethics it had gained tentative expression in Libertinage, as well as in the cult of *"gloire"* and the new Epicureanism;[22] in letters we encounter it in the writings of Corneille, Molière, and La Bruyère;[23] even theology had acknowledged the new self-love, as we see in the works of such disparate thinkers as Pascal and Bossuet.[24]

The new conception of human nature reflected more than a sense of disengagement from Christian morality; it also expressed a disavowal of the claims traditional society had made upon the individual. Happiness as fostered by self-love is by no stretch of the imagination a social phenomenon. It is rather a starkly private experience, isolating the individual from his fellows, frequently placing him in such a state of tension with them as to cast doubt on the value of social (and all the more of political) life. Thus if the state were to succeed in awakening egoistic man's sympathy, it must undergo a change of purpose. A new foundation for authority was required, one which provided a creed congenial to the new mentality if the individual was to feel a genuine sense of political obligation, since the older claims derived from an ascetic conception of life fundamentally incompatible with his own. Such legitimizing notions as divine right and immemorial custom, which had proved so effective in the past, lost their magic ring; deprived of the vitalizing forces of piety and chivalry, the old preachments of self-abnegation fell on indifferent ears. Politics required to be placed on a new, entirely mundane axis: to become, for the first time, a distinctly human instrument. Reason of state must be made to defer to the reason of self-interest, properly understood, and political authority must be justified only in terms of its agency in the pursuit of this interest.

The Reef of Egoism

Mere nullification of the older standards was not the main task of the new psychology, however. Egoism—once incorporated within a compelling worldview—offered a new standard, imposing its obligations upon thought as well as deed. It was Descartes who, more than any other, provided this comprehensive vision. Others had already injected egoism into philosophy in limited ways, but these had offered only partial glimpses of a secular world, while Descartes presented a panorama: only *he* held out the promise of a scientific scheme total both in scope and in relevance for the problems of worldly existence.[25] With Descartes philosophy becomes all-encompassing and, at the same time, practical. To be worthy of his title, the philosopher must create an entire tree of knowledge, rooted in metaphysics yet bearing fruit of the most useful variety, such as mechanics, medical science, and morals.[26]

The Cartesian philosophy remains faithful throughout—both in scientific and ethical thrust—to the new mentality which inspired it. Self-assertiveness and the lust for knowledge are fused indistinguishably within the new scheme—so completely, indeed, that the philosopher-scientist usurps the medieval paladin's position as epic hero. Knowledge ceases to be understood as passive acquisition within a social context; it is viewed instead as a personal trophy to be gained on a new sort of battlefield, an arena of intellect in which the philosophic hero must engage the ubiquitous forces of error in deadly combat.[27] And his struggle must be a lonely one since, unlike his heroic predecessors, he has neither comrades nor gods to assist him.[28] Yet at the same time, he has unprecedented scope for heroic assertion. Possessed of an unutterable quality of self-awareness[29]—an ability to grasp truth clearly and distinctly—and a Herculean capacity for self-discipline, he can create knowledge virtually from his own soul: "This method of ours resembles indeed those devices employed by the mechanical crafts, which do not need the aid of anything outside of them, but themselves supply the directions for making their own instruments."[30] Thus can philosophy impose total coherence upon the world, and insurmountable problems cease to cloud the human ken.[31]

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[21] Whereas Hobbes and Mandeville were viewed with opprobrium at home, La Rochefoucauld was widely acclaimed among his countrymen.

[22] J. S. Spink discusses these trends in his *French Free Thought From Gassendi to Voltaire* (London: Athlone Press, 1960). See also Nadal's "L'Ethique de la Gloire au Dix-Septième Siècle."

[23] A. J. Krailsheimer, *Studies in Self-Interest: From Descartes to La Bruyère* (Oxford: Clarendon Press, 1962), chaps. 3, 11, 12.

[24] Krailsheimer, chaps. 5, 7, 10. Nor was the psychology of self-love confined to intellectuals. It also had strong economic roots in the life style of the new bureaucrats created by Louis XIV. See Gustav Lanson's two essays: "La transformation des idées morales et la naissance des morales rationnelles de 1680 à 1715" and "L'eveil de conscience sociale et les premières idées de reformes politiques." Both essays are included in *La Revue du Mois,* 9 (Jan. 1910), 5–29 and (Apr. 1910), 409–430, respectively.

[25] Hobbes—that other great philosopher of egoism—lacked Descartes's prestige in mathematics. Moreover, Hobbes's political conclusions were unacceptable. See my concluding section.

[26] *The Principles of Philosophy* in *Philosophical Works of Descartes,* trans. Elizabeth S. Haldane and G. R. T. Ross, 2 vols. (New York: Dover Publications, 1955), I, 211. Except when otherwise indicated, my citations of Descartes are from this collection.

But the new philosophy spoke to the heart (suitably recast as the pineal gland) as well as the mind, and its message was most welcome, for it conveyed an ethical creed that legitimated the motives of self-interest. The egoist learned he had an obligation to seek happiness: not sagacity or beatitude, but sheer physical pleasure, that forbidden fruit of the passions. With Descartes, the passions gain full acceptance; properly disciplined, they provide the great motive force for action. Passionate intensity loses its shamefulness so completely as to challenge the position of Christian grace as the final measure of felicity: those men whom the passions most move are "capable of partaking most of enjoyment in this life."[32] Yet happiness, like knowledge, is not gained without self-discipline; it, too, has its duties. The happy man must harness his passions in order to enjoy them. He must become total master of circumstance as well; he must make the world yield itself to his requirements. And the determined hero can succeed as readily in this struggle as in the battle for truth, for here, too, he possesses an *élan*, private and invincible: "internal joy has some secret force which renders fortune more favorable."[33]

Descartes's system was widely acclaimed; indeed his synthetic feat earned him the title of presiding intellect in early-modern France and, to a certain extent, all Europe as well: "There was not a Frenchmen who, if he thought about such things at all, was not to some extent affected by him . . . nor was there a single foreigner of note who was not indebted to him. . . . At first denounced, put on the index, persecuted, condemned, the disciples of Descartes, when fifty years are passed, are appointed to learned professorships, deliver lectures, bring out books. Theirs are the honors now; *they* wield the sceptre."[34] After Descartes, happiness held undisputed reign among secular thinkers as the final aim of action, and the isolated ego was accepted almost without demur as the starting point of speculation. To think otherwise was to lapse into "unscientific" modes of thought, to engage in superstition or sentimentality.

But the new philosophy created a quandary for ethical and political thought. Descartes had vested the individual with a sense of uniqueness and potency such as he had never possessed, but Descartes had also promised to provide this individual with a science of morality which, presumably, would reconcile his private demands with those of society and the state. The latter promise, the creation of "the highest and most perfect moral science which, presupposing a complete knowledge of the other sciences, is the last degree of wisdom,"[35] was never fulfilled. Perhaps it was impossible to fulfill. One of the most salient characteristics of Cartesian man is that, in all important concerns, he needs no one. Whether he is seeking truth or happiness, he need not concern himself with others. Yet morality, in its traditional sense (certainly as Descartes's progeny understood it), demands that the individual look askance at exclusively self-oriented behavior. It involves him in a relationship of fundamental involvement with—and hence obligation to—others, requiring that he venture beyond himself in interest and commitment. Thus Descartes had the effect of offering two mutually exclusive goals for conduct; his promise resolves itself into a dilemma.

This dilemma provided a strong stimulus for the development of a distinctly modern, secular vein of French ethical and political thought. We encounter it first in the writings of Bayle and Fontenelle,[36] who pass it on to the eighteenth-century philosophes. Although the philosophes reflect innumerable differences in doctrine, interest, and temperament, they share in common a preoccupation with the political problem created by egoism. Most are determined to effect a "scientific" solution to this problem, to attack it in a manner congenial to the Cartesian framework. Thus Helvétius differs from the more humanitarian philosophes neither in intention nor orientation; his departure is confined to the "extreme" conclusions he draws from common premises.[37]

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[27] For Descartes's joust with error, see his *Discourse on the Method of Rightly Conducting The Reason and Seeking for Truth in the Sciences,* I, 90ff.

[28] *Discourse,* p. 127.

[29] *Ibid.,* I, 101; *The Search After Truth by the Light of Nature,* I, 324–325.

[30] *Rules for the Direction of the Mind,* I, 25.

[31] *Ibid.,* p. 25.

[32] *The Passions of the Soul,* I, 427.

[33] *Lettres sur la morale,* ed. Jacques Chevalier (Paris: Boivin et Cie [n.d.]), p. 100.

[34] Paul Hazard, *The European Mind* (New York: World Publishing Company, 1963), pp. 130–131.

[35] *The Principles of Philosophy,* I, 211.

[36] See M. Thamin's "La philosophie morale en France à la fin du XVII^{e} siècle."

Utilitarianism and the "Closed" Society

There remains a final constituent of Talmon's "messianic mentality" to be examined: the tendency to accept social utility as the only standard of judgment in political matters.[38] Here his case would seem to be most convincing, since it is Helvétius, that most authoritarian of philosophes, who is generally credited as the father of this doctrine. But here again we encounter the tendency to override crucial differences and to ignore context. Most of the philosophes were utilitarians, in some sense.[39] Even the humanitarian Voltaire—more conservative than his younger colleagues in many respects—was unequivocal on this score, insisting that "virtue and vice, moral good and evil, is therefore in all countries that which is useful or harmful to society; and in all times and places he who sacrifices most to the public is the one considered most virtuous."[40] Utilitarianism was compelling because, more than any other doctrine, it appeared to provide the egoistic mentality with the ethical coherence it had formerly lacked, extending the premises of the new world-view into the civic orb. There was no room for doubt concerning the egoistic basis of the utilitarian notion since it placed man firmly at the center of his universe. Deliberative action found its justification no longer in the vagaries of divine providence or the vagueness of teleological nature; the worth of a course of action was to be calculated solely in terms of its consequences for happiness. It was a simple doctrine, sufficiently clear and distinct for Everyman to grasp. At the same time, it bore the *cachet* of scientific sophistication, for it seemed to derive, with an almost Euclidean certainty, from Cartesian premises. Descartes had justified his method preeminently in terms of utility, and since he had accorded morality the final position in his scheme of knowledge, who could deny that utility must ultimately be cast in social terms?[41]

Utilitarianism appeared to offer a political standard uniquely suited to the needs of French libertarians. Granted that the state must be accorded more power and scope than its English counterpart, utility seemed to provide a principle of political authority that could both muster and restrain this power. If the utilitarian ruler could rest secure in the knowledge that his subjects, properly enlightened, would never seek to thwart him in the legitimate exercise of his authority, he must also realize that the very nature of this authority placed him in a position not too distant from that of the constitutional monarch. Unlike previous conceptions of authority—categorical, immured in religious mystery and tendentious history—the utilitarian standard was objective, scientific, and thus conditional. The state, its *raison d'être* visible to everyone, was to be held accountable for the maintenance of public happiness—a condition as real as (perhaps even more dramatically tangible than) any claimed by British subjects.

But a major flaw underlay this extension of the new worldview into politics, and it is here, in his response to this anomaly, that Helvétius radically diverges from his colleagues. While the utilitarian doctrine was admirably suited to the task of delineating the legitimate functions of government, it seemed unable to offer a persuasive code of political conduct for the citizen—one which, without gainsaying his egoistic nature, could explain why he ought to obey the state. In seeking to fulfill the Cartesian promise of a scientific morality, the doctrine of social utility had begged the central ethical problem posed by egoism, thus merely recapitulating, rather than resolving, the dilemma Descartes had created: why should the self-regarding individual concern himself with the happiness of others? Utilitarianism assumed that men were irredeemably egoistic, impelled by nature to pursue their pleasures. Yet at the same time it assumed they could stifle this impulse when it collided with the happiness of others; only a proper bargaining process was required, an authoritative exchange to establish the dominant trends in happiness. Thus utilitarianism appeared to flounder in its own psychological naiveté, for it could not accept the quintessentially private quality of egoistic pleasure. It is of the very nature of the egoist always to enhance his own pleasures, even at the expense of all others.[42] His happiness, immensurable, acknowledges no common standards; it accepts no bargaining, knows no marketplace.

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[37] D. W. Smith attributes the objections raised by the other philosophes against Helvétius to his "extreme" development of their own premises, in *Helvétius: A Study in Persecution*, pp. 165ff. There exists only the most scanty literature concerning Helvétius' role in the Enlightenment. Irving L. Horowitz's *Claude Helvétius: Philosopher of Democracy and Enlightenment* (New York: Paine-Whitman, 1954), describes Helvétius as a proto-Marxist thinker. Ian Cumming's *Helvétius: His Life and Place in the History of Educational Thought* (London: Routledge and K. Paul, 1955, and Mordecai Grossman's *The Philosophy of Helvétius: With Special Emphasis on the Educational Implication of Sensationalism* (New York: Bureau of Publications, Teachers College, Columbia University, 1926), stress his role as an educational theorist.

[38] *Origins*, pp. 4ff. Popper also condemns happiness, the utilitarian standard: "of all political ideals, that of making the people happy is perhaps the most dangerous one. It leads invariably to the attempt to impose our scale of 'higher' values upon others, in order to make them realize what seems to us of greatest importance for their happiness; in order, as it were, to save their souls." *Open Society*, II, 237.

[39] French utilitarianism appears to have originated as a late seventeenth-century protest against the encroachments of Louis XIV. See Lionel Rothkrug's *Opposition to Louis XIV: The Political and Social Origins of the French Enlightenment* (Princeton: Princeton University Press, 1965), pp. 299–372. For discussions of utilitarianism among the philosophes, see Kingsley Martin's *French Liberal Thought in the Eighteenth Century* (New York: Harper & Row, 1963), chap. 7, Lester G. Crocker's *Nature and Culture: Ethical Thought in the French Enlightenment* (Baltimore: The Johns Hopkins Press, 1963), chap. 5, and Peter Gay's *The Party of Humanity: Essays on the French Enlightenment* (New York: Alfred A. Knopf, 1964), pp. 200ff.

[40] *Traité de métaphysique*, included in *Oeuvres Complètes de Voltaire*, edited by Louis Moland (Paris: Garnier Frères, Libraires-Editeurs, 1879), XXII, 223.

[41] Descartes also appears to have been the inspiration for the notion of a calculus of happiness. See Louis Bredvold's "The Invention of The Ethical Calculus," in *The Seventeenth Century: Studies in the History of English Thought and Literature from Bacon to Pope*, ed. Richard F. Jones (Stanford: Stanford University Press, 1951), p. 166.

Most of the philosophes responded to this deficiency in the utilitarian doctrine by qualifying egoism—seeking to attenuate its force—and by lowering their expectations regarding politics. Without denying the ineradicability of self-love, they sought to minimize its social and political significance, describing "natural" peoples—such as Voltaire's Hurons and Hottentots or Diderot's Polynesians in the *Supplement to Bougainville's Voyage*—untroubled by egoism. In one of his rare rhapsodic moments, Voltaire even claimed that self-love could be transcended (at least within the family) by genuine love for others,[43] and he consistently maintained that the anarchy latent in the egoistic view of man could be mitigated by the emollient of enlightenment. Others, such as d'Holbach and La Mettrie had sought to restrain egoism with doctrines of determinism.[44]

Yet while egoism did not reduce the philosophes to utter despair regarding the possibility of humane politics, as it did Rousseau in his second *Discours*, it drastically reduced their political expectations. Without cogent principles the philosophe could offer only his personal example on behalf of obedience: he must always uphold the law, even as he seeks to change it.[45] This "realist" posture[46] of minimal expectations, however effective for purposes of combating specific abuses, nonetheless reflects a tacit abandonment of the quest of ethical coherence that had inspired traditional political philosophy. The civic realm remained a force alien to the egoistic individual; its claims were not his own, and therefore not obliging. Descartes's goal of a comprehensive science must be relinquished and concomitantly, his heroic conception of knowledge, since it appeared to defy extension into politics.

With Helvétius, however, utilitarianism acquires a different import. The heroic impulse, recaptured, sweeps away the psychological inconsistencies that assailed this doctrine in its other manifestations, and the "self-lover" becomes the dutiful citizen. Other notions of utility were deficient, in Helvétius' view, because they were based on a conception of hedonism that granted too much to nature and too little to art. It had been assumed that the individual's pursuit of pleasure was both given and directed by nature. This impulse, therefore, would always take precedence over merely political considerations, such as public happiness. But suppose politics could supplant nature; suppose the state, armed with education, could itself direct the individual in his pursuit of pleasure—suppose it could, having reduced these pleasures to a determinate number, acquire a monopoly on the means of their gratification? Then the notion of public happiness would assume a reality coequal with self-love, without our having to compromise the egoist premise, for in pursuing his most intense, his most private pleasures, the citizen of our carefully contrived state would also be fostering the public good. He would feel obliged to obey the state as the source of all he holds most dear: as the sum and substance of happiness.

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[42] "It is the nature of extreme self-lovers," wrote Bacon, a canny observer of the breed, "as they will set an house on fire, and it were but to roast their eggs" ("On Wisdom For a Man's Self," in *The Complete Essays of Francis Bacon, Including the New Atlantis and Novum Organum*, with an Introduction by Henry LeRoy Finch [New York: Washington Square Press, 1963], p. 64).

[43] *The Philosophy of History* (New York: The Citadel Press, 1965), p. 30.

[44] For d'Holbach, society is based upon natural inequality; therefore "morals, like the universe, are founded upon necessity or upon the eternal relation of things." *The System of Nature, or, Laws of the Moral and Physical World*, translated by H. R. Robinson (New York: G. W. and A. J. Matsell, Publishers, 1835), p. 66. Aram Vartanian provides a thorough discussion of La Mettrie's doctrine of determinism in his *La Mettrie's L'Homme Machine* (Princeton: Princeton University Press, 1960).

[45] Voltaire claimed that history fails to reveal a single example of a philosopher opposing himself to the laws. *Thoughts on the Public Administration*, included in *The Works of Voltaire*, translated by William F. Fleming (Paris: E. R. Dumont, 1901), XXXVII, 230.

[46] Peter Gay describes this posture in his *Voltaire's Politics: The Poet as Realist* (Princeton: Princeton University Press, 1959). Also, Gay's *The Enlightenment: An Interpretation*, II, chaps. 7–10, provides an excellent general description of the philosophes' sociopolitical thought. In addition, see chap. 4 for their conception of human nature, as well as Lester G. Crocker's *An Age of Crisis: Man and World in Eighteenth Century French Thought* (Baltimore: Johns Hopkins Press, 1959), chaps. 7, 8, and 12.

But if Descartes's quandry was to be resolved and his promise fulfilled, his method must be altered to accommodate our new, more grandiose hero, this artificer of happiness. Descartes had mistaken root for topmost branch in constructing his tree of knowledge, for he had naively exalted the private individual as nature's final master. His error had been merely procedural, however, a misarrangement of parts; the system required only to be placed on its proper axis. Our philosopher must speak for the state, rather than the self. The individual's sense of uniqueness and prowess must be transferred to the whole. Thus with Helvétius, the body politic acquires a soul, a personality all its own.

Utilitarianism as Transcendent Egoism

> If we would erect a magnificent monument, we should, before we lay its foundation, choose the ground, pull down the ruins with which it is encumbered and clear away the rubbish. Such is the business of the philosopher; let him not be accused of constructing no new edifice; it is he that now substitutes a morality that is clear, sound, and deduced from the very wants of man, for one that is obscure . . . and fanatic, the scourge of the present and of past ages: it is to the philosophers, in fact, that mankind will owe this first and sole principle of morality: *the public good is the supreme law.*[47]

In thus stating his task, Helvétius leaves no doubt as to the source of his inspiration. We again encounter our familiar paladin—bane of ignorance, deliverer of mankind—entering his arena. His contest for Truth's hand assumes the exact form Descartes had prescribed in his *Discourse on Method,* both in preparation and execution. He had dutifully retreated into isolation and purged himself of error; he had grappled with his soul until, ravaged, it had yielded the secret of felicity. Now, thus armed, he was prepared to create man in Descartes's image: to shape the amorphous, inert clay with his all-embracing consciousness; in giving this mass a purpose and a technique for self-discipline, he would make of it an autonomous agent.

But it is evident from the outset that our new philosopher would work with a different clay. His is a consciousness that aspires to transcend the merely private, to provide philosophy with a more exalted subject, embracing "all the parts of a government" in its grasp.[48] Like Descartes, he has his mandate from Nature, but she has grown wiser now, realizing that her mandate must necessarily be political: "whoever is endowed with these abilities is, in a manner, designed by Nature to fulfill, with respect to the legislature, the part of a thinking minister; and, to justify the observation of Cicero, that a man of genius is never a mere citizen, but a true magistrate."[49] Such a role, however, is incompatible with personal ambition. The philosopher must seek to become a selfless instrument of this greater whole—more a function than a man, a cognitive activity of the body politic. Consequently, the hero's ritual changes in substance, if not in form.

His quest for solitude had been governed by a desire to escape, rather than to discover, that fount of private awareness which Descartes had sought—to effect an "absolute detachment from personal interest."[50] Only then can he hearken to the greater voice within which speaks for the public weal.[51] Similarly, his self-purification acquires a different significance, for the meaning and prognosis of error have changed. Error can no longer be defined in relation to the individual as with Descartes—as an external, social intrusion, a belief that had found its way through thought's back door without facing the ordeal of personal scrutiny. The individual is himself a source of error; his self-interested claims must themselves be purged. It is as a collective force that error is pernicious, for here it affects not merely the private intellect but entire nations, eroding their strength and unity.[52] Such error has retarded the progress of morality "because men, being forced to unite in society, and to give themselves laws, were obliged to form a system of morality before they had learnt, from observation, its true principles."[53]

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[47] M. Helvétius, *A Treatise on Man, His Intellectual Faculties and His Education,* translated by W. Hooper, 2 vols. (London: [no publisher], 1777), II, 436.

[48] Helvétius, *De L'Esprit; or, Essays on the Mind, and Its Several Faculties* (London: [no publisher], 1809), p. 127.

[49] *Ibid.*

[50] *Ibid.*, p. 126.

[51] *Ibid.*, p. 74.

[52] *Ibid.*, p. 178.

Descartes had been formally correct in his diagnosis: unhappiness is attributable to an intellectual source, incorrect knowledge; its remedy must await an intellectual feat. Thus the philosopher retains his supremacy among men; it rests with him to discover the "true principles" of morality and to suggest the disciplinary measures for achieving them. But the knowledge and discipline provided by the new philosopher must be different, for Descartes had demanded both too much and too little of reason. Too much, because he had assumed that since error was intellectual in source, it could be corrected by disciplined thinking; the individual had only to adhere resolutely to his criteria of clarity and distinctness. Collective error, however, does not admit of such facile eradication. Since the moral beliefs of a people are a product of its laws, moral transformation can only be achieved by legislation.[54] And while Descartes's conception of discipline was naively intellectualistic, his notion of happiness afforded intellect too little scope. In his view, moral science need only supply a beacon for nature; the individual could trust his passions implicitly in his pursuit of happiness, for nature had designed them all for some praiseworthy use.[55] Only their "evil uses or excesses" create unhappiness.[56] He could rest assured that his passions, properly governed, would never rend his soul with strife.[57] Nor would they produce social conflict, for nature presides over our collective relations as with an unseen hand.[58] As we have seen, it is precisely this subservience to nature that had created seemingly insoluble problems for the utilitarian doctrine. But Descartes' determination to take nothing for granted in his quest for knowledge had faltered when he attempted to formulate a naturalistic ethic. He had neglected to question the very basis of morality, accepting uncritically a view advanced by classical tradition.

Helvétius needed only to remedy this oversight by substituting a positivistic conception of morality more in the spirit of the new naturalism: "But what is morality? The science of the means invented by men to live together in the most happy manner possible."[59] Moral truth thus becomes doubly artifactual: as a science it is fabrication; moreover it has as its purview invented means. And art triumphs over nature in the manufacture of happiness; the state's spokesman is freed from all restraint.

The new conception of morality does not entirely dispense with nature, however; suitably tamed, it provides Helvétius's scheme with its great principle. Men "invent" morality, but they do not (or should not) do so arbitrarily, for there exists a natural rule which serves as a standard for judging the validity of social arrangements. If men have not hitherto succeeded in discovering this principle, they, rather than the principle, have been diminished, for it exists, as objective and invariable as a geometric proposition.[60] Indeed, a morality constructed upon such a principle would be worthy of the veneration due a universal religion.[61] Helvétius finds in "corporeal sensibility" the fundamental principle upon which to build his monument:

> It is an evident proof of the truth of this principle, that it explains all the modes of being of mankind, that it develops the causes of their understanding, their stupidity, their love, their hatred, their errors and contradictions. This principle ought to be the more easily and universally adopted, as the existence of corporeal sensibility is a fact allowed by all, as the idea of it is clear, the notion distinct, the expression determinate, and, lastly, as no error can mix itself with so simple an axiom.
>
> Corporeal sensibility seems to have been given to men as a tutelar angel, charged to watch incessantly over their preservation. Let men be happy; this perhaps is the sole view of nature, and the sole principle of morality.[62]

His enthusiasm for this seemingly banal principle appears ludicrous unless we understand its strategic value for him. The content of "corporeal sensibility" seems to consist of two scarcely original psychological assertions: first that the mind is, at its inception, a *tabula rasa* upon which impressions are stamped by experience and second, that all human motivation is reducible to the pleasure-pain impulsion.[63] But in Helvétius's hands "corporeal sensibility" becomes a potent weapon for annulling the claims raised by individuality against the state. It is this natural principle that provides him with the means for politicizing self-love. Happiness ceases to be a condition over which the individual has some measure of control; corporeal sensibility makes of it a political commodity: "This happiness is dependent on the laws under which [men] live, and the instructions they receive."[64]

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[53] *Essays on the Mind*, p. 172. The analogy with Descartes's conception of error remains exact in form. See *Meditations*, I, 174ff.

[54] *Ibid.*, p. 123.

[55] *Passions of the Soul*, I, 411–412.

[56] *Ibid.*, p. 425.

[57] *Ibid.*, p. 353.

[58] "For God has so established the order of things, and joined men together in a society so closely knit, that although each individual looks out only for himself and feels no charity for others, he would not fail, in ordinary matters, to concern himself with them to the utmost extent of his power, provided that he use prudence . . ." *Lettres sur la morale*, pp. 107–108.

[59] *A Treatise on Man*, II, 12–13, n.

[60] *Ibid.*, I, 56–57.

[61] *Ibid.*

[62] *Ibid.*, II, 469.

[63] See *Essays on the Mind*, I, and *A Treatise on Man*, I, sect. IV, chap. 22, and II, sect. V, chap. 2, in particular.

Yet the political art exists only as a possibility. Nature does not yield easily to such contrivance; she has made the individual a zealous guardian of his "interest." Private interests play the same role within the body politic as had the passions for Descartes's individual: properly disciplined, they provide its mainspring, but uncontrolled they are the root of all social conflict, the greatest stumbling-block to unity. Interest must not be understood in its obvious economic sense, however: with Helvétius it takes on a far broader significance, acquiring psychological, moral, and even epistemological implications. Although the mind is congenitally a *tabula rasa,* it is not without an internal selective principle. We learn whatever nature teaches us to be of significance in our pursuit of pleasure; our hedonistic mechanism remains our ultimate mentor. And since our experience has always been left to chance, each individual has acquired his own assessment of his interest, including a strategy of conduct and a private conceptual universe. Thus it is differences in interest that account for the unhealthy diversity of opinion in matters of politics, morality, and metaphysics.[65] As a consequence, social progress has been stifled by the powerful forces of these individual interests:

> like the stars, which suspended in the deserts of space, are there moved by two principal motions, the first slow, in common with the whole universe, and the second more rapid, and peculiar to those bodies: so each society is moved by two different kinds of interest. The first and weaker is in common with the whole society in general, that is, with the nation; and the second, more powerful, is absolutely peculiar to the individual.[66]

But we can employ this self-regarding principle for political purposes. By controlling the individual's experience, we can deprive his interest of its idiosyncratic quality; since he affirms as "good" that which is pleasurable, we can control his moral dimension simply by controlling the objects of his pleasure: "No individual is born good or bad. Men are the one or the other, according as a similar or opposite interest unites or divides them."[67] Thus interest constitutes an attenuated version of egoistic passions, but with an important difference. "Interest" is much less complex than the egoist's unpredictable passions, and therefore more easily subject to political control. The issue of reconciling the claims of self-love with the public good ceases, therefore, to founder on conceptual shoals. The sole issue remaining is practical: the philosopher must devise the disciplinary means for resolving the antithesis between the true, public, interest and its diverse private approximations; he must manipulate the strings of private interest.

In order to accomplish this task, however, reason requires the assistance of a corporate will, a legislator. The philosophe is only the architect of the moral edifice; without the legislator he can do nothing. It is legislation that creates the desired unity, performing two vital tasks. Its first and more pressing function is that of compulsion. Helvétius evinces at times a nearly Hobbesian concern with forcing the individual into obedience: "Man is by nature intolerant. If the sun of reason enlighten him for a moment, he should seize the opportunity to bind himself down by sagacious laws, and put himself in a happy state of impotency, that he may not injure others if he should be again seized with the rage of intolerance."[68] Men can, by means of a legally established "mutual and salutary fear," be forced to submit to justice;[69] indeed they can be compelled to virtue.[70]

Ad hoc legislation and fear are, however, only stopgap measures, hardly worthy of the corporate hero. It is not by means of internal stalemate that the science of morality can begin its vaunted progress towards perfection. Legislation must have a positive, long-range function; it must impress itself upon the mind in more permanent fashion. Helvétius follows La Mettrie's precedent[71] in providing the state with the means for executing this most important task: if it is true that men pursue their private interests at the expense of the state, it is only because they have been encouraged to do so as a result of faulty education. To hold individuals responsible for their antisocial pursuits is to flatter their self-esteem. They are no more responsible for their iniquities than for their happiness. It is imperative that we regard vice, as well as "the understanding, the virtue, and genius of man as the product of instruction."[72] The laws, therefore, must serve an educative function if they are to succeed in abolishing the force of individuality. This task is, of course, a difficult one, but "there is nothing impossible to education: it makes the bear dance."[73] In order for legislation to have its educative effect, however, the philosophe must work in the closest conjunction with the legislator.

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[64] *A Treatise on Man,* I, 2.

[65] *Ibid.,* p. 175.

[66] *Essays on the Mind,* p. 72.

[67] *Ibid.,* II, chap. 2.

[68] *Ibid.,* I, 348–349.

[69] *Ibid.,* p. 298.

[70] *Ibid.,* p. 318.

[71] I have discussed La Mettrie in this context in my "La Mettrie: The Robot and the Automaton," *Journal of the History of Ideas,* XXXI (Oct-Dec. 1970), pp. 555–572.

Helvétius is very careful to avoid La Mettrie's mistakes in his manipulation of hedonism; he seeks to preclude the very possibility of drawing similar conclusions from his argument. La Mettrie had attempted to deny the egoist his inner vitality, but he had left an inscrutable element of "organization" within the individual. Helvétius is aware of the dangers of this unpredictable capacity: to explain the human understanding in terms of organization is to attribute to it an unknown cause, an occult quality.[74] And such a postulate is unnecessary, for all of the supposedly mysterious principles of human action are reducible to the simple predictable rule of corporeal pain and pleasure.[75]

Helvétius does seek the same effect as La Mettrie, however, since both are concerned to annul the political significance of egoism. But for Helvétius it is not necessary to invoke La Mettrie's mechanistic analogies to accomplish this task; personal experience and elementary common sense suggest that character is situational in its entirety; one has only to alter a man's situation in order to change his character.[76] As private individuals, men have no control whatever over their lives. Let the individual examine his life but for a moment. What is most fundamental, most enduring about it: is it his incessant pursuit of happiness, as Descartes had suggested—is it this which imparts coherence to his existence? If so, his life is but a ludicrous delusion, for he has no control over this happiness. Let him recall his moments of greatest triumph and deepest despair, his interludes of rapture and agony: can he really say that he was responsible for them? If such men are honest must they not conclude with Helvétius that

> . . . their exaltation or disgrace, their happiness or misery, are the product of a certain series of circumstances, of an infinity of chances unforeseen, and is apparently insignificant. I compare the little incidents that produce the great events of our lives, to the hairy fibres of a root that insinuate insensibly into the clefts of a rock, and there increase that it may one day spring up.[77]

The individual attains no coherence in the course of his life, nor does he come into the world with any measure of innate constancy; he is born without knowledge—without innate ideas—and without passions.[78] Even self-love, that passion so fundamental to the egoist, is an acquisition: "In short, if we acquire even the sentiment of self-love, and if we cannot love ourselves without having previously felt the sensation of corporeal pleasure and pain, all then in us is acquisition."[79]

Man is of himself a cipher, born with a constitutional endowment exactly like that of every other member of his species. It is therefore absurd for the individual to hope for personal recognition and achievement. One cannot help admiring Helvétius' determination in thrusting home this point, for he does not settle for easy examples. He takes on genius, that most sublime of aberrations from the mass pattern. Genius is no more than a result of a "strong and concentered" attention to a given field of inquiry, arising from a lively "taste" for that field. Since men possess no congenital ideas or tastes, genius is necessarily an acquisition, like any other aspect of character.[80] To "explain" the genesis of genius, however, is not to gainsay it. Helvétius utilizes an additional ploy; rather than seizing upon genius as a disposition, he attacks instead its fruits: "Almost all philosophers agree, that the most sublime truths, once simplified and reduced to their plainest terms, may be converted into facts, and in that case present nothing more to the mind than this proposition, *white is white,* and *black is black.*"[81] Creations of genius thus reduce themselves to frills embroidered upon the most obvious of truisms. Although men vary in their tastes, these are "nothing more than unconnected and sterile facts."[82] Moreover, while it is possible that individuals can entertain sensations of an entirely incommunicable—and hence irreducibly private—nature, they cannot cherish ineffable ideas. All ideas are simple, and if clear, easily communicable; consequently there are no ideas that Everyman, collective man, could not apprehend.[83] Survey all culture and you find the same monotonous uniformity: whether man engages in proverb-coining, legislating, or artistic creation, his work can never escape the mold of sameness.

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[72] *A Treatise on Man,* I, 2.
[73] *Ibid.,* p. 279.
[74] *Ibid.,* p. 91.
[75] *Ibid.,* pp. 91–120.
[76] *Ibid.,* p. 278.
[77] *Ibid.,* pp. 36–37.
[78] *Ibid.,* p. 280.
[79] *Ibid.,* II, 8.
[80] *Ibid.,* I, 32. Even here Helvétius has preserved his formal continuity with Descartes. Descartes had also deprecated the role of genius in order to underscore the significance of method: "anyone who has perfectly learned the whole of this method, however moderate may be his talent, may see that no avenue to the truth is closed to him from which everyone else is not excluded." *Rules for the Direction of the Mind,* I, 28.
[81] *Ibid.,* p. 223.
[82] *Ibid.,* p. 170.

Helvétius never quite succeeds, as he admits, in reducing genius entirely to mediocrity: the actual creative moment is, and must remain, an affair of unpredictable chance.[84] In making this admission, he reveals the motive for his concern with genius quite explicitly. If genius is a product of situational, rather than organizational, chance, then all variation in the pattern of humanity can be almost completely overcome. But if genius emerged from its encounter with Helvétius with a few bedraggled remnants of its grandeur intact, his assault had been sufficient to make his point: "Men that are commonly well organized, are like certain sonorous bodies, that without being exactly the same, still yield the same number of sounds."[85] He delivers the human race up to his reader like a bag of translucent marbles, identical in size, weight, and color; their only possible mode of individuality might consist in some flaw in their construction that would relieve them from the onus of being "commonly well organized."

Thus the individual enters society for his education. He comes not with demands, ambitions, or even curiosity, but rather as a passive choir boy: he "is by nature an imitator, an ape,"[86] docile to example.[87] Government, functioning in its capacity as the "principle instructor" of this youth,[88] performs the task of rendering him sociable. And with sociability comes his humanity. He acquires an identity; indeed he participates in the most exalted identity to be found in creation, for he becomes a member of a society. The state takes the amorphous organism and makes of him a man of culture, equal to the best that humanity has to offer. Since the organism is unaffected with congenital self-love, the state has little difficulty in conditioning him to altruism: "from a good education, the idea of my own happiness will be always more or less closely connected in my memory with that of my fellow-citizens; and . . . the desire of the one will produce in me the desire of the other. . . ."[89] He can even be trained to compassion if his education, his laws and his government lead him to it.[90] There is not the slightest prospect for an attitude of recalcitrance on his part, for shame is to be employed as an infallible instrument of efficient socialization: "A child cannot long sustain the contempt, insults, and railleries of his comrades: there is no social defect that such treatment will not correct."[91]

Political education is thus Helvétius's final solution to the problem of egoism.[92] It is by means of this force that individual pleasure is made dependent on the state. The individual must be socialized at the price of all spontaneity or individuality. Reduced to one among a multitude of identical units within the corporate body, he performs his function. There is nothing unique even in his happiness; because his pleasure is identical with that of all the other units, the sole standard of happiness is quantity. Since the state optimally controls most pleasurable objects, the individual need not actively seek happiness; like the protestant's grace, it either arrives or fails to arrive, depending upon the corporate will. He need not even seek sexual gratification, for if Helvétius is to be taken seriously, the hive benevolently procures for its efficient members, thus preventing recalcitrance in the same manner as the squaw who masturbates her child to sleep at night.[93]

Moreover, political education directs the egoist's attention away from himself, fostering a collective spirit of Spartan intensity. The sophisticated egoist now realizes that love of himself is really love of his country, repository of all pleasure. He knows that as an individual he is incapable of genuine autonomy; indeed he would shun personal autonomy even if it were attainable, for his private 'interest' tends to isolate him from the greater whole. Only as a member of the state can he share a measure of reflected prowess: only the state is without external constraint. No rules, no allegiances bind this collective entity. Consequently, there are no moral qualms attached to its relentless quest for aggrandizement; since the self-love of patriotism is absolutely exclusive of the love of mankind, we owe no fealty to the latter.[94]

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[83] *A Treatise on Man*, pp. 157–158.

[84] *Ibid.*, pp. 255ff.

[85] *Ibid.*, p. 171.

[86] *Ibid.*, II, 438.

[87] *Ibid.*, I, 280.

[88] *Ibid.*, p. 26.

[89] *Ibid.*, II, 14.

[90] *Ibid.*, p. 18. Cf. La Mettrie's conception of 're-morse' in his *Anti-Sénèque*, included in *Oeuvres philosophiques de M. de La Mettrie* (Amsterdam, 1774), II.

[91] *Ibid.*, p. 400.

[92] Again, Helvétius is exploiting a familiar form; in this instance, the educational ideal underlying the Greek *paideia* tradition. But the Greek ideal, as we encounter it for instance in Aristotle, sought to employ education preeminently for the development of character. For Helvétius, on the other hand, the main function of education is to instill obedience

[93] *Essays on the Mind*, 278ff.

At least some vestiges of the Cartesian sense of uniqueness were retained, however, in the politicization of the egoist: "Each nation," after all, "has its particular manner of seeing and feeling, which forms its character. . . ."[95] The individual can thus enjoy a sense of vicarious participation in uniqueness. But such enjoyment provides little comfort when measured against the monotony of one's personal predicament: "Why, say strangers, do we perceive at once, in all the French, the same spirit, and the same character, like the same physiognomy in all Negroes? Because the French do not judge or think for themselves, but after the people in power. Their manner of judging for this reason must be sufficiently uniform."[96]

Helvétius fell victim to his own argument. In *De L'Esprit,* he had described the progress of his moral science with an air of inevitability. One had but to expose the errors and unmask the opponents of this science in order to set it upon its course.[97] Of course his method required its resident genius, but there is no indication that there would be any difficulty in finding such a man. But after having written so forcefully against human prowess, Helvétius discovers, as he concludes *De l'Homme,* that his belief in genius has disappeared. Thus there is little prospect of moral progress, after all, "because genius is required to substitute good laws in the place of bad, and courage to make them received. Now in almost all countries the people in power have neither the necessary genius to form good laws, nor sufficient courage to establish them, and brave the clamors of ill-designing men."[98] The corporate egoist emerges, then, with a redeeming quality. His lethargy and mediocrity at least have the merit of self-destructiveness.

Helvétius had gone too far; his politicization of hedonism was unacceptable to the other philosophes. Even Rousseau, who was personally quite sympathetic with Helvétius, could say no more by way of exoneration than: "True, M. Helvétius has written a dangerous book . . . But he has quit his office of 'farmer-general,' he has made the fortune of an honest girl, he is devoting himself to making her happy, he has on more than one occasion given help to the unfortunate: his actions are worth more than his writings."[99]

Conclusion

If the other philosophers were vehement in their rejection of Helvétius, they could not have failed, nonetheless, to see a part of themselves in him. They shared in common with him not merely a problem and a method, but also a conception of law—and it is here that their affinity with him must have aroused most chagrin, for it is their legal views that set them apart most distinctly from the British and, at the same time, posed the greatest danger to their humanitarian aims. It was in enlightened legislation that the philosophes found their hope: although they varied greatly in their conception of the function of law, most were legalists. And while they admired British legal institutions, they radically diverged from British modes of thought in their conceptions of the source, the function, and the scope of law.

Tradition, custom, convenience—a thinker mindful of the history and the contemporary sociopolitical institutions of France could not invoke these congenial English legal sources. Even natural law, as rendered by the English, would not do for the philosophes. The Hooker-Locke tradition had overlooked the crucial question, since this conception of natural law assumed natural sociability. Hobbes had been more profound, but his conception of law was better desinged to subdue than to improve humanity; moreover Hobbes had lapsed from his naturalistic construction of law, buttressing it with divine sanction. If law was to be an effective instrument for amelioration, it must be rooted in the biological core of the human organism. The quest for legal certainty must begin with a Cartesian self-purge. The thinker must look within, divesting himself of all the culturally and biographically conditioned. He must strip away the strata of civility until he discovers within himself a complex of starkly biological needs. At this level only is he truly man, universal man. Wherever and whenever we find him, he is essentially the same; whether engaged in clever salon repartee or in the most barbaric rite, the natural man is activated by the same biological impulses. It is in this man that we find our legal fountainhead.

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[94] *Essays on the Mind,* p. 187c.

[95] *A Treatise on Man,* I, 272.

[96] *Ibid.,* pp. 276–277.

[97] *Essays on the Mind,* pp. 172–185.

[98] *Essays on the Mind,* p. 187ff.

[99] Letter to M. DeLeyre, October 5, 1758, included in *Citizen of Geneva: Selection from the Letters of Jean-Jacques Rousseau,* ed. Charles Hendel (New York: Oxford University Press, 1937), p. 153.

"Natural law" is what remains as the indispensable requisites for commodious existence after we have effected our self-purge. It is not by any means a grandiose legal standard; indeed it is lowly, even seamy. But it is one which bad laws and bad opinions cannot erase, at least so long as man remains recognizably human. The philosophes again differed in their descriptions of these laws of human nature. Sometimes, even, an individual thinker is unable to identify them consistently.[100] They did agree, however, that law must have its ultimate source, not in the divine precept or the self-evident axiom, but rather in the fundamental biological needs—that is to say, in the passions. The ultimate proof for the existence of a natural law must be "I feel" (rather than "I see") "that it is so."

The philosophes could not accept English legal conceptions because they expected more of law than did the English. In England, law was understood to serve an essentially negative function. It was a protector, safeguarding rights and privileges that had been won in other arenas. The philosophes, too, were concerned with this legal function: they, too, loved their liberty and sought its protection in law. But to them, the legal system exercised a much more determinative influence upon human affairs, whether for good or evil. Because it is rooted in man's most fundamental biological needs, it can alter his very being. At its best, it offers man much more than merely a "standing rule to live by," as Locke had suggested. It is a creative force, the sole instrument by which mankind can attain its true humanity. Without law, humanity is unthinkable; man could be only a stupid brute.[101] And with bad laws, man is even more abominable than the brutes, for he devours his brethren and even himself. It is law that civilizes, that makes men reasonable, gentle, and appreciative of the finer things life has to offer.

To observe, however, that a number of the philosophes' factual and methodological presuppositions reflected a departure from classical libertarian beliefs is only to underscore the modern quality of their thinking. It is both gratuitous and stultifying to stigmatize this departure with the rubric "totalitarian," for to do so in no way advances our understanding, either of our tradition of political speculation or of our contemporary situation. It is to substitute labels for critical thinking, to ignore the vital differences which separate Voltaire, d'Alembert, and Diderot (*inter alios*) from Helvétius; it is, indeed to reject modernity itself, for our contemporary situation more closely resembles that of the philosophes, in many respects, than that of the classical English libertarians. We, too, are votaries of a scientific orientation to life which—at least if we accept the sociopolitical preachments of some of its champions[102]—tends, at times, to override liberty in the name of efficiency. Again, like the philosophes, we find ourselves increasingly unable to accept the legal presuppositions of the English libertarians, for we, too, have learned from our social science that man is a creature of his collective relations. The notion that he is possessed of an inviolable realm of private experience—an indispensable assumption for libertarians—thus accords no more with our contemporary perceptions than with those of the philosophes.

Finally, we are still prey to the same political problem that haunted the philosophes; we have been no more successful than they in discovering a cogent argument on behalf of political obedience in a secular world. Because our world is one devoid of transcendent schemes or objective purposes—notions which have themselves been scorned as totalitarian—we appear to have nothing to offer the conscientious objector by way of ethical persuasion. Like the philosophes, we can, at best, seek to avoid the issue, or, like Helvétius, we can seek to render it meaningless.

Nor is it likely that we can answer the queries of the political iconoclast any more effectively than did the Frenchmen. We, too, are haunted by the specter of Gyges, that nemesis of political thought conjured up by Glaucon in Plato's *Republic*. Discovering a magic ring which gave him the power of invisibility, Gyges hurled a challenge across the ages: "Suppose I employ my new power to achieve the utmost in political happiness," he asks in effect; "suppose I use it to cheat and betray, even to murder in my pursuit of happiness. If there are no objective rules binding my conduct—no gods, no impersonal morality—then what is to stop me? Can you even say that I am wrong?"[103] This challenge could not be met by the French thinkers, and it is doubtful that we in the twentieth century have succeeded where they failed.

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[100] For Voltaire's vacillation on natural law, see Peter Gay's *Voltaire's Politics*, pp. 343–346. La Mettrie ultimately repudiates natural law. See my "La Mettrie," pp. 568ff.

[101] Hence Voltaire's reported jibes at Rousseau's noble savage. And Voltaire is equally contemptuous of Adam, Christianity's prototypical natural man. See his poem, *Le Mondain*.

[102] For example, Norbert Wiener contemplates the possibility of a cybernated *machine à gouverner*. See his *The Human Use of Human Beings: Cybernetics and Society* (Garden City: Doubleday & Co., Inc., 1954), pp. 178ff. Nor is this Strangelovian orientation confined to the physical sciences. B. F. Skinner's *Beyond Freedom and Dignity* (New York: Alfred A. Knopf, 1972) amply illustrates that the behavioral sciences are not immune. See also his *Walden Two* (New York: The Macmillan Co., 1962).

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[103] *The Republic of Plato,* translated with introduction and notes by Francis MacDonald Cornford (New York: Oxford University Press, 1967), pp. 44ff: This is substantially the argument propounded by the title character in Diderot's *Rameau's Nephew.* It is unlikely that we today could improve upon Diderot's response: "There was in all he said much that one thinks to oneself, and acts on, but that one never says. . . . Neither more nor less detestable than other men, he was franker than they, more logical, and thus often profound in his depravity." *Rameau's Nephew and Other Works,* trans. Jacques Barzun and Ralph H. Bowen (Indianapolis: The Bobbs-Merrill Co., 1956), p. 74.

Political Parties and the Porkbarrel: Party Conflict and Cooperation in House Public Works Committee Decision Making*

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Introduction

In the annals of American political lore, porkbarreling has long been synonymous with domestic legislation having obvious political content. The term "porkbarrel," meaning the distribution of public works expenditures on the basis of political influence, has been applied most frequently and most pejoratively to the legislation processed by the House Public Works committee.[1] And understandably so, for political meaning *does* inhere in public works proposals; public works projects can profoundly alter the living circumstances of citizens within and beyond the areas in which they are built. Thus members of the committee that recommends the construction of roads, dams, public buildings, water pollution treatment plants, the drafting of harbors, the abatement of beach erosion, or any number of economic development projects, are presumed to be in a unique position to alter electoral probabilities in favor of themselves and other members of the House.[2] In the lore of American politics, then, authorizing public works projects is the result of spontaneous, widespread favor trading. One can account for the authorizations by the "simple matter of *quid pro quo* . . . , you scratch my back and I'll scratch yours. . . ."[3]

Such bipartisan favor trading cannot, however, account for the broad patterns of Public Works committee behavior, *party conflict* and *party cooperation*. To account for these patterns of party conflict and party cooperation, one must begin with party affiliation, not with exchange processes. Public Works legislation, as Julius Turner reported more than twenty years ago, is moderately partisan.[4] Since the time of Turner's study, the committee's jurisdiction has expanded to include many more issues that are likely to split the parties. Most committee legislation will engender party disputes because committee Democrats and Republicans cannot arrive at a consensus either on regional allocation issues (allocations benefiting a minority in each party) or on traditional party issues. The parties have yet to reach a consensus on aid to economically depressed areas. Committee Democrats and Republicans cannot reach agreement on the authority of the federal government or on its position in the economy. The federal government's position in the economy drives the parties apart over issues such as whether private or public power should be supported; whether the federal government or the state governments should assume responsibility for treating water pollution; how the financial burden of the Interstate and Defense Highway System should be distributed; and, finally, whether or not new construction in the District of Columbia should be undertaken. Nor can the parties agree on the authority of the federal government in approving state water quality standards or promulgating federal standards or penalizing the states if they do not comply with highway beautification standards. Very often, then, Public Works Democrats and Republicans are divided when the committee reports legislation to the House. When there is such committee division, it usually means that the committee majority party is attempting to provide leadership for change and must, therefore, depend on the House majority party for success.

But public works legislation is still best char-

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[1] Stephen K. Bailey and Howard D. Samuel, *Congress at Work* (New York: Henry Holt & Company, 1952), chap. 7; N.B. pp. 166–168.

[2] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley & Sons, Inc., 1966), chap. 11. Professor Warren E. Miller and Donald E. Stokes discuss the significance of favorable images as against the significance of issues with partisan content in congressional elections.

[3] Jack C. Plano and Milton Greenberg, *The American Political Dictionary,* 2nd ed. (New York: Holt, Rinehart & Winston, Inc., 1967), p. 145.

[4] Julius Turner, *Party and Constituency* (Baltimore: The Johns Hopkins University Press, 1951), p. 70.

acterized as only moderately partisan. Public Works Democrats and Republicans are frequently united when reporting bills to the House. Nonetheless, party affiliation is more important than exchange processes in accounting for this committee party cooperation. Party cooperation is manifest in the national allocation issues (allocations benefiting a majority in each party), such as projects of the Army Corps of Engineers, federally funded highway mileage, federal dollars for waste treatment construction plants, and public buildings *outside* the District of Columbia. This party unity is misleading, however, in that it suggests a consensus between Democrats and Republicans on national allocation issues. Since, however, the distinct possibility of partisan porkbarreling exists, such a consensus is very unlikely. Why, it must be asked, should rational calculating Democrats and Republicans entrust part of their party's fate at the polls to an ad hoc coalition building process such as porkbarreling? The threat of partisan allocations has induced the congressional parties to adopt relatively rigid allocation rules such that neither party can make substantial gains at the polls at the expense of the other. So, while Public Works committee members are, *to some degree,* in a position to push their pet projects and ideas, committee proposals on national allocation issues cannot be explained in terms of bipartisan favor trading. Instead, the committee must use the allocation formulas as the measure of acceptability in the House. The Public Works committee's relationship with the House of Representatives is thus often routine, allowing little or no leeway for change through committee recommendations.

In identifying and accounting for these patterns of party conflict and party cooperation and linkages in them with House decisions, evidence from protocols, congressional documents, and roll calls has been used. During the 1st and 2d sessions of the 90th Congress, all but two of the sitting members of the House Public Works committee were interviewed. Forty-two open-ended, structured interviews were conducted with 37 committee members and former committee members. Interviews were conducted on an anonymous basis and averaged 55 minutes in length. Analysis of the internal operations of the committee relies heavily on the interview protocols. Committee reports and House debates on all the major bills from the 84th through the 90th Congresses proved to be essential supplements to the protocol data. Since some of these documentary data can be quantified, they have been used to help establish patterns of internal committee behavior. In addition, documentary evidence, particularly the debates, proved to be indispensable for assessing the relationship between the committee and the House. Finally, the roll-call data proved to be helpful for all aspects of this study. Analysis of the 68 conflict roll calls on Public Works committee bills from the 80th through the 90th Congresses helped establish patterns and account for them. The roll calls also proved helpful in analyzing the relationship between the committee and the House. No one particular kind of evidence for any important aspect of this study was used; all the evidence available from any of the sources used has been brought to bear as seemed appropriate.

The Members and the Issues

Crucial to the relationship between issues and party affiliation on the House Public Works committee is the distribution of membership goals, for they affect committee party conflict. Generally, congressmen find a committee appealing for at least three different reasons: prestige, bringing federal largesse back to the district, and subject-matter interest.[5] Both prestige and constituency goals have a marked tendency to minimize party conflict; subject matter interest is likely to amplify party conflict.[6]

The Members. House members who want a Public Works seat will nearly always be given it. From the data in Table 1, which reports some results of interviews with committee members, it is clear that most Public Works members (60 per cent) are constituency members—congressmen who see themselves as having come to the committee to accomplish something for their districts. A few (19 per cent) are interested in "getting a log rolled" for their state delegation (the delegation members), and about one-fifth (21 per cent) are assigned to the committee because preferred seats on other committees are not available (the shuttle members).[7] Constituency members often mentioned,

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[5] Richard F. Fenno, Jr., "Congressional Committees: The Comparative View" (paper presented at the 66th meeting of the American Political Science Association, Los Angeles, California, September 8–12, 1970), pp. 2–11.

[6] *Ibid.*, p. 29.

[7] It should be noted that the large minority of members assigned to Public Works because other seats were not open contradicts the electoral benefit criterion for committee assignments developed in Nicholas A. Masters, "Committee Assignments," *American Political Science Review* 55 (June, 1961), 345–357. On p. 357, Masters asserts the following: "Although a number

Table 1. Assignments to Public Works

Congressman's Party	Conditions of Assignment[a]							
	Constituency Members		Delegation Members		Shuttle Members		Total	
	N	(%)	N	(%)	N	(%)	N	(%)
Democrat	16	6	3	14	2	9	21	56
Republican	6	35	4	24	6	41	16	44
	22	60	7	19	8	21	37	(100)

[a] Two members are excluded: a Democrat who was placed on the committee to help vote out the St. Lawrence Seaway legislation and a Republican who wanted a seat because of his occupational background.

during interviews, a particular problem or geographical characteristic in their district: "My district is bordered on three sides by two big rivers." "My district is bordered on three sides by rivers." "We need industrial development in our district worse than anything else." "Water pollution!" "Beach erosion!" "I have over 40 per cent of the water projects of [my state] in my district." "We've got to get a road in there [a part of the district]." "We have a real watershed problem." This constituency orientation is really no less important for the few members who take the seat on behalf of their state delegation: "My state has to have someone on the committee because it, in fact the whole area, has real water problems . . ." or, "We have the TVA and we have a lot of Corps work down there." Finally, the constituency aspect of a Public Works seat is evident in a negative way from the perspective of the shuttle member who hopes to move on to a new assignment: "The committee just isn't that important to my constituency . . ." or, "There really isn't anything on the committee that I'm interested in."

Image building in the district is the primary reason for being on Public Works and, as such, it has a direct impact upon transferring from the committee. Since image building is the major reward of a Public Works seat, the shuttle member will leave the committee as soon as possible, *irrespective of his rank*. Since Republicans have a disproportionate share of shuttle members, Republicans have disproportionately more transfers to other committees: if Republicans had transferred at the same rate as Democrats from the 80th through 89th Congresses, they would have had 14 instead of 22 transfers (the Democrats had 20). But not all shuttle members are able to transfer. Inevitably, then, some will become ranking subcommittee minority members—two of the top five Republicans in the 89th, 90th, and 91st Congresses. Because some of the legislation is important to their state or region, delegation members do realize some political return. Nonetheless, these members are as likely as not to transfer from Public Works; if, however, they are unable to transfer during the first four years, they will remain on the committee. Public Works thus has a nucleus of veterans—constituency members who would never leave of their own volition in addition to delegation members who stay. Even more than delegation members, constituency members refer readily to the tangible political consequences of delivering for the district.

Thirteen of these twenty-nine constituency and delegation members were specifically asked if "delivering the goods" brought them votes, and all said yes. "Public Works is a sugar committee," observed a former veteran Republican. "I could always go back to the district and say, 'Look at that road I got for you. See that beach erosion project over there? And those buildings? I got all those; I'm on Public Works.' " "It is the result of being on the committee—what they can see—that helps. They've got to see some brick and mortar," added a Democrat. Another Democrat spoke

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of factors enter into committe assignments—geography, group support, professional background, etc. —the most important single consideration—unless it can be taken for granted—is to provide each member with an assignment that will help to insure his re-election." And, on p. 354, Masters writes: "Assignment to Public Works, Interior and Insular Affairs or Merchant Marine and Fisheries are usually based on the ecological make-up of the members' districts, so as to allow them to serve their constituent interests and protect their incumbency." But since such a large proportion of members are apparently assigned to Public Works who do not want to be, it is clear that the party leadership does not systematically attempt "to provide each member with an assignment that will help to insure his re-election" (p. 357). Because of the relative abundance of Public Works seats it is impossible for the leadership to apply an electoral benefit criterion even if they so desired.

Table 2. Minority Reports on Major Public Works Bills, 84th through 89th Congresses

Reports	Water and Power	Water Pollution	Roads	Economic Development	Public Buildings and Grounds	Total Bills
No Minority Report	12	6	8	0	1	27
Minority Report	11	6	6	5	1	29
	(23)	(12)	(14)	(5)	(2)	(56)
Minority Reports Submitted by:						
Republicans Only	10	5	3	5	1	29
Democrats Only	0	0	0	0	0	0
Both Parties	1	1	3	0	0	5

for many of his colleagues when he said: "If you're going to stay around here, you've got to take care of the folks back home. . . . It's the bread-and-butter issues that count—the dams, the post offices and the other buildings, the highways. . . . You can point to all these things you've done."

Typically, then, Public Works veterans share an interest in bringing construction into their districts. Superficially, this sharing of interest suggests a pattern of favor trading in committee decision making. Party conflict, however, is the committee's dominant decision-making characteristic. Party splits are evident in the committee's internal behavior as well as in its behavior on the floor. Minority reports were filed for 52 per cent of the major legislative proposals the committee recommended to the House. As indicated in Table 2, from the 84th through the 89th Congress the minority reports were submitted on 43 per cent of the road measures, 45 per cent of the pollution control measures, one-half of the public buildings and grounds bills, 58 per cent of the water and power measures, and all of the economic development bills. Party splits are also evident in the 44 roll-call votes on committee legislation for the same congresses; a majority of each party stood on the opposite side of the issues on 75 per cent of the non-trivial roll calls—those roll calls where the losing vote was at least 10 per cent.

The Issues. Three things seem to account for this apparent inconsistency between the shared interest in projects and this pattern of party conflict: (1) the diversity of the committee's jurisdiction, (2) program goals, and (3) party affiliation. The heterogeneity of the issues channeled through Public Works makes it unlikely that any member will have a political stake in each. Not infrequently, then, little or no constituency influence bears on the decisions of many members. Moreover, as noted at the outset, traditional party issues such as power, authority of the federal government, and expenditure levels are referred to the committee. What difference would all of this diversity make in the committee's behavior if members were interested only in the projects? Not much, if Public Works members were not also interested in programs. Committee members are decidedly program-oriented. Moreover, their party affiliation, more than any other single factor, determines their policy preferences.[8]

Constituency and delegation members enjoy the subject matter; they take genuine pleasure in the legislation they handle and great personal satisfaction in their legislative accomplishments. To them Public Works is the "construction committee." Tangible "things that you can see" are, in their view, the products of its decisions. Public Works is the committee that "builds things"; the committee that harnesses the nation's water supply and promotes and protects the "greatest public works program in the history of mankind," the interstate highway system; the committee which, through its flood control and highway construction, preserves property, creates wealth, and, most importantly, saves lives; the committee that brings "comfort" and "reassurance" to flood victims because "it's important for those people to know that somebody, especially the Congress, cares." It's the committee that gets the job done; huge dams, though costlier, are much preferred to a series of small watersheds because "you can't catch one of those damn floods in a bunch of tin cans." Above all, they resent its being labeled "the porkbarrel committee." To these members Public Works authorizes only economically justified projects —"for every dollar that goes out, at least one dollar must come back."

But *what* should be built up, *what* should be authorized, and *how* it should be done are

[8] On the significance of party affiliation in the House of Representatives, see Randall B. Ripley, *Party Leaders in the House of Representatives* (Washington, D.C.: The Brookings Institution, 1967), chap. 6.

questions which often divide the parties. To begin with, there are the allocation issues; there is a remarkable disparity in the perspectives of uninterested Democrats and Republicans on allocation questions. Consider, for example, the relationship between shared interests and party conflict on the Appalachian regional development legislation. In both 1965 and 1967 the Democratic majority supported the Appalachian Democrats, whereas the Republican majority (charging "favoritism") opposed the legislation and hence the Appalachian Republicans. The uninterested Democrats thus voted for and the uninterested Republicans voted against the new federal program authorizing selective benefits. The lineup on bills such as this suggests that committee party conflict is inversely related to shared constituency interests.

This inverse relationship between the incidence of constituency interest and party conflict varies, itself, from one program area to the next. Extremes in variation between program areas are manifest in the projects of the Army Corps of Engineers projects and the authorizations for construction in the District of Columbia. Authorizations for construction in the District of Columbia are obviously as devoid of constituency influence as any measure can be. It is not then surprising to find on these issues frequent Public Works party conflict, such as the fierce battle in the 89th Congress over the vice-presidential mansion. On the other hand, the widespread constituency interest in the Corps projects provides substantial incentive for cooperation. Virtually every committee member, sooner or later, will have a project somewhere in the Corps's labyrinthine evaluation process. Like the overwhelming bulk of Corps projects, authorizing the annual statutory limit of thirty federal buildings *outside* the federal city engenders little party conflict. Similarly, no noticeable party conflict stems from the authorization of mileage for the interstate highway system. Nor did the parties ever dispute the method of allocating funds for construction of water waste treatment plants; virtually every congressman wants a plant and virtually every congressman can claim credit for one. In fact, of the 29 minority reports presented in Table 2, just one—occasioned by President Eisenhower's resistance to the 1956 rivers and harbors omnibus bill—was addressed to an allocation question in which a majority of each of the two parties had a long-standing, permanent constituency interest.

For Public Works the relative differences in shared interest from one kind of program to the next are the keys to understanding the two "faces" of the committee: party conflict and party cooperation. But the relative incidence of shared interests does not account for all Public Works party conflicts. In addition, allocation disputes, other party differences respecting finance and the amount of federal government authority contribute to conflict. In both 1955 and 1956 Democrats were adamantly opposed to the so-called "Clay-bond financing plan" to finance the proposed interstate highway system; Republicans, however, gave solid support to the plan which had been endorsed by President Eisenhower. And it has been the Republicans, not the Democrats, who have insisted that the Federal Water Pollution Control Agency share control with state capitals in funding waste treatment construction plans and policing polluters.[9] These instances are but suggestive of the kinds of issues that contribute to committee party conflict and which are, to some degree, similar to those to be found on Education and Labor.[10]

Committee Democrats and Republicans, then, cooperate on national allocation issues but are in conflict on regional allocation and traditional party government issues. The parties cooperate on allocation decisions in which virtually every member has a constituency stake, just as the two parties do on Interior.[11] Conversely, when only a minority in each party shares a constituency stake in an allocation decision, Public Works Democrats and Republicans will disagree just as in the case of Agriculture *but* for a different reason; on Public Works, there is not the coincidence of commodity interest and party affiliation.[12] When only a minority in each party shares a constituency stake on an allocation question, the question becomes a party issue because Democrats prefer to support their minority but Republicans do not.[13] Issues pertaining to the authority of the federal government or its position in the economy consistently divide the parties.

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[9] These results are reported in James T. Murphy, "The House Public Works Committee: Determinants and Consequences of Committee Behavior" (Ph.D. dissertation, University of Rochester, 1969), pp. 317–333.

[10] Richard F. Fenno, Jr., "The House of Representatives and Federal Aid to Education," in *New Perspectives on the House*, ed. Robert L. Peabody and Nelson W. Polsby, 2nd ed. (Chicago: Rand McNally and Co., 1969), pp. 286–289; Eugene Eidenburg and Roy D. Morey, *An Act of Congress: The Legislative Process and the Making of Education Policy* (New York: W. W. Norton and Company, Inc., 1969).

[11] Fenno, "Congressional Committees," pp. 25–26.

[12] Charles O. Jones, "Representation in Congress: The Case of the House Agriculture Committee," *American Political Science Review*, 55 (June, 1961) 367.

[13] These results are consistent with those on labor, urban, agricultural, and Western issues reported in David R. Mayhew, *Party Loyalty Among Congressmen*

Such are the divisive forces at work on Public Works that committee members emphasize their party affiliations at the expense of committee unity. "There are a number of things," said a veteran Democrat, "that are party matters, and you aren't going to get together on them." "It is," related a Republican newcomer, "the job of the majority to govern; the minority's job is to oppose." A high-ranking veteran Republican clarified the nature of the opposition: "We hear all this talk about developing Republican alternatives. Well, what are our alternatives? What is our position going to be?" "Frankly," added a veteran Democrat, "sometimes we have to let them know what the facts of life are. After all, they are the minority. If they want to write their bill, well, then they can go out and gain the confidence of the people and bring a majority in here and do it." Public Works members clearly value the idea of party government.

The Two Faces of Public Works: Party Conflict and Party Cooperation. The attachment to party affiliation and, further, to the notion of party government at the expense of committee unity can usefully be discussed in terms of Peter M. Blau's idea of particularistic social values—"preferences for attributes like one's own."[14] Such values distinguish non-organized collectivities such as age groups, sex groups, the electorate, or a population in a geographical territory from organized collectivities such as Democrats or Republicans or congressional committees in which the "value standards that govern the orientations and associations" within the collectivity are dependent upon *"the relationship between their status attributes."*[15] Such organized collectivities are distinguished by some collective interest or goal such as power and prestige on the House Ways and Means and Appropriations committees, both of which have the value of the greatest possible degree of *committee unanimity or consensus.*[16] Among Public Works members the attachment to party at the expense of committee affiliation suggests that Democrats and Republicans value the greatest possible degree of committee *party unity.* Further, as organized collectivities, they have mutually exclusive goals: for the majority party, the Democrats, the collective interest is the capacity to determine policy outcomes; for the minority party, the Republicans, the goal is to devise effective challenges to the majority's capacity to determine committee recommendations.

What accounts for the remarkable difference between the value of *party unity* on Public Works and *committee unity* on Appropriations and Ways and Means? Further, why is the behavior of Public Works so different from Interior's characteristic party cooperation when the constituency goal is the primary reason for wanting a seat on either committee?[17] Finally, why do the program-oriented Public Works members place such a premium on party government when the program-oriented Education and Labor members do not?[18]

The value difference between Appropriations and Ways and Means and Public Works is, I think, attributable to the relative prestige of these two top-ranking committees as against the 11th-ranking Public Works.[19] The prestige goal shared by congressmen on the Ways and Means and Appropriations committees provides a powerful incentive for them to subdue the conflict ordinarily generated by the numerous party issues brought before these committees. That is to say, these congressmen hold that some degree of committee unity is essential to committee influence—the predisposition of the House to accept their proposals. This influence, these members hold further, is necessary for the power and prestige they want from the committees. In Fenno's words: "The extraordinary effort at internal integration [on Appropriations], especially at minimizing partisanship, brings success on the floor. Success on the floor results in committee influence, and committee influence determines committee member influence."[20] And, in the words of a Ways and Means Republican: "It's the issues that are partisan, not the members."[21] It is in

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(Cambridge: Harvard University Press, 1966). On the general relationship between interdependent interests and reciprocity, see Lewis A. Coser, *The Functions of Social Conflict* (Glencoe: Free Press, 1956), pp. 75–76 and *passim.*

[14] Peter M. Blau, *Exchange and Power in Social Life* (New York: John Wiley and Sons, Inc., 1964), p. 266.

[15] Blau, p. 266 (italics added).

[16] Richard F. Fenno, Jr., *The Power of the Purse* (Boston: Little, Brown and Co., 1966), pp. 82–90; John F. Manley, *The Politics of Finance: The House Committee on Ways and Means* (Boston: Little, Brown and Co., 1970), pp. 53–58.

[17] Fenno, "Congressional Committees," p. 6.

[18] Fenno, "Federal Aid to Education," pp. 289–293; cf. Eidenberg and Morey, *An Act of Congress,* chaps. 2–3.

[19] George Goodwin, Jr., *The Little Legislatures: Committees of Congress* (Amherst: University of Massachusetts Press, 1970), pp. 114–115.

[20] Fenno, *Power of the Purse,* pp. 500–501.

[21] John F. Manley, "The House Committee on Ways and Means: Conflict Management in a Congressional Committee," *American Political Science Review,* 60 (December, 1961), 927.

this aspect, the modification of conflict to gain influence, that Ways and Means, Appropriations, and the third-ranking Rules differ from lower-ranking committees such as Public Works, Education and Labor, Interior, and Agriculture.[22] No matter how unified, no matter how successful these lower-ranking committees might be, their jurisdictions effectively preclude having as much influence in the House as the prestigious committees. The top three committees—the prestigious committees—control, after all, either directly or indirectly, the fate of nearly all legislation of all other committees—the non-prestigious committees.[23]

When congressmen, therefore, seek assignment to a non-prestigious committee, they do so for constituency goals and subject-matter interest. Many behavioral similarities and differences among non-prestigious committees can be traced to differences in the distribution of these membership goals. For example, the similarity in the distribution of constituency goals between Public Works and Interior is paralleled by similarities in party conflict and cooperation, with one major exception: the constituency stake in the legislation of both provides an apparent incentive for party cooperation, *but* on Public Works the committee is beset with party issues many of which—allocation issues—are directly related to the constituency stake.[24] Likewise, party conflict on both Public Works and Education and Labor can be attributed primarily to the subject-matter interest found on both committees; in each case, the subject-matter interest is, in turn, affected by party affiliation. That Public Works places a higher value on party government than does Education and Labor can be attributed to the differences in the issues between them. Issues processed by the Education and Labor committee are of such a nature that party affiliation does not consistently influence the preference orderings of its members as it does Public Works members. Education and Labor Democrats, for example, will be divided by constituency influence on education bills involving church-state issues; similarly, southern and northern Democrats will disagree on any union or integration issue.[25] Moreover, Education and Labor issues are typically more earthshaking than Public Works issues. Should a Public Works Democrat, for example, be opposed to an amendment for a public power project, he can take a quiet, leisurely walk; the Education and Labor member, however, will be under heavy pressure from his constituents as well as other members to make a lot of noise in openly supporting or opposing almost any issue before the committee.

Does Public Works party cooperation, as is evident in the frequency of committee unanimity in reporting major bills, suggest the committee is an organized collectivity? When interviewed, committee members consistently failed to mention a committee unity value and, further, expressed bewilderment and some irritation when asked questions designed to probe for the existence of a shared committee goal such as success, prestige, power, and others. Such questions produced only some evidence of a fraternal feeling. A veteran Republican noted, for example, that Public Works is a committee with a "great deal of camaraderie"; it is a "good committee." "A lot of that division," cautioned a veteran southern Democrat, "isn't so important or so much as it might look like. . . . They need things, too. . . . I know we helped Bill [Cramer] out on that canal in Florida." Members do expect help from one another on things in which they have a mutual interest (e.g., the Corps projects, the public buildings) or in which questions or party "philosophy" are not at stake (e.g., disaster relief). "All I require," said a veteran Democrat, "is that the project meet the ground rules." But then "that one-to-one ratio [the economic cost-benefit criterion that projects must meet to be authorized] is like putting on a pair of drawers—you stretch them a little once in a while to make them fit." Members of both parties agree that "there is a tendency to help each other out," that "you can bring these projects to fruition because you get support on both sides." This fraternal feeling suggests but a modicum of favor trading; it does not indicate a shared value for the committee suggesting the existence of an organized collectivity.

Thus far, then, the data indicate that committee Democrats and Republicans have a basis for solidarity but do not indicate what governs its degree; for party cooperation, the data indicate neither a basis nor possible determinants of variation in cooperation.

Party Conflict and Cooperation

Even though Public Works consists of two organized collectivities, there is no reason to suppose that the party unity value alone will produce a pattern of solidarity. For while it is perhaps true of organized collectivities that shared values "constitute the medium through which its members are bound together . . . [and that] they serve in this way as functional substitutes for the sentiments of personal attraction that integrate the members of a face-to-face group," the values alone are an insufficient guarantee of solidarity.[26] In an organized collectivity, just as in a small group, situations will inevitably arise in which at least one member will have an incentive to promote his interest at the risk of damaging the collective interest.[27] This insufficiency is quite clear in the case of the House Public Works committee.

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[22] The standard reference on the House Committee on Rules is James R. Robinson, *The House Rules Committee* (New York: Bobbs-Merrill Company, Inc., 1963). For committee rankings, see Goodwin, *The Little Legislatures*, pp. 114–115.

[23] Richard F. Fenno, Jr., "The Internal Distribution of Influence: The House," in *The Congress and America's Future*, ed. David B. Truman (Englewood Cliffs: Prentice-Hall, Inc., 1965), pp. 52–70.

[24] Fenno, "Congressional Committees," pp. 23–28.

[25] Fenno, "Federal Aid to Education," pp. 284–300.

For any allocation bill not every Democrat is likely to have a constituency stake; moreover, for legislation in general, there is great variability in the policy preferences of committee Democrats. Similarly, there is substantial variability in the policy preferences of committee Republicans; and, conversely, a set of committee Republicans often has a constituency interest in a bill sponsored by the majority but opposed by the minority. The dispersion of policy preferences within the parties and the presence or absence of a constituency stake constitute a direct threat to the goals of each of the two parties. For if a congressman disagrees with his party's position, or if he does not have a constituency stake in a bill, it is a perfect opportunity for him to "hold out" either to change his party's position or to barter for one or more favors in exchange for his vote. The threat of holding out is particularly obvious in the case of a majority party—such as the Democrats for all but two Congresses from the 80th through the 92nd—that is attempting to change the legislative status quo. And it becomes all the more obvious when the ratio of seats between the two parties is very close.

Values of an organized collectivity, then, provide the basis for solidarity, but they do not govern its degree. When incentives contrary to achieving an organized collectivity's goal exist, informal rules of behavior are invariably adopted. These informal rules are generally thought of as norms. Because norms specify legitimate behavior as well as provide rewards and sanctions for conformity to them and deviations from them, they can govern the degree of solidarity within an organized collectivity.[28] Since each party on Public Works is an organized collectivity, and since there exist substantial incentives for contrary behavior in each party, it is reasonable to suppose that one or more norms exist within each party.

Partisanship on the Public Works Committee. Members of each of the two parties are expected to be partisan, to use their party affiliation as the yardstick for what they ought to do. Subcommittee chairmen expect partisan reciprocity from one another and ranking subcommittee minority members are expected "to present the minority position effectively." Partisan behavior is also expected of the other veterans and of junior members as well. This means that the subcommittee chairman or ranking member is not to be "bucked," that questions in hearings and amendments in subcommittee and full committee mark-up are to be "cleared." It obviously means also that Public Works members are expected to vote with their party. Veteran Public Works Democrats are fond of quoting the late Speaker Rayburn's dictum about the rewards and sanctions of legislative life: "If you want to get along, go along." "One thing you notice right away," said a newcomer, "is that you are supposed to vote with your party." "Unless you are really thick," added another, "you pick things like that up right away. It's no problem." "It's assumed" that Democrats will go along with the subcommittee chairman. "A good reason," advised a ranking Republican member, is required to be let off the party hook. For both Republicans and Democrats, "a good reason" means that members are not expected to vote "against their district."

Conformity to this *norm of partisanship* is effected through the myriad of possible rewards and sanctions in the committee's incentive structure. When Chairman Fallon acceded to minority demands to establish an Economic Development Subcommittee to oversee the Economic Development Administration, dependable, interested Representative Ed Edmondson (Oklahoma) got the chairman's nod for the chairmanship over the more senior, interested, but not so dependable Representative Frank Clark (Pennsylvania). The late Chairman Buckley created an entirely new subcommittee

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[26] Blau, *Exchange and Power in Social Life*, p. 267.

[27] Blau, pp. 255–263.

[28] George C. Homans, *The Human Group* (New York: Harcourt, Brace and World, Inc., 1950), pp. 121–127; John W. Thibaut and Harold H. Kelley, *The Social Psychology of Groups* (John Wiley and Sons, Inc., 1959), pp. 126–142; Theodore H. Newcomb, Ralph H. Turner, and Philip E. Converse, *Social Psychology* (New York: Holt, Rinehart, and Winston, Inc., 1965), chaps, 8, 11, and 12; cf. Charles M. Price and Charles G. Bell, "The Rules of the Game: Political Fact or Academic Fancy?," *The Journal of Politics*, 32 (November, 1970), 839–855.

Table 3. Roll Calls on Major House Public Works Bills, 1947 through 1968: Differences between Northern and Southern Committee and House Democrats[a]

	House Public Works Committee Democrats:			
	Difference between Northern and Southern Democrats Insignificant		Difference between Northern and Southern Democrats Significant	
	N	Proportion	N	Proportion
House Democrats:				
Difference between Northern and Southern Democrats Insignificant	22	.34	0	.00
Difference between Northern and Southern Democrats Significant	43	.66	3	1.00
Totals	65	1.00	3	1.00

[a] *Southern Democrats are those representing districts in the Confederate states.*

H_o: Proportion Southern Democratic Yeas = Proportion Northern Democratic Yeas

H_a: Proportion Southern Democratic Yeas < Proportion Northern Democratic Yeas

Results are significant at $p \leq .025$.

in the 87th Congress—Watersheds—for the dependable but politically vulnerable Frank Smith (Mississippi) to help him in a primary battle against Jamie Whitten ("there was a longstanding need for the subcommittee [but] the timing was political"). To veteran Democrats, dependable newcomer Representative James J. Howard (New Jersey) is "just a jewel" who has a large stack of "IOUs"; a not-so-dependable newcomer, whose fiercely independent idealism led him to vote with the committee Republicans against the use of proxies and to "buck" his subcommittee chairman on other occasions is "in hot water" and "hasn't got a prayer." It is in this process of rewarding the conformist and punishing the nonconformist that the newcomer learns what is expected and the veteran is reminded.

That the norm of partisanship can have a profound impact upon committee party solidarity is demonstrated by the data in Table 3. In Table 3 the voting differences between committee southern and northern Democrats are compared to the differences between House southern and northern Democrats for all major committee bills from 1947 through 1968. From the data in Table 3 it is clear that divergences in policy preferences between southern and northern Democrats prevail in the House as a whole but not among Public Works committee members. On 96 per cent of the 68 roll calls, differences between southern and northern committee Democrats are statistically insignificant; differences are statistically significant on just 3 roll calls. House southern and northern Democrats differ significantly on 67.4 per cent, or 46, of the 68 roll calls; differences are insignificant on just 22 roll calls. From the standpoint of the norm's effectiveness, those cases in which House differences are significant while committee differences are insignificant are the most interesting. On 66 per cent of the roll calls in which committee Democratic differences are insignificant, House Democratic differences are significant. If not for the norm of partisanship, it seems reasonably clear that committee southern and northern Democratic differences would be significant on each of these 43 roll calls. The spirit of this very effective norm is perhaps captured best by the southern Democrat who, during the course of negotiations for the Highway Beautification bill in 1967, said in reference to an identifiable swing group: "We can scuttle the whole thing on ya if we are together but we sure would like to go that extra mile with ya."

It is not always possible, however, to "go that extra mile." Without question, the most obvious limitation of the norm's impact on party solidarity is the influence of constituency. This factor is apparent, for example, in the data in Table 4 in which voting differences between committee members who can be thought of as economic development Republicans (10 per cent of the population in the congressman's district eligible for benefits) and noneconomic development Republicans are compared with similar groups in the House. For the eight roll calls on economic development legislation from 1965 through 1967, the data in Table 4 clearly show that differences between the two groups of Republicans are most likely to be significant.

Table 4. Economic Development Roll Calls, 1965 through 1967: Differences between Economic Development and Other Committee and House Republicans[a]

	House Public Works Committee Republicans:			
	Difference between Economic Development and Other Republicans Insignificant		Difference between Economic Development and Other Republicans Significant	
	N	Proportion	N	Proportion
House Republicans:				
Difference between Economic Development and Other Republicans Insignificant	1	.50	0	.00
Difference between Economic Development and Other Republicans Significant	1	.50	6	1.00
Totals	2	1.00	6	1.00

[a] Economic Development Republicans are those representing districts in which at least 10 per cent of the population is eligible for benefits.

H_o: Proportion Economic Development Republicans Supporting Party Position = Proportion Other Republicans Supporting Party Position

H_a: Proportion Economic Development Republicans Supporting Party Position < Proportion Other Republicans Supporting Party Position

Results are significant at $p \leq .025$.

On 75 per cent of the eight roll calls, both committee and House economic development Republicans are significantly different from the other Republicans; on just one roll call, the committee economic development Republicans differed significantly from their House counterparts; and again, on just one roll call is there a lack of significance between the groups. Though the data in Table 4 are but a slight indication of the influence of constituency on internal party splits, its general significance is apparent in the following remarks of a committee Republican:

> A congressman from Poverty Hole, New York, or New Hampshire, or a Duncan from Tennessee, or a Clausen (who has some EDA in that long district of his) sure as hell isn't going to vote against the EDA stuff. And Bill Harsha can't be expected to vote against Appalachia. You know, sometimes we do have to rise above our principles. Someone once said that no one has repealed the law of self-preservation.

Constituency influence does control when constituency interests are at stake, but, when the parties do disagree, constituency interests are not at stake frequently enough to offset the generally controlling effects of the norm of partisanship. Hence, though constituency influence is important, it is not nearly so significant as the stabilizing effects of the norm of partisanship upon Public Works committee party solidarity.

Partisanship and Segregation Between Committee Democrats and Republicans. Party solidarity is constantly apparent in the norm of partisanship. Each congressman simultaneously holds that members in his party should be partisan, but that congressmen in the other party should be less partisan, less inclined to ignore the opposite party in preliminary negotiations, and less devoted to the practice of caucusing and party regularity. This paradoxical expectation is clearly manifest in the contradictory observations made by committee Democrats and Republicans about committee party leaders. About the two most recent chairmen, some Democrats made the following remarks: (1) A former subcommittee chairman said, "Charlie Buckley was a great fellow. He was hardly ever around here, but everyone loved him." (2) A veteran Democratic staffer noted that "Mr. Fallon is not quite so much the straight party man that Chairman Buckley was. Fallon is not at all above tripping over the traces on some Administration bills. Buckley never did." (3) A veteran Democrat noted that "Mr. Fallon is certainly not controversial." In contrast, a former veteran Republican sized up the committee under Buckley and Fallon as follows:

> I'll tell you one thing about that committee when I was on it. Buckley was the chairman, and it was a pretty damn lousy committee. He'd appoint some-

> body acting chairman and those guys would ride roughshod over us. It was straight party-line vote on everything. Now I think it is different since Fallon has taken over. My impression is that it is not quite as partisan as it used to be. I think he is more apt to give a little bit, accept some Republican amendments.

A high-ranking Republican added that Fallon is:

> a very mild-mannered man who doesn't like disagreements—and I say that with special reference to the White House. He is far preferable to Buckley. He was just terrible. He treated the minority like scum! Fallon is very different. He treats us as though we are part of the committee. They just rammed that stuff through when Buckley was chairman.

But not even Fallon received a very high score from the Republicans. At least one veteran observed that Fallon was "not exactly" his "candidate for the fairest chairman in the House." "You know," he added, "some of these chairmen have damn good reputations for being fair, and so forth. Hell, we never have enough time to file a minority report on this committee!"

Similarly, Republicans who are defended by their party colleagues elicit scathing remarks from the Democrats. A subcommittee chairman appraised the ranking minority members during his tenure as follows:

> We've had a number of ranking members since I've been here. Now, let's see, first there was a fellow by the name of Dondero. He was a real fine member, just real great. He had a lot of friends. Then we had this fellow McGregor. He was antagonistic, cantankerous, just obstreperous—he was opposed to almost anything you could bring up. Auchincloss . . . was another Dondero. He was just a prince of a guy. And now—now we've got Bill Cramer. He is another McGregor. He is always opposed.

But a veteran non-shuttle Republican noted:

> Cramer is the best; he is the best, by far, of the four. The greatest leader, the most effective one I have served under as a member of the committee. He's a fighter, a pusher. Auchincloss wouldn't fight like Cramer does. Dondero was the kind of guy that everybody loved. You followed him because you loved him. McGregor was a little bit like Cramer. He would fight. He was pugnacious. But he just wasn't as good as Cramer. Cramer is probably the brightest, the most able man on the committee.

In fact, to a man, minority members held Cramer to be a "likeable," "brilliant," member who does his "homework," and who probably is the "most effective ranking member in the House"; and, if perhaps he is somewhat "bullheaded," "adamant," or occasionally "over-zealous," it is just as well because the committee is so "partisan." Minority members note, for example, that Auchincloss was an "awfully nice guy" but that he "didn't push real hard." "Cramer," they note on the other hand, "works with us." "We have," said a veteran, "the majority's respect; they respect me now." "In the past, on this committee as well as a number of others . . . ," explained a high-ranking veteran, "the ranking minority members have worked out things with the majority and just let it go at that. The members weren't in on it, weren't informed, and that's all there was to it." But whereas Republicans praised Cramer for his vigorous leadership, Democrats consistently saw him as unnecessarily partisan —"I don't care what kind of box you put that Cramer in, he'll come out partisan every time." A veteran Democrat best captured the extreme degree of segregation between the two parties when he said: "They have some terrific guys—some that are almost Democrats."

Party Cooperation. Even though the great majority of committee members share constituency goals, they do not articulate a committee goal compatible with them. It thus appears that constituency-related issues alone cannot account for party cooperation. When an allocation decision is before the committee and the House, the truth of the matter seems to be that the two parties *distrust* each other. After all, public works projects are inherently political and, as such, are presumed to have substantial electoral significance. Since each party values majority status, each expects the other to porkbarrel—to authorize and appropriate projects on the basis of political clout or favor—when the other is in the majority. Because they foresee the possibility of partisan allocations and because they take the allocation of public works benefits so seriously, congressmen insist on a fair allocation of the goodies. Hence, they have agreed upon permanent ways to reduce electoral risks when a majority in each party is affected by an allocation. Such agreements are impossible when a minority in each party is affected, because Republicans, as noted previously, consider any such allocation inherently unfair. Moreover, agreements are probably unnecessary because Democrats will vote with their minority as a matter of party allegiance. Only when a majority in each party benefits from an allocation can party cooperation be expected. Since committee Democrats and Republicans are confronted with exactly the same situation as House Democrats and Republicans, the electoral risk-reducing devices explain party cooperation in the committee as well as in the House.

Each member of the House expects public works allocations to be applied across the board without exception. Congressmen expect "fair" treatment for their district, city, state, region, and party. This *expectation of equity* is somewhat parochial, being invoked in debates on public works bills from many different vantage points. The New York delegation, for example, was convinced that New York State should be reimbursed for the interstate highways built in New York prior to the Interstate Defense and Highway Act of 1956. Accordingly, they supported Chairman Buckley's 1959 amendment providing for such a "reimbursement" since it was but "an equitable giving-back of something . . . unfairly taken."[29] But the unmoved delegations from the states who would have to forego the Highway Trust Fund allocations to benefit New York defeated the amendment. Some members of the Pennsylvania delegation opposed the 1957 authorization for the Niagara Power Development Project. The districts these members represented were outside the geographical limits (150 miles for the New York Power Authority) imposed on the distribution of power.[30] Likewise, some members representing districts within the range of the TVA opposed the limitation imposed on TVA power distribution. (The limitation was the asking price for the TVA bond revenue measure which made the TVA self-financing.)[31] Similarly, urban Democrats were less than pleased with the Public Works and Economic Development Act of 1965, because its eligibility criteria effectively sliced out the cities.[32]

Notwithstanding this expectation of equity, Congress has adopted a number of proposals opposed by a minority. Moreover, some of these proposals such as the TVA bond revenue bill, the Niagara power bill, and the Public Works and Economic Development Act of 1965 benefited only a minority in each party, and each bill was opposed by the Republicans. Such majority programs or small bundles of projects, as Turner found, divide the parties.[33] The first several Appalachian regional development bills which consistently split the committee and House are typical of this kind of party division. Though a bill was reported out in 1964, there were not enough votes in the House to call it up. With the influx of 67 Democratic freshmen after the 1964 election, however, the Democrats were able to report out a modest bill which passed by a vote of 257 to 165. The partisan distrust engendered by such majority party behavior is manifest in the remarks of two Republican party leaders during the floor debate on the 1962 Accelerated Public Works Act—the bill did not provide safeguards against partisan porkbarreling but could, conceivably, apply to a majority in each party. Former Minority Leader Charles A. Halleck (Ind.) tagged the measure "a $900 million political slush fund for the President."[34] To this, former Rules veteran William H. Avery (Kan.) added the measure was "a precedent for political boondoggling" and, mixing metaphors, threatened the Democrats by saying: "You are in the majority now. I suppose you want to play games with this. But some day it might be on the other foot. Some day we might be looking the other way."[35] Since neither party can assume it will be the majority party each congress, there is substantial reason for each party to distrust the other when a majority in each can benefit from an allocation.

Congressmen have, therefore, reduced electoral risks by adopting fixed allocation formulas. Most public works allocations are thus so routine as to suggest automation. A few examples, all of which fly in the face of the image of public works legislation as porkbarreling and of Public Works committee members as dispensers of political goodies, will convey the flavor of the routinization: (1) the Army Corps of Engineers' projects (the most frequent recipient of the porkbarrel label) must have benefits equal to or greater than their costs to qualify for authorization; (2) of the 41,000 miles authorized in the 1956 Interstate and Defense Highway Act, just 1,000 are susceptible to purely political influences, the balance of the mileage being allocated according to the 1947 plan of routes; (3) one-half of the water pollution control dollars are allocated to states in direct proportion to population and the other one-half is allocated in inverse proportion to income.[36] By no stretch of the imagination do these fixed allocation formulas *guarantee* boodle to *any* member of the committee or House; the formulas guarantee *only* fair treatment irrespective of party affiliation.[37] And just as these electoral risk-reducing allocation formulas explain committee party cooperation, membership goals explain the relatively infrequent, mild departures from the fixed rules. Though the committee cannot adopt its own set of rules, the cost-benefit ratio is "stretched," some "political miles" are gained, or an economic development project gets placed on the agenda for a committee member irrespective of party.

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[29] *Congressional Record*, 105, p. 17968.

[30] *Congressional Record*, 103, pp. 13196–13211.

[31] *Congressional Record*, 105, pp. 7703–7728; N.B. 7703–7705. See the discussion of the Democratic erosion of support in Aaron Wildavsky, "The TVA and Power Politics," *American Political Science Review*, 55 (September, 1961), 576–590.

[32] *Congressional Record*, 111, pp. 20240–20244.

[33] Turner, *Party and Constituency*, pp. 51–53; p. 70.

[34] *Congressional Record*, 108, p. 17333.

[35] *Congressional Record*, 108, p. 17927.

[36] For the Corps cost-benefit ratio, see Inter-Agency Committee on Water Resources, Subcommittee on Evaluation Standards, *Proposed Practices for Economic Analysis of River Basin Projects*, 2nd ed. (Washington, D.C.: Government Printing Office, 1958); for

Committee Success and Influence

Since Public Works is a mixture of party conflict and cooperation, the committee's behavior presents an unusual opportunity to compare success on the floor of the House under two radically different conditions. Is the House as likely to adopt Public Works proposals when the committee is united as it is when the committee is divided? And, in either case, to what can the success be attributed? For example, should either accurate anticipation of House demands or committee influence be inferred from success? Finally, does the meaning of success for committee-House relations depend upon whether the committee is united or divided?

Committee Success. The House typically adopts Public Works committee recommendations whether the committee is divided or not. From the 84th through the 89th Congresses, Public Works succeeded in having adopted 44 of the 56 major bills reported. For all this legislation from 1955 through 1966, the House was more likely to consider measures having unanimous support of the committee than those having only the support of the majority party. Of 27 bills reported by a united committee, the House considered 25; of 29 bills reported by a divided committee, the House considered 20. For the bills considered during the period in question, the House was about as likely to adopt proposals having bipartisan support as those supported just by the committee majority. All 25 bills having bipartisan support were adopted; and all but one of the 20 bills having only partisan support were adopted. Taking account of all the bills reported, the House adopted 91 per cent of the proposals supported by committee Democrats and Republicans but just 68 per cent of the bills supported only by committee Democrats.[38]

Most committee Democratic defeats—9 out of 10 bills defeated—occurred because bills were not called. Since at least some bills are reported as part of a long-term strategy, committee majority party success is actually higher than 68 per cent. And when reported bills are called up, the committee Democrats are just as successful—as measured by the number of amendments adopted—as the Democrats and Republicans are together. From the 84th through the 89th Congresses, when the committee was divided, the House adopted just two of the fifty-nine committee minority (Republican) amendments. All told, the House adopted only eight—five Republican and three Democratic—of a total of seventy-four floor amendments opposed by the committee majority. When the committee was united, just two out of eleven contested floor amendments were adopted. Such has been the committee's success.

What does this success indicate about committee-House relations? There is, first, a remarkably striking relationship between the behavior of committee Democrats and Republicans and House Democrats and Republicans. When Public Works is divided by party, there is a very strong relationship between committee party unity and House party unity as measured by the roll-call votes. On the 68 roll calls on major bills from the 80th through the 90th Congresses, the correlation coefficient, *r*, between committee and House Democrats is .959, and the coefficient between committee and House Republicans is .881. These unusually strong correlation scores are taken from regression equations used to "explain" House party unity as a function of committee party unity. Squaring these coefficients of correlation, r^2,

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highway legislation generally, see U.S. Department of Commerce, Bureau of Public Roads, *Federal Laws, Regulations and Other Material Relating to Highways* (Washington, D.C.: U.S. Government Printing Office, 1965); for water pollution control legislation, see U.S. Congress, Senate Committee on Public Works, *A Study of Pollution—Water,* Committee Print, 88th Cong., 1st sess., June, 1963.

[37] Since all Bureau of Reclamation projects reported out by the House Interior Committee must go through exactly the same process as the Corps projects, the explanation for party cooperation on Public Works should be just as valid for party cooperation on Interior. And since the Bureau of Reclamation projects—a national allocation issue—constitutes the overwhelming bulk of Interior's legislative proposals, mutual partisan distrust should account for Interior's party cooperation, the most salient characteristic of its behavior. See Fenno, "Congressional Committees," pp. 36–37; also see Inter-Agency Committee on Water Resources, Subcommittee on Evaluation Standards.

[38] Only one of the 12 bills lost was actually called up and defeated.

yields the total amount of variance explained in House party unity by committee party unity. The total amount of variance thus explained is 91.5 per cent for Democrats and 77.2 per cent for Republicans. Public Works committee party unity is then an unusually reliable predictor of House party unity.

Because Public Works committee party unity is such a good predictor of House party unity and because committee decisions temporally precede House decisions, it is tempting to speculate on whether or not committee party unity *determines* House party unity. Adding to the temptation to speculate about a causal relationship between committee party unity and House party unity are images, often found in the protocol data, of committee members trying to win votes in the House before bills are called. A very high-ranking Democrat said: "If a man delivers his state delegation, he has done a good job." Persuasion is often a necessity. Because of the kinds of issues which occasion committee party conflict, the committee majority cannot use boodle to generate support in the House; committee Democrats must persuade most House Democrats to vote for bills that do not have constituency benefits for them. Obviously, the necessity for partisan persuasion is occasioned by division. Committee division often occurs in the context of legislative discontinuity in the form of new programs or significant new departures in old programs. But much of the committee's legislation has a remarkable continuity. And when this legislative continuity is taken into account, the temptation to infer causality from committee success and the relationship between committee and House party unity is not appealing. Legislative continuity on public works legislation is accompanied by party cooperation. Party cooperation usually means that there are widespread shared interests in the boodle and fixed allocations which make House demands unusually explicit. And these demands temporally precede committee deliberations. It would thus appear that when there is legislative continuity the committee might well be limited to anticipating House demands; when there is legislative discontinuity, however, it might be possible for the committee majority to exercise influence.

Cooperation and Anticipation: The Corps Projects. Of all the Public Works committee's legislation, the Corps projects have the greatest continuity and perhaps best reflect the committee's problem of anticipation. Corps projects are considered by Public Works on a project-by-project basis. A project's life begins with a congressional request for a Corps study. Such requests set in process a series of decisions which, for projects surviving each stage, span an average time of eleven years and eight months. Besides a multitude of Corps decisions up and down the line of authority, the committee and the House each make two decisions vital to the projects' success. Public Works, acting in light of the Corps' recommendation based on a feasibility study, decides whether or not a requested project should be surveyed. If, by committee resolution, a project's survey is authorized, the Corps surveys the project and makes appropriate recommendations to the committee. If the committe decides to recommend the project for authorization, it goes into the large bundle of projects known as the "omnibus rivers and harbors bill." These omnibus bills are usually considered biennially.

At each stage in this authorizing process, a project's worthiness is measured in terms of the cost-benefit ratio. To survive the authorizing process, a project must have benefits equal to or greater than the costs of construction and financing.[39] "The Corps evaluation," in the words of the late Clem Miller, "is a bundle of tangibles and intangibles: 'local interest,' 'cost-benefit ratio,' 'commercial use,' etc."[40] If a project is "economically justified" it will be approved unless its benefits are a matter of dispute. Benefits often become a matter of dispute when either the railroads (calculation of navigation benefits) or the private utilities (calculation of power benefits) question the Corps' calculation. Railroads, for example, opposed Rep. Michael J. Kirwan's (D. O.) Lake Erie and Ohio Canal project ("Mike Kirwan's Ditch"). Since the project offered a competing mode of transportation, the railroads were quick to note that, if benefits were calculated differently, the project would not qualify. Though disputes occasionally arise, there is a long-standing, solid base of support for the ratio in the House. Almost every member of the House, at some point in his career, will have one or more projects in process. "There is nobody, not a member of the House, that is not interested in water. . . . Nobody escapes," explained a subcommittee chairman. The support stems from the idea of a *cost-benefit criterion* for *each* project.

But the support is not just for a cost-benefit ratio. The support is for a specific cost-benefit ratio—namely, unity. Unlike the specifics of benefits and costs, the idea of the ratio and the unity criterion is apparently fixed. Efforts to change the unity criterion have been unsuccessful. Illustrative is the House's response to an amendment offered (85th Congress, 1st session) by a former Appropriations member, Rep. Melvin Laird (R., Wis.) to increase the limit for funding survey studies from 1:1 to 1:2:1.[41] Besides eliminating a number of surveys, the Laird amendment called for a change in the rules of the game. Predictably, the House objected. Floor manager of the bill Rep. Joe L. Evins (D., Tenn.) held the amendment to be "unwise and certainly most unscientific."[42] Rep. Ed Edmondson (D., Okla.) held it to be "an unscientific and unrealistic approach to the problems of flood control" and added that the amendment "would interfere with urgently needed programs in 11 of the 48 states at once.[43] Rep. Glenn W. Andrews (D., Ala.) noted that the amendment would eliminate a project in his district (on the Chattahoochee River) which had a "favorable cost ratio."[44] Rep. Wright Patman (D., Tex.) opposed the amendment because its adoption would eliminate "a very constructive and useful project in the district . . . [he had] . . . the honor to represent—the Cooper Dam and Reservoir."[45] Reps. Everett P. Scrivner (R., Kan.) and D. S. Saund (D., Calif.) observed, respectively, that the proposed change in the unity criterion was "arbitrary" and "unfair on the basis of all that has been done before."[46] It is undoubtedly impossible for Public Works to change the ratio. It was noted by the 1953 Subcommittee to Study Civil Works (the Jones committee) that "a project cannot be expected to create wealth unless the benefits are clearly in excess of the costs."[47] But no bills were reported and no committee resolutions were adopted to implement the Jones committee's recommendation. A veteran Republican exclaimed: "There would be hell to pay if we tried that!" when the author inquired if it was possible to change the ratio from 1:1 to 3:1. Such a change would exclude dozens of projects.

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[39] For an extended discussion of the authorization process, see Arthur Maass, *Muddy Waters* (Cambridge: Harvard University Press, 1951), pp. 22–24.

[40] Clem Miller, *Member of the House* (New York: Charles Scribner's Sons, 1962), p. 16.

Public Works, as manifest in the Corps legislation, would undoubtedly never be able to change any of the national allocation formulas. And the committee members apparently know this. They thus feel obligated to defend the unity criterion in the cost-benefit ratio and are unwilling and indeed probably too wily to recommend changes in the ratio or any other fixed formula such as the highway allocations. This inability to propose successfully any changes suggests House dominance over the committee—a relationship in which the committee must anticipate House reactions in order to be successful. "If," said a committee staffer, "we ever reported a Corps bill with only about twenty projects in it, every member who didn't have a project in the bill would be jumpin' up and down and hollerin': 'Where's my project?' " The Public Works committee is thus constrained to report bills—omnibus bills—that will receive widespread support. These omnibus bills, which typically have unanimous committee support, are paradigmatic for legislative proposals affected by the fixed allocation formulas. So, then, Public Works committee success on unanimously reported legislation means that the committee is doing what the entire House expects it to do.

Conflict and Influence: Appalachian Regional Development. While the Corps projects are paradigmatic for continuity in public works legislation, the Appalachian Regional Development proposals have been paradigmatic for public works' legislative discontinuity. Proposals for developing the Appalachian region are relatively new and sporadic. And because the Appalachian legislation benefits only a minority in each of the parties, it has generated party conflict. The committee majority party must, therefore, depend on the strength of the House majority party, most of whose members have no constituency stake in the legislation.

Differences in the two Appalachian proposals reported by Public Works in 1965 and in 1967 correlate with the decrease in House Democratic party strength from the 89th to the 90th Congress. The correlations of this decrease in Democratic strength with differences in the legislation do suggest anticipating and eliminating opposition. At the same time, however, reenacting a program that benefits only a minority of congressmen in each party suggests a certain degree of influence.

Democrats, it will be recalled, reported out but could not call up the Appalachian bill. With the influx of 67 freshmen as a result of the 1964 election, however, Democrats passed such a bill in the 89th Congress. But in the

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[41] *Congressional Record,* 103, p. 9710.

[42] *Congressional Record,* 103, p. 9710.

[43] *Congressional Record,* 103, p. 9712.

[44] *Congressional Record,* 103, p. 9713.

[45] *Congressional Record,* 103, p. 9713.

[46] *Congressional Record,* 103, p. 9713.

[47] House Committee on Public Works, Subcommittee to Study Civil Works, *The Civil Functions Program of the Corps of Engineers, United States Army,* House Committee Print No. 21, 82nd Cong., 2d sess., p. 35.

1966 elections Democrats lost a net of 47 seats, mostly outside the South. Committee Democrats suffered a net loss of 34 supporters of the 1965 bill, bringing the majority for a 1967 bill uncomfortably close to the necessary 218 votes for passage—a change from 257 to 223. Significant changes thus had to be made from the 1965 to the 1967 legislation. The anticipation of opposition in these changes is quite striking.

The 1965 bill (S. 3) authorized a total of $1,092,400,000—$840 million for highways and $252.4 million for public works and other federal aid programs. These dollars were for Appalachia as defined in S. 3—all of West Virginia, most of Pennsylvania, and parts of New York, Ohio, Virginia, North and South Carolina, Georgia, Alabama, Tennessee, and Kentucky. As noted above, much of this support for the 1965 bill (89th Congress) was not available when Public Works began consideration of the 1967 bill (S. 602). This bill increased nonhighway authorizations by $185 million and provided for $175 million in additional highway authorizations for the Appalachian region. In addition, the Senate version contained a Title II which increased the monetary authorization for regional commissions authorized by the 1965 Public Works and Economic Development Act. The key to the bill's success, however, was the new definition of the Appalachian region: one new county in New York, two in Alabama, and 18 in Mississippi. These new counties were added without commensurate increases in the monetary authorization levels. The increases in nonhighway authorizations were in the bill as originally introduced, and the new highway authorization pertained only to two roads in Pennsylvania and New York.

By effectively holding the authorization level constant while expanding the number of districts eligible for Appalachian dollars, the committee successfully reported the bill. Rather than delete the counties and reduce the basis of support, Public Works Democrats kept the new counties. Retaining these counties provided a number of strategic advantages. For one thing, the committee avoided a tough conference fight. Such a fight would have been a certainty because Senate Public Works Committee Chairman Jennings Randolph (D., W.Va.) was firmly committed to Senator John Stennis of Mississippi to keep the counties in the bill. Keeping the new counties in the bill made it easier for committee Democrats to add still another county in Tennessee. This new definition of the Appalachian region correlates with four votes picked up in the Mississippi delegation. More important than the four votes, however, Mississippians spearheaded the drive among southerners to salvage the bill. And the other provisions unquestionably helped. The change in two Republican votes in New York State, for example, is related to the road allocation for New York and Pennsylvania. With the large majority in the 89th Congress, committee Democrats could get along without the Mississippians and possible Republican support picked up in the new provisions. But in the 90th Congress support of the Mississippi delegation along with new support such as that from New York and Tennessee was crucial.

As is apparent in these Appalachian bills, when the committee Democrats and Republicans are divided, the Democrats often move on their initiative. In such cases, the fate of committee majority party proposals indicates a mixture of House dominance and committee majority party influence. To the extent that adjustments must be made to win support, the House clearly dominates the committee. But there is substantial autonomy in that the committee majority is successfully reporting legislation appealing only to a minority of the House. Obviously, committee Democrats must depend on party loyalty for their success. That committee Democrats can take advantage of party loyalty means they can successfully report legislation without resorting to log-rolling. For the most part, then, majority party success in the Public Works committee means the majority party is generating legislative programs to which *it* is committed, not necessarily to programs to which a majority of the House is committed.

Conclusion

This analysis of the House Public Works committee against a backdrop of the behavior of other congressional committees has assessed the significance of the relationship between the issues and the distribution of membership goals with respect to the degree of party conflict; the significance of the relationship between norms and committee party solidarity on the one hand and the relationship between electoral risk-reduction and party cooperation on the other; and, finally, the response of the House of Representatives to committee party conflict and cooperation.

To a degree unknown in any congressional committee studied to date, the House Public Works committee is a partisan committee. As it is generally considered to be, Public Works is a constituency-oriented committee. But it does not follow that Public Works is an essentially bipartisan, backscratching committee. Far from it, Public Works committee members, besides being constituency oriented, are program oriented. As program-oriented legislators, they are inclined to distinguish good programs from bad programs on the basis of their party affiliation. In addition to this party-influenced program orientation, the constituency-oriented allocation issues are so diverse that Democrats and Republicans often do not share a constituency stake in any particular piece of legislation. Moreover, many issues processed by the committee are not allocation issues at all but are traditional party issues such as the role of the federal government. For all these reasons —program orientation, party affiliation, diversity of the issues—Public Works Democrats and Republicans are driven apart. The parties are thus in conflict, and each party seeks as much internal unity as possible and each thus has a norm of partisanship designed to ensure unity.

Porkbarreling has been challenged as an explanation for Public Works authorization because it cannot possibly account for the fixed allocation formulas. That is to say, the fixed formulas, the safeguard against partisan porkbarreling, guarantee an equitable distribution, a distribution uninfluenced by considerations of power or party. To explain public works outcomes on large national allocation bills, one first notes the party cooperation, then the fixed formulas, and then searches for an explanation of the formulas. The explanation offered here for party cooperation on omnibus public works bills is partisan distrust. Because the broad, national allocation of public works has such enormous electoral significance, neither party can entrust its fate to the other when it is in the minority. Hence, the allocation formulas.

Notwithstanding a general expectation of equity in the House, partisan considerations do influence outcomes on legislation more selective in scope than the national allocation bills. This has been particularly true of the economic development legislation of the 1960s and early 1970s, which benefits a minority of Democrats and a much smaller minority of Republicans. Democrats are apparently willing to be partisan on such legislation because they know that when Republicans are in the majority they will not advance programs designed to benefit a minority of House Republicans.

The committee patterns of conflict and cooperation and the associated House responses demonstrate that committee success is not perfectly correlated with committee unanimity and that it is treacherous to infer committee power or autonomy from success. When the committee is united, the success appears to be attributable to an accurate response to House demands; when the committee is divided, however, its success is attributable to both accurate responses and autonomy. Unfortunately, there is no obvious, feasible method of separating out the effects of the two factors. Nonetheless, this mixture of party division, influence, and success does run counter to previous assertions and conventional wisdom.

APSA PRESIDENTIAL ADDRESS, 1973

Culture and the Comparative Study of Politics, or The Constipated Dialectic

ROBERT E. WARD

Introduction

LUCIAN W. PYE
Massachusetts Institute of Technology

In recent years the American Political Science Association has greatly increased the burdens and obligations of its President, but there remains a traditional prerogative of the President which we deeply respect, and this is his annual privilege to stand before us and lecture us on any topic of his choosing. Before allowing him, however, to exercise this right we do require that he suffer through a public recital of what has made him, in our judgment, such a worthy.

(I must in all conscience warn of impending crisis if we don't check the tilt in the ratio of headaches to heartwarmings that go with the job of President, for this year our President was overheard musing about the advantages of foregoing the presidential address, an obvious ploy to embarrass us by suggesting that the office was becoming all work and no honors.)

Now for the recital of virtues:

For the first time in its history the Association has honored a specialist in Asian politics, and in doing so it has chosen well, for Robert E. Ward has also been selected President of the Association of Asian studies, and consequently he has the extraordinary distinction of serving concurrently as president of two of America's major scholarly associations. I like to think that Robert Ward's masterful and elegant handling of the Business Meeting, and his clear articulation of our convoluted procedures can in part be attributed to his orientalist's background.

It was not that he was a lonely tea drinker among the coffee drinkers of political science which brought him to his commitment to Japanese studies but rather World War II. Fortunately for Political Science, his highly developed sense of the work ethic prevented him from discovering at the onset of his adulthood that an equal, if not more congenial, setting for honing his fine sense of comparative analysis could have been the vineyards and cellars of France. What might so easily have been an honorable vocation turned into a most gracious avocation. Meanwhile, for more than 25 years, Bob Ward devoted himself to building at the University of Michigan an internationally respected Center of Japanese Studies. During this period while constantly facilitating the work of others, he himself engaged in field work which culminated in vivid, straightforward studies of Japanese politics from the village and prefect level, to the party system and ultimately at the highest reaches of constitutional development. The Japanese have recognized Robert Ward as an authority on their politics and have made him a charter advisor of the new Japan Foundation.

During these years of commitment to the work ethic, he allowed his extraordinary talents as a tennis player, which had once put him on the Stanford team, to deteriorate grievously. Fortunately, friends have rallied about and brought Bob back to his senses so he has been able in the nick of time to revive his game, and thus prove that not just in matters Japanese is he the master of his students and colleagues.

This year Bob Ward has returned to his alma mater and to California, his childhood home. Many people will have difficulty picturing Ann Arbor without him, for he was an institution there. For many it is still inconceivable that he has moved, but a few people, aware of Bob's recently developed skills as a bird watcher, sensed that in time he might become restless for the sight of new varieties and species. Others see in his move a sign that California wines have finally arrived.

Robert Ward has assumed so many duties of the scholar-statesman that it is impossible to list them, yet not to mention some of his positions would be to neglect those qualities of judicious wisdom, even-handedness and fair-mindedness which so epitomize Bob's character. In recent years as Chairman of the Board of Directors of the Social Science Research Council he brought together the concerns of many fields in an effort to maintain the vitality of one of our few national, multidisciplinary institutions; as a member of the Board of Foreign Scholarships he has defended the interests of academicians in the Fulbright Program; and during this last year he has expended endless hours trying to educate Congress and the Administration about the importance of area studies for this country as he has valiantly led the struggle to save the NDEA programs. His extraordinary success in winning over skeptical congressmen, senators, and administration officials must constitute one of the great acts of political advocacy by a scholar for purely scholarly goals.

How Robert Ward has avoided being a dean or a college president can only be explained by the fact that he combines with his judiciousness a lot of good sense and a proper scale of values. Also he has a basic commitment to the elaboration and evaluation of ideas. His great analytical gift has been his ability to take an elaborate theory and cast away the verbal chaff and find the few grains of enduring value. His unpretentious contributions to the intellectual work of the SSRC Committee on Comparative Politics will, in my judgment, be recognized as among the most parsimonious statements of those elusive concepts, political modernization and political development.

We are all indebted to him for all that he has done in so many quiet ways for the discipline of political science, not the least of which was his imaginative choice of New Orleans for this meeting. I happily present to you, President Ward.

Culture and the Comparative Study of Politics, or the Constipated Dialectic*

ROBERT E. WARD
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It may well be that presidential addresses are read largely, perhaps exclusively, by subsequent presidents when the arts of procrastination finally fail and they in turn are driven to confront the doleful prospect of writing their own address and, in the course of so doing, are moved to seek a feel for what has gone before. At least this has been my circumstance. I have browsed these presidential pastures, usually with profit, frequently with pleasure, and, occasionally, with real delight. On the basis of these forays I would recall to the memories of some of you and commend to the attention of all others at least one such prior performance. Its style is literate, its wit well-honed, and its message compelling if not comforting. It is entitled "An Obituary of a Political Scientist" and was delivered twenty-three years ago to the Southern Political Science Association by our colleague, Jasper Shannon.[1]

Dr. Shannon's address was an allegory—poignant, mocking, bitter and yet funny, exaggerated but possessed of a discomforting core of truth. It consists of autobiographical fragments from the writings of Caspar Don Quixote Milquetoast, a recently deceased and not altogether mythical professor of political science—writings which, in microcosm, trace Dr. Milquetoast's own career and shifting professional commitments and, in macrocosm, reflect the "development" of the discipline between 1917 and 1950, a dark and pre-scientific age known to most of us only by dim repute.

Professor Milquetoast has an impressive albeit dwindling capacity for enthusiastic and wholehearted commitment to the reigning professional passions of the discipline. The flavor is well conveyed by a passage from his writings dated April 10, 1917:

> Political Science is a new study, one whose frontiers are scarcely clear. Reflection and investigations reveal certain laws of politics as clear and definite as those of physics. First of all, it cannot be disputed that the whole trend and tendency of human relations is towards more democracy. Man is an incurable political animal. The evils which have crept into democratic governments are the result of too little democracy. Without question the cure for the evils of democracy is more democracy. The rebirth of American democracy is on its way with the introduction of the initiative and referendum, the development of proportional representation in cities, the movement for the short ballot and the growth of the direct primary, together with federal control of Big Business monopolies. As we look back to the dark days of the last half of the nineteenth century, it is evident that the vision of democracy held by Jefferson and Lincoln was only temporarily obscured. La Follette, Bryan, Theodore Roosevelt, and now one of our own professors of political science, Woodrow Wilson, have made clear the truth which Rousseau undertook to demonstrate a century and a half ago; namely, that human nature is basically and fundamentally good though it has been temporarily corrupted by political institutions. It is true that some critics have wanted to know who created these institutions if not men; and if men were good, why did they create them? This is unworthy of the true student of government for we have simple and conclusive evidence of the contrary in our contemporary world. The present war in which we are now unfortunately engaged is showing once more the inevitable progress towards the perfection of democracy.
>
> When this war is over, undoubtedly some kind of world organization will arise and, with American Democracy in the lead, we will move towards a fuller and more complete freedom. The triumph of democracy is only a question of time.

A marginal notation added in December, 1923, indicates that: "This chapter needs revision in the light of subsequent developments in the Harding Administration."

The entry for 1925 reads:

> It is more and more clear that governmental problems must be solved at the grassroots. The principal issues of government are essentially those of administration. Government is not only a police agency, but it is a service agency as well. The trend of the time is towards urbanization and centralization. The census returns show that rural America is passing. We must make our cities more healthful, more beautiful, more livable. This demands the integration of power in the hands of

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* Presidential address delivered at the Sixty-Ninth Annual Meeting of the American Political Science Association at the Jung Hotel, New Orleans, Louisiana (September 6, 1973). Dr. Ward is Professor of Political Science and Director of the Center for Research in International Studies at Stanford University.

[1] *Journal of Politics,* 13 (February 1951), 3–18. Quoted by permission of the author and the *Journal of Politics.*

> one man who can give us better city government. The invention of the city manager plan combined with the employment of a merit system in government is what is essential to sound local government. Efficient business practice must be applied in city government even as it has been in our well-run corporations. Businessmen must play a larger role in government so that their organizing and administrative talents may be brought to the solution of political problems, just as they have so competently improved our standard of living.

This, too, bore a subsequent annotation dated November, 1929: "My confidence in business efficiency is somewhat shaken."

A note of doubt and discontent creeps into our now unemployed hero's credo in 1933:

> In previous chapters I have neglected the importance of economics for politics. A perusal of Charles A. Beard's *The Economic Interpretation of the Constitution,* his *Economic Basis of Politics,* or his more recent volumes, *The Rise of American Civilization,* will convince the most stubborn mind that economics is the clue to politics. The only trouble is the way economics is taught. Professors of economics are generally as dismal as their subject and are creatures of little imagination who have become slavish idolaters of the capitalist system. They are proof of Plato's contention that we become like that which we study. Congressional investigations have showed that much of the economic order is wrong. It will have to be turned upside down. We need to study the Russian system more closely. They have solved the problem of unemployment while millions here are near starvation.

But then after four years as a public administrator with WPA, Caspar concluded in 1937:

> . . . I confess my feelings are somewhat contradictory . . . I have always been an opponent of monopoly, but I had thought only of business monopoly. My experiences in government lead me to question the total monopoly of government as much as the partial monopoly of business. I suspect my senses are now more keenly attuned to the doctrine of the separation of powers than formerly . . . Bureaucracy really exists and is not solely a chimera of the Republican National Committee . . . Why I never saw the close relationship of the welfare and police state I do not know . . . It is really tragic what we political scientists do not know. I think I will turn to psychology for help . . .

Needless to say he did and by 1939 found that

> all they [the psychologists] have told me is that life is a dream and a bad one at that . . . Psychoanalysis has told me little about political science but much about political scientists.

Thereafter our hero staggered on towards 1950, death, and final repose, insatiable in his quest for the Holy Grail of professional truth. At points en route he concluded variously. In August, 1945:

> . . . My life, everything, is now dependent upon the ability of a Missouri ex-necktie salesman in Washington and an ex-divinity student in Moscow. Not even the gremlins could have created such a condition . . . Honestly, I am beginning to have a nostalgia for Calvin Coolidge.

And, finally, in August, 1950:

> Occasionally I feel an urge to be an historian, a mortician of the past, for hindsight is so much more helpful than foresight. Thirty years ago I mounted my good horse and rode off to war against politicians. Now I pray for more and better politicians. I seek not uniformity and efficiency, but cohesion and peace. These can come only through compromise . . . I am tempted to say, "Blessed are the politicians for they are the architects of peace. Oh, Lord, save us from preachers, generals, idealists, educators, and administrators!"

The fragments end with an injunction to his wife: "Please destroy this manuscript . . ."

Thesis and Antithesis: Behavioralism Versus the "Area Approach"

Dr. Shannon brings perspective and humor as well as irony to bear on a question that too seldom engages our systematic attention: "How over time does the discipline change?"—against such a background, I dare not say "develop." In an immediate sense we are all in varying degrees aware of and sensitive to the disciplinary fads, fancies, and feuds of the moment. But memory is a selective faculty—perhaps mercifully so—and we seldom reflect on these sometimes painful matters in deeper perspective or in much detail. Were we to do so, I suspect we would discover a phenomenon that I hereby denominate as "the constipated dialectic." It has theses and antitheses aplenty, but whether it leads to synthesis remains questionable.

I would speak to you this afternoon about a particular instance of just such a "constipated dialectic" and do so with cathartic ends in mind. The context is that of comparative politics, the general topic is the practical implications and consequences of cultural differences for the comparative study of politics, and the specific thesis and antithesis involved are the oft-alleged incompatibility of what, for shorthand purposes, I will call respectively the area and the behavioral approaches to the comparative study of political systems.

What is the nature of the problem? As best

I can tell and reduced to its simplest and most basic elements, it seems to be as follows.

Many behavioralists allege that something called "the Area Approach" lacks rigor and scientific potentiality. It is viewed as descriptive and relativistic, often historical or institutional rather than behavioral in focus, and normally idiosyncratic in terms of its findings. In current professional parlance these are not terms of praise.

A few so-called "area types" respond with counter-charges of cultural illiteracy, gross ethnocentrism, uncritical scientism, and scornful characterizations of those members of the opposition who venture abroad as "itinerant methodologists" or worse.

And thus, thesis confronts antithesis and battle is joined.

The fray is a strange one, however, which is explicable more in psychoanalytic than in objective terms. To begin with, the area camp is in considerable disarray. Even in a physical sense it is by no means a unitary force. There really is no single area movement, approach, or method comparable in any sense to even so diffuse a phenomenon as the behavioral movement or approach. What actually exist are very loose congeries of academics united only by a common interest in particular areas such as Japan, China, India, Slavic Europe, or Latin America and lacking any common identity or organization that transcends such specific regions or cultural areas. They certainly do not share anything so pretentious as a methodology or a theory.

Second, the area group, despite truly remarkable accomplishments, is not at all complacent about its own scholarly performance. Everywhere in area circles one encounters fervent injunctions about going beyond traditional area studies, of escaping from the "area box," as it is frequently put. Considering that a brief twenty-five years ago the universities of the United States possessed the merest handful of scholars fluent in any non-Western languages or competent in such subject matters as the politics, history, economics, literature, or philosophy of any non-Western society and that today its endowment in these respects is not even remotely rivaled anywhere else in the world, such self-abnegation on the part of those responsible for this achievement is, to say the least, remarkable.

These two characteristics—a lack of unity and appreciable intellectual dissatisfaction with their own performance—combine to produce a third relevant phenomenon: the area group is far more aggressed against than aggressing. The charges come largely from the behavioral side and are usually justified as necessary to the effort to convert political science into a more scientific discipline. Occasionally an area proponent replies, but, to use the military metaphor, the area tactic has been one of skirmishing rather than pitched battles or frontal attack.

The locus of this anti-area animus in what I have been calling in grossly oversimplified terms "the behavioral camp" is also of interest. In my experience it is not shared by many of the founders or leaders of what has been termed the Behavioral Revolution. Any such bias is notably absent—indeed, often specifically disavowed—in the works of such scholars as Almond, Dahl, Deutsch, Eulau, or Truman. In fact it is difficult to trace the evolution of the animus in specific terms. Like Topsy—it just growed, and eventually became a part of the behavioral view, widely shared in many, although not in all, quarters.

If, for the sake of argument, one grants that all this is true, that I am providing an essentially accurate description of an episode of disagreement and strife within the discipline, the question remains: "Does it really matter? Isn't this just one more tempest in the academic teapot? Why should we care?"

I think that it does matter, and I would like to devote the balance of my remarks to an attempt to explain why I believe this to be so. I have three categories of argumentation in mind:

(1) The grounds for dissension are mostly spurious;
(2) The episode provides important and, I hope, constructive insights into the processes of intellectual change and commitment within the discipline; and
(3) It points the way to catharsis and synthesis. The dialectic need not continue constipated and lacking in synthesis.

Allow me to develop these points in sequence.

Is There an Area Approach?

My first point was that the grounds for dissension are largely spurious. I would argue that there really is no such thing as an "area approach."

There are undoubtedly individual political scientists of faculty rank who in their teaching or research specialize to varying degrees in the politics of particular areas such as Japan, Nigeria, India, or Brazil. The forthcoming Lambert survey of area programs estimates that there are probably several thousand of them. As one of their number, I have always found it a bit difficult to understand why this was in any way more remarkable—let alone more questionable—than specialization in the political systems of Great Britain, France, Germany, or the Commonwealth. These latter have been standard parts of the discipline since the nineteenth century. Logic aside, however, it is true that the large-scale extension of systematic professional interest to non-Western political systems that developed after World War II did entail both stress and change within the discipline. Those involved in this intellectual diaspora were faced with at least three critical problems: (1) an unusually formidable language barrier—few American scholars spoke or read such languages as Chinese, Arabic, or Hindi; (2) the manifest inadequacy of the traditional categories of comparative political analysis which were largely legal and institutional in nature and had been developed with only the Western European and American experiences in mind; and (3) the problem of devising alternative categories of analysis sufficiently broad and precise to incorporate both the traditional Western political systems and a variety of quite different non-Western polities.

The shared characteristics of what some have called "the area approach" derive from this particular set of problems which are common to all scholars working in non-Western settings. These characteristics are basically commonsensical. First, such so-called area specialists place great emphasis on the importance of learning at least the major language of the society concerned. Communication is prerequisite to effective study or learning. Our most common languages—English, French, German or Spanish—are usually not adequate for this purpose. Therefore, one must learn to speak and read the languages that are. It is that simple.

Second, area specialists are apt to be unusually sensitive to the impact of culture on politics. The postwar study of comparative politics in non-Western settings began with a recognition of the sterility of trying to analyze these polities in terms of their constitutions, laws, and executive, legislative, and judicial systems. Political institutions that bore familiar Western names simply did not perform functions analogous to those of their Euro-American namesakes. The obvious question was: "Why?"—the equally obvious answer: "Cultural difference."

From this rather elementary perception stemmed a much more detailed and sophisticated search for the cultural determinants of particular non-Western political institutions, events, attitudes, and behaviors. The questions asked were not basically different from those involved in a similar analysis of the American or British political systems. They differed primarily with respect to the investigator's point of departure—and, I might add, that of his audience. One started more or less from scratch. Little or no prior research that was directly relevant existed; the basic data in currently useful forms had literally to be created. Lacking were the enormous advantages of operating in the culturally familiar settings of developed Western societies with scholarly traditions that are both compatible and, in important degree, cumulative. The result was that political scientists working under these circumstances shortly became very sensitive—perhaps excessively so—to the total cultural matrix in which politics is set; and they felt obliged to write and teach about the societal setting and determinants of politics to a degree that is uncommon in professional writings about the American political system where both author and readers usually share so much more of the contextual knowledge concerned, or at least think that they do. It is only in this limited sense that an area specialization may be said to be interdisciplinary. While sensitized to the relevant theories and findings of other disciplines, it is still basically a mono-disciplinary approach.

A final characteristic that area specialists tend to share is a high regard for the importance and relevance of history as a determinant of political outcomes. This is really a subcategory and a consequence of their concern with the cultural context of politics. One does not study culture in an historical vacuum. The same should be true of politics. It is a conceit born of the Enlightenment and reinforced by certain trends in current social science theorizing that political attitudes, behavior, or institutions can be explained or understood in terms that do not involve a substantial historical element. Professional exposure to societies where the so-called premodern stages of history are chronologically so much closer to the present, where nation states have emerged well within the reach of living memories, and where people still live and strive to cope with problems that seem by our standards archaic imbues one with a particularly keen sense of the contemporary relevance of history.

This set of characteristics—a concern with language, with culture, and with history—is really all the "theoretical" or "methodological" baggage that area specialists possess in common. I personally do not find it a very impressive endowment. As I said earlier, it is basically commonsensical, wholly predictable, and, I should say, inevitable given the nature of their professional subject-matter. It is certainly not of an order of complexity, precision, interrelatedness, or pretentiousness that would justify labeling it as a methodology or even a technique. Where then is "the area approach?"

The matter does not stop at this point. Richard Lambert of the University of Pennsylvania has recently completed—but not yet published—a massive and most impressive review of language and area programs in the United States. In the course of his activities he tried not only to identify area specialists but also to determine what they actually do. The results are interesting. For example, among the more than four thousand college and university teachers with area specializations in Lambert's sample, 73.6 per cent taught at least one course that had no area-specific required reading material.

The same group were asked to rate themselves on a seven-point scale as to the degree to which their teaching, research, and other professional activities were concerned with their area rather than their disciplinary interests, a rating of seven indicating complete area orientation. The mean scores were 4.1 for teaching activities, 5.3 for research, and 4.8 for other professional activities. Only a minority (14.6 per cent for teaching, 33.2 per cent for research, and 24.3 per cent for other professional activities) regarded themselves as completely area rather than disciplinary in their orientations. It should also be noted that these figures represent results for all area specialists regardless of discipline and are seriously skewed toward the area pole by large numbers of historians and humanists whose disciplines are traditionally organized along area lines. They would be far lower for political scientists with area specializations.

In the light of these findings, I would submit that not only is there no shared body of theory or methodology that can legitimately be labeled an "area approach," but also there are relatively few political scientists who see themselves as more area than disciplinary in their professional activities and orientations. It is in this sense that I view the grounds for dissension between the behavioral and area camps as well as the concomitant charges against area specialists within the discipline as largely spurious. To revert to the symbolism implicit in the name of Professor Shannon's antihero, they remind one of Don Quixote's famous tilt with the windmills—sincere, energetic, and zestful to be sure, but sadly ill-formed.

Why the Tensions between Area Specialists and Behavioralists?

But the windmills were there! The valiant but myopic Don was right in at least this respect. And in a sense the windmills of the mind at which some of our colleagues have been tilting are also there. Which brings me to my second point. This episode that pits some behaviorally inclined members of the profession against some area-inclined members provides potentially important insights into the processes of intellectual change and commitment within the discipline.

What has actually been going on? In my estimation we have here a case of parallel development leading to intradisciplinary hostilities. Permit me to explain. In doing so I will ignore the pre-1945 antecedents of both area studies and behavioralism. They are significant in both cases but isolated and small in scale and disciplinary prominence.

In their early modern stages, area studies were a newborn phenomenon in the sense that our vastly expanded national relations and concern with Japan, China, and the U.S.S.R. in particular—although not necessarily friendly—produced a widespread awareness of our abysmal ignorance of these major societies and their cultures. The postwar world was really new in this sense. It expanded enormously our national spheres of interest, concern, involvement, and responsibility. A sort of communications revolution ensued and brought with it a demand for specialized higher education in the languages, histories, cultures, and politics of the non-Western peoples involved. In any event, the interest and response generated in academic circles was genuinely academic and professional in nature. The early area programs were financed by the universities themselves with substantial assistance from the Rockefeller Foundation and the Carnegie Corporation of New York. Government funds were not involved.

The oldest organized area programs date from about 1947 and relate primarily to Japan, the U.S.S.R., and China. For the most part, programs specializing in other areas—South or Southeast Asia, the Near and Middle East, Latin America, Africa, or Eastern Europe are of considerably later vintage, most having been established since 1959. The ages of area programs range, therefore, from about twenty-six years down to ten or eleven years. The average is probably around fourteen.

Their organizational form varies widely from the loosest type of committee structure to full-fledged centers and institutes and occasional departments. So does their leadership. Most directors are historians or humanists. Practically all area programs are interdisciplinary in the sense that their staff includes representatives of two or more—occasionally as many as ten or twelve—different academic disciplines ranging widely over the humanities and social sciences. Relatively few of them, however, have separate staffs. The individuals concerned are mostly on loan from departments and thus professionally dependent primarily on their disciplinary rather than their area connection for such basic matters as appointment, pay, and promotion. This is important.

Important also are certain aspects of the developmental history of area programs. There was a certain mystique about them in the early days and some rather ill-considered and exaggerated claims were made about areas as a basic mode or unit of scholarly organization alternative to the standard disciplines. In practice, such claims were seldom taken very seriously, however, and far more significant was the sense of excitement aroused by being involved in an activity that was simultaneously pioneering in character, calculated to redress the older ethnocentric biases of the profession, and doing so along lines that promised substantial achievement in both humane and disciplinary terms.

Such circumstances inevitably fostered group loyalties and a mild sort of academic nationalism. It is noteworthy, however, that these loyalties were parochial and focus not on the area approach or movement as such but on the particular country or region with which a given scholar was involved, to wit, Japan, China, India, etc. What bound these groups together was not shared theory or methodology but a common interest and stake in studying and interpreting the language, literature, history, economy, or politics of the area concerned. Note also that in the early days at least this specific area focus tended to rule out any more general comparative interests. The tendency was to study and explicate the political systems of these non-Western areas for their own sake as major and neglected institutions about which very little was known. For the most part these early investigations were not guided by theory partially because there were then—as now—no very persuasive universal models of political systems and, partially, because from a scholarly standpoint the most impressive and demanding aspect of the societies concerned was our almost total ignorance of their workings. Practically speaking, it was felt that some measure of basic information gathering had to precede the construction of grand theory.

This did not, however, connote a total absence of theoretical or methodological concerns. The political scientists involved tended to share a profound dissatisfaction with the traditional forms of political analysis developed in Western settings. Their search for alternative modes of analysis had a great deal to do with the emergence of the structural-functional approach and the new emphasis on process rather than form in the study of foreign political systems, as well as with the renaming and restructuring of the field that we now call comparative politics rather than comparative government. They were also among the first to use systematic interview techniques in societies other than our own and thus to add a new dimension of life and realism to what had been a decidedly documentary if not dreary tradition.

What I would like particularly to emphasize, however, is not so much the state of the area art in its earliest stages—exciting, innovative, parochial, mildly xenophobic, descriptive, and but marginally concerned with theory and methodology—as the subsequent processes of change which are common to its later stages and are in a number of cases still emergent. I will do so with specific reference to political scientists who have area specializations and largely ignore the other disciplines and specialists involved in the area endeavor.

The important thing to note is that nothing stays the same; everything changes. If one's prime concern is with the scholarly quality of this area-oriented product in terms of teaching and research—as I believe it should be—a number of factors besides the competence of the individual scholar must be taken into account. Among them are the age and stage of scholarly development of the particular area group concerned, the state of indigenous scholarship in that area and thus of the native resources upon which the area specialist may draw, the ease or difficulty of access to the area for purposes of study or research, and the availability of funds for research and training purposes. All of these factors are subject to change and, as they change, so does the area field concerned.

Several aspects of this process of change are of particular importance. First, the professional loyalties of the political scientist who is also an area specialist have always been bifurcated. He may participate actively in an area program, but usually his career interests lie basically with the discipline. He is, therefore, impelled to keep abreast of changes in the discipline and is apt to be affected by them in very much the same fashion as are his nonarea-oriented colleagues. Second, the mild xenophobia that characterizes the early stages of area organization does not normally persist. With the passage of time, the initial mystique of both the language and the culture diminishes, particularly for the social science members of an area program. They tend to become less absorbed with the particularities of the area and progressively more concerned with how their area knowledge fits into such more general frameworks as political development or comparative politics. Finally, and most important of all, the process of generational change has time to operate. New cohorts of political scientists trained along newer disciplinary lines infiltrate the ranks of the older first-generation area specialists and bring with them all of the theoretical and methodological apparatus of their day in graduate school.

The result of this joint process of attrition and development is an area field that differs greatly from what it was five, ten, or twenty years ago. Its practitioners are trained primarily as political scientists. Their professional interests and skills reflect quite faithfully those of the discipline at large. They differ mostly in the sense that they have spent an additional year or two in graduate school or abroad acquiring language skills and general area competence.

This then is the first of the two processes of development that I set out to explain. It is also the thesis in the dialectic that I am positing. The parallel development of the behavioral movement constitutes the antithesis.

This audience does not need still another account of the rise within the profession of that composite of theories, methods, and techniques that is loosely subsumed under the title of behavioralism. You have lived with the experience and are familiar with the history involved. Permit me, however, to dwell briefly on those aspects of the movement that are in my opinion responsible for engendering its antipathy to area studies and area specialists.

First, the life spans of the two movements—area studies and behavioralism—largely coincide, with area studies being somewhat the older of the two, having started in organized fashion about 1947 rather than in the early 1950s. This overlap facilitated several kinds of rivalry, both real and fancied. To begin with there was obviously a potential for conflict between a creed that proclaimed "The root is man," to borrow Eulau's famous phrase and one that at least seemed to be saying "The root is area cum culture." That in fact few area specialists in social science fields really upheld such a tenet did not much matter. They could with some superficial validity be portrayed as doing so.

At a more mundane level some behavioralists also felt that they were in competition with area specialists for scarce foundation funds. In the 1950s and for much of the 1960s behavioralism was a young and struggling movement. Some of its most characteristic techniques, in particular survey research, were uniquely expensive, and the dollars were hard to come by. This was precisely the period when the major foundations were providing relatively handsome support for a selection of area programs, and given the adverse opinion of area studies held by many behavioralists, it was natural that they should both envy and resent what they perceived as a regrettable misallocation of scarce funds.

There was also some rivalry at the campus level. At the major universities in particular, area specialists were usually organized in centers or programs which were externally funded in substantial part and which enjoyed a peripheral status in the contentious hierarchy of their colleges of liberal arts. The behavioralists, on the other hand, were apt to be based either in the Department of Political Science or in research institutes. In either event they tended to view the area centers and personnel as rivals for both support and status within the university community. This, too, was not conducive to amiable relationships.

In the 1950s and early '60s, in particular, there were also strong contrasts between the professional working styles, methods, and products of behavioralists and area specialists—contrasts that could only accentuate the other causes for dissension. The behavioralists in those days worked almost exclusively in the field of domestic American politics. By so doing they inherited the fruits of the richest storehouse of social science data and findings in the world. To it they brought new approaches, greater rigor, important discoveries, and, ultimately, an impressive increment of new knowledge. Their work was characterized by a predominant concern with methodology, theory building, and canons of proof, by the emphasis placed on mathematical and statistical techniques, and, ultimately, by their attempts to achieve for the discipline a more truly scientific status.

The area specialists on the other hand were at this time contemplating the legacy of social science data and findings that awaited them in such places as China, India, Bolivia, or Kenya. It varied enormously in quantity, quality, and accessibility. In some cases—China and Russia for example—the borders were physically closed to meaningful social science research, and it was necessary to observe from a distance and to develop the black arts of Kremlinology and Pekinology. In others the borders were open but the social science cupboard was bare. Practically no one had ever seriously studied the political systems concerned in terms that had contemporary utility. In still others one encountered rich and ancient traditions of indigenous scholarship that had in varying degree been modernized. But the prime emphasis of such scholarly communities was apt to be historical, literary, or philosophical; and, where they addressed themselves to contemporary politics, there was a high probability that they did so in descriptive, legal, and institutional terms. In few of these cases was there much cause for rejoicing among American-trained political scientists and area specialists confronting these scenes with the sorts of questions engendered by their own recent revolt against legal and institutional analysis at home.

The professional consequences of circumstances such as these are not well understood by those who have not confronted them in person. Practically speaking, one never really starts quite from scratch. There is always some sort of pre-existing literature or lore to be discovered and assayed for its ore content. Thereafter, if one has any sound sense of the impact of cultural difference on the shape, content, and interrelations of political institutions, attitudes, and behavior, it is not wise to plunge right into the sort of rigorously and compatibly defined microstudies of political attitudes and behavior that are the norm at home. You may not be measuring the same thing at all when dealing with concepts so shifting and elusive as party identification, leadership styles, or sense of personal political competence or efficacy. Some prior sense of overall cultural form and style and of the interrelations of particular political institutions, attitudes, and behaviors therewith is a functional prerequisite to more refined ventures. So are the collecting and evaluating of basic political data that are simply taken for granted in this country.

This type of scholarly inquiry has, in my opinion, very appreciable value in its own right. Ideally, it need not preclude a growing attention to more scientific modes of inquiry; but, in fact, in the early years of an area venture it often tends to do so. For the most part this has been the predominant style in what I characterize as the initial stage of the development of a given area field. This has been particularly true of the older area fields such as the Japanese or Russian. It is probably less so in the case of the newer fields such as Africa where the relative youth and the recency of academic training of the individuals involved act as a countervailing variable. In any event it is easy to see how in the 1950s and early '60s the American-oriented, scientifically fixated behavioralist element in the discipline came to look askance at their foreign-oriented, culturally fixated area colleagues on grounds of incompatibility of working styles, methods, and scholarly products. The practice of the time largely substantiated such a judgment, and few on either side really understood or took the trouble to investigate what the other group actually did and why they did so.

More fundamental, and also more lopsided, than this, however, is what I can only define as the ideological element in the behavioral versus area confrontation. I have earlier made the point that the level of ideological—or, if you will, methodological and theoretical—commitment to an "area approach" was never very high in area circles and that it has diminished in later years, especially for the social science contingent involved. The typical attitude is one of concern about the so-called "area box" and whether or not the work of area specialists is contributing appropriately to something often described as the mainstream of innovation in the discipline. Very little ideological ardor or arrogance is involved.

The situation is different with many of the behavioralist group—with numerous notable exceptions, particularly among the intellectual leaders of the movement. I find the phenomenon fascinating, especially against the background so admirably depicted in Dr. Shannon's "Obituary of a Political Scientist." Like Caspar Milquetoast, the discipline has to an extent seldom acknowledged lurched from one ruling obsession to another. In the space of thirty-three years (1917–1950)—slightly more than the classic definition of a single generation—Caspar's career embodies at least five such commitments, impressive in zeal if not in duration: to evolutionary democracy, to efficiency in government via city managers and business methods, to economic determinism and socialist politics, to the merit system, and to psychological determinism. But whereas Caspar's capacities for enthusiastic dedication dwindled with time and progressive disillusionment, those of the profession show little sign of doing likewise. They are more reminiscent of the valiant Don Quixote, untouched by repeated rebuffs and ever ready to pick himself up from the dust, mount his staunch Rosinante, and ride off to new and more glorious quests and combats.

It is not that the discipline lacks a solid underpinning of impressive accomplishments that have added enormously and cumulatively to its sophistication and understanding of political phenomena since the early years of the century. It is rather the undiminished ardor with which, against such a background, it embraces new causes and panaceas. What a truly stupendous "forgettery" must be involved! What a magnificent, if short-sighted, capacity for emotional as well as for intellectual commitment! How remote from an attitude of cultivated and cautious skepticism toward all professional nostrums! Perhaps this is a subconscious reason for our recent aversion to historical inquiry?

It must be added that if Caspar Milquetoast's successive intellectual idols have been able to mobilize even briefly an impressive following and degree of commitment within the profession, their potency pales before the totems of behavioralism. No magic—unless that of religion or of nationalism—has proven as strong or as energizing as that of science. A social scientist who is really convinced of the scientific quality of his methodology and findings is reminiscent in several respects of the legendary Presbyterian rising from his knees to do the will of God. Not infrequently such apocalyptic zeal carries over into professional relations with colleagues of less faith or different persuasion. This would seem sometimes to have been the case with attitudes toward area specialists, inevitably rendering more difficult their relations with behavioralists.

It is sad to think that the process I have described may be all that lends substance to this particular controversy within the discipline. If the diagnosis is accurate, the estrangement is compounded largely of ignorance, misunderstanding, and misplaced zeal. Insofar as it has substance, this is found in circumstances which prevailed in the earlier stages of development of both area specialization and behavioralism and then probably only in marginal degree. Nevertheless, the animosity has been real and has had unfortunate consequences in terms of the training, staffing, and funding of political scientists. Most important of all—fruitful professional relations between behavioralists and area specialists have been impeded and rendered unnecessarily difficult.

The Path to Synthesis

The third and final point that I want to address relates to the possibility of synthesis and catharsis. Must the relation between behavioralists and area specialists continue to be estranged and barren? I think not—and I would like to offer a few observations and speculations on this score.

Fundamentally, two emerging and—it is hoped—reinforcing trends are involved in this possibility of synthesis. On the one hand, the area specialists in political science have for the reasons stated earlier long been subject to professional influences that have moved them steadily away from idiosyncratic area orientations in the direction of far greater interest in the sorts of methodological, theoretical, and comparative concerns that are central in the discipline. Even a cursory examination of the research designs of work in progress in most area fields will demonstrate the extent to which this is true in practice. The trend is, of course, not universal; but it is certainly impressive and growing.

On the other hand, as you are all aware, the interests of many behavioralists have been shifting to the international and comparative scenes in recent years. This is a development of major significance. In its origins and for most of its brief history, behavioralism in the discipline of political science was almost wholly an American movement. Although gradually it has acquired foreign converts, including some very distinguished ones, it began in this country, its leaders have almost always been Americans, and its data base and findings were long geared almost exclusively to the American scene. The results were, of course, impressive; but the degree to which they may in fact be culture-bound has never been adequately explored.

The problems involved are numerous and serious. For example, no behavioral field is better developed, more sophisticated, or more mature than that of electoral attitudes and behavior. And, I would assume, since the American Voter series, no concept has been more seminal or central to the conduct of scholarly research in this field than that of "party identification." But unless I misread him completely, Donald Stokes, a principal author of the American Voter series, was suggesting in his

1972 paper before this Annual Meeting that the role of party identification or partisan self-images in electoral change is significantly different in Great Britain than in the United States.[2] I am practically certain on the basis of my own experience in Japan that the concept lacks any very close cognitive equivalent in that quite different cultural milieu. One does not have to go far to uncover comparable problems of equivalence in such popular fields as elite studies or political socialization. Apparently, cultural variations can make a critical difference with respect to the validity or, perhaps, even the relevance of some of our more cherished conceptual units of analysis and comparison. If so, this cuts very close to the heart of the entire comparative endeavor insofar as it is couched in cross-cultural terms.

Lest anyone think that the problem is limited to this particular level, let me recall to your memories Frederick Frey's splendid essay on "Cross-Cultural Survey Research in Political Science."[3] There he sets forth in impressive and appalling detail the sorts of problems that await the scholar who seeks to optimize the accuracy and reliability of data obtained through a given survey instrument administered in several different cultures. If not sufficiently dismayed by Frey's account, you might go on to peruse in the same volume Dell Hymes' equally prescient and pioneering but even more basic essay entitled "Linguistic Aspects of Comparative Political Research."[4]

I point out these difficulties not to discourage further cross-cultural and comparative research by political scientists but to suggest that the profession has yet to face effectively the attendant problems of training and research strategy. If it is true that cultural difference is this critical a variable in the entire comparative enterprise and if the problems can be so severe between cultures as similar as those of Great Britain and the United States, we are in very serious trouble. If our research abroad, particularly in non-Western settings, is to acquire a respectable behavioral dimension, distressingly few of our behaviorally inclined colleagues have been adequately trained for this purpose. For the most part, they really don't know enough about the languages and cultures involved to be able to cope effectively with the scholarly problems posed by cultural difference. Methodology alone is not enough.

I see two long-term solutions to this problem. The first lies in a synthesis of behavioral and area training. Fortunately, this is already under way; but the initiative comes more from the area than from the behavioral side. Increasingly, at a number of our major universities a Ph.D. in political science with area specialization differs in only one significant respect from a straight disciplinary doctorate. The individual concerned meets the same requirements, receives the same training in statistics, methodology, and theory, and is distinguishable from his other colleagues only by reason of the fact that he has invested an additional eighteen months to two years in language and area work in order to secure his degree. As the entry requirements become more difficult and the better universities cut down still further on the size of their entering graduate classes, this tendency becomes even more pronounced. From the profession's standpoint this is a most satisfactory solution. It results in the implantation of what a colleague of mine used to refer to as "two skills in one skull"—an ideal synthesis.

The only real problem with this solution is cultural lag within the discipline. Both graduate students and faculty with area specializations are still viewed with suspicion in many behavioral quarters. They are not infrequently regarded as a different breed, inherently inferior in quality, and of dubious loyalty to the cause—not true believers. The result is a subtle but effective form of discrimination which tends to steer individuals who are not really dedicated to an area interest along other and more orthodox career paths.

I submit that this is not in the best long-term interests of either the profession as a whole or, for that matter, the future development and enrichment of the behavioral approach. We have spoken for years of the comparative mode as the only truly scientific approach to the study of politics. If we really believe this, we must equip ourselves and train our successors to understand and deal far more effectively with the problem that is fundamental to all cross-national and much domestic research—cultural difference. One does not do this by exalting methodology at the expense of language and area training. Both are essential to the future of comparative politics.

I indicated a moment ago that I could envisage two long-term solutions to this problem. Actually they are complementary, not mutually exclusive. One is the "two skills in one skull" technique just described. The second has a better-known rubric—"the internationalization of social science."

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[2] "Some Comparative Reflections on Electoral Cleavages in Britain," unpublished paper, pp. 2–10.

[3] In *The Methodology of Comparative Research*, ed.: Robert T. Holt and John E. Turner, New York, The Free Press, 1970, pp. 173–294.

[4] *Ibid.*, pp. 295–341.

It sometimes seems to me that there are curious and little-remarked parallels between the postwar course of world politics on the one hand and the global social science scene on the other. I would not press this analogy too far, but let me mention a few of the similarities I see.

In both spheres the period from 1945 to about the mid-1960s is at least mildly reminiscent of the attitudes, values, and performance characteristics implicit in the phrase, "The American Century." If American foreign policy may be said to have been characterized during this period by global pre-eminence; by either monopoly or supremacy in nuclear weaponry; by worldwide involvements and engagements; by a more or less general acceptance of the superior wisdom, virtue, and efficacy of its own motivations, policies, and acts; by what on balance can only be termed its "good intentions"; and by enough explicit or tacit acceptance of these views in some foreign circles to facilitate their acceptance at home, do not certain analogies suggest themselves with respect to the postwar record of American social science in general and political science in particular?

It was not only the cities and the political and economic institutions of the ancient civilizations of Europe and Asia that were decimated by the war. Their scholarly institutions suffered equally, thus creating what—to pursue the initial metaphor—we may call a "vacuum" into which American intellectual and academic aid, prestige, ideas, training, and influence flowed or pushed. The domestic immunity of the United States to the physical consequences of the war plus its unprecedented affluence and the fervor and energy of some of its social scientists resulted in a species of worldwide scholarly penetration that was certainly unprecedented in so short a space of historical time. This was particularly the case in the social sciences.

The reception of this enhanced American presence abroad varied, of course, but the aid was accepted and in time enough converts were made to the views and methods of American social science so that new currents of scholarship were set in motion throughout the world. The entire process was undoubtedly much helped by the fact that during most of this period the United States was in the throes of the so-called Behavioral Revolution and that this seemed to many at home and not a few abroad the newest, most vital, exciting, and promising development in the field of the social sciences to be found anywhere. The result has certainly not been a mass capitulation of foreign systems of higher education in the social sciences to the American persuasion, but it has resulted in a vastly expanded American presence, and a degree of activity, ferment, and influence that has never before characterized the external reach of American social science.

The result is that in postwar times American social scientists have been concerned, involved, and active abroad in both the West and the non-West to a completely unprecedented extent. Whatever the underlying assumptions and short-term consequences of this American social science diaspora, a great deal of contact of academic subcultures across national boundaries ensued; and one began to hear talk of something called rather vaguely "the internationalization of social science."

This is not the occasion to discuss the manifold problems and pitfalls of such an endeavor. I merely want to point out the physical and intellectual improbability of long-term progress in the study and understanding of comparative politics so long as the endeavor is based largely on American money, personnel, methods, and theories. The cultural and intellectual arrogance and the degree of ethnocentrism that underlie any such assumption should be obvious. However, such can be our passion for the cause of social science and our unquestioning acceptance of the rectitude and value of our professional endeavors that the question may never be raised in these terms. It should be routine! There is no day-in and day-out equivalent for the cultural sensitivity and understanding of a talented scholar native to the society concerned who has also been trained in a social science tradition that is in working terms at least compatible, if not identical, with our own. There is also a real function and need for the perspective and objectivity of the foreign observer, particularly if he adds to his linguistic and area skills the perceptiveness of a de Toqueville or a Bryce; but such individuals cannot really replace native scholarship. Among other things, there is simply too much to be done and far too few qualified foreign scholars to do it.

It is curious how seldom we discuss this question of the role of non-Americans in the advancement of comparative social science. If we did, it would never occur to most of us to think that this was really an American monopoly. Yet we often act as if this were the case, especially where the other cultures concerned are non-Western. In any long-run sense it should be obvious that the bulk of the data gathering and of the scholarship with respect to any society is going to be done by its own scholars, if it is done at all. There is a role for Americans, as there is for all other foreign scholars of comparative bent and appropriate training; but this is likely to be supplementary and, eventually, derivative in the sense that it builds upon and criticizes the products of local scholarship.

In the interim pending the advent of a time when particular societies have produced conditions in which such fruitful interaction across national lines by social scientists is possible, there are problems of a scope, complexity, and virulence that sometimes surpass belief. I do not underestimate these. There will undoubtedly be more instances of governments that choose to regard the raw data of social science as national possessions akin to oil deposits or classified military intelligence. In such aggravated cases the only remedy is time; the only effective prescription—continued effort plus patience. This type of denial of access, while worrisome and inconvenient, is not fatal and is often partial or temporary. The Chinese and Russian cases demonstrate what can be accomplished even in the face of extreme difficulties.

Granting the problems involved, our long-term interest clearly lies in supporting more general freedom of access to the data of social science by all responsible and objective scholars, providing training and other forms of professional assistance to foreign communities of political scientists where this is both feasible and desired, and seeking to extend opportunities for collaborative research with foreign political scientists both at home and abroad where the nature of the project involved renders this both efficient and practicable. A program of this sort lends substance to that otherwise undefined phrase "the internationalization of social science." It is also calculated to add an invaluable second dimension to the profession's capacity to carry out effective cross-cultural research in comparative politics—the first dimension being the aforementioned combination of behavioral and area training that I have referred to as "two skills in one skull."

This is the type of synthesis that, in my opinion, the discipline of comparative politics really needs. Given the developments already in process in both the behavioral and area camps, it seems to me that the time is ripe for its realization. By this I mean that in an increasing number of individual cases behavioral training and skills have already been combined with their area counterparts. All that remains to be done is to acknowledge openly the utility, indeed the essentiality, of both the area and behavioral traditions to the further progress of the discipline and to expand the numbers receiving such dual training. At that point synthesis will at last have been achieved and the dialectic consummated. I venture to predict that so glorious a dénouement would rejoice the shade of the late Professor Caspar Don Quixote Milquetoast.

COMMUNICATIONS

TO THE EDITOR:

In his brief article, "Regression Analysis and Discriminant Analysis: an Application of R. A. Fisher's Theorem to Data in Political Science," (*APSR*, 67 [June, 1973], 555–559), Fred Kort demonstrates clearly that multiple regression can be successfully used to perform discriminant analysis between dichotomous groups. He thus reinforces the growing consensus that regression can be used as a general data analytic system for models that are linear in the coefficients, if the normal assumptions about the behavior of the error term can be made. Kort shows that one can derive the coefficients of the general discriminant function:

$$Z_i = c_1X_{i1} + c_2X_{i2} + \cdots + c_kX_{ik}, \quad (1)$$

from the coefficients of the analogous regression equation:

$$Y_i = b_0 + b_1X_{i1} + b_2X_{i2} + \cdots + b_kX_{ik} + e_i, \quad (2)$$

and that the relationship between these two sets of coefficients is:

$$c_j = b_j \frac{N}{N_1N_2}, \quad (3)$$

where N_1 and N_2 are the sample sizes of the two groups to be discriminated. Kort goes on to argue that one should use this relationship to transform the regression coefficients into the discriminant function coefficients, and then proceed with the discriminant analysis.

There are at least two puzzling aspects to this presentation. First, he argues that regression analysis is inappropriate when the dependent variable is dichotomous, and that therefore one must turn to discriminant analysis. Yet the transformation in (3) above yields nothing more than would the results of a regression where all the data vectors for the independent variables were multiplied by the constant (N_1N_2/N). Since the coefficients of one method are simply a linear transformation of those of the other, no new information is gained, and furthermore the discriminant analysis approach would presumably have to fall victim to all the pathologies which Kort argues make regression inappropriate.[1] The fact that it does not implies that regression is robust against these pathologies. Second, the conclusion one must draw from this logic is that Kort's recommended transformation is unnecessary. Specifically, one can equally well use the predicted values of the dependent variable from the regression equation to discriminate between the two groups, and no computation of the Z's of the discriminant function need be done. The cutoff point to be used in discriminating between the two groups is the mean of the group means of the predicted values of the dependent variable, exactly equivalent to the procedure described by Kort. In his example, he finds the cutoff point for Z to be 0.0506; using the predicted values for Y one gets a cutoff value of 0.555. This value not surprisingly conforms to the identity:

$$Y^* = Z^* \frac{N_1N_2}{N} + b_0, \quad (4)$$

where Y^* and Z^* are the appropriate cutoff points, and b_0 is the regression intercept from (2) above. Thus the original regression equation can be used equally effectively to discriminate between the two groups.

Kort also demonstrates that the R^2 and F statistics from the regression can be used to measure the significance of the discriminant analysis. This fact should be intuitively obvious, since the difference between the two methods is nothing more than a change of scale. Indeed, if such transformations did affect the results of an analysis, consider the plight of a researcher who would then find one set of significant results when his data were measured in dollars, and a different set when the same data were measured in cents! The use of R^2 as a measure of the efficacy of the discriminant analysis, however, is not particularly intuitive. In the analysis Kort reports, for example, $R^2 = 0.558$, yet 27 out of his 32 cases were correctly classified, or a proportion of 0.844. This proportion, a simpler but more intuitively obvious statistic, does provide a useful criterion for comparing different analyses. However, the standard F-test of the increase in explained variance should also be employed, as it provides a criterion of statistical significance. In addition, the analysis above clearly demonstrates that the usual t-test of the differences of the coefficient estimates from zero can also be employed to test the efficacy of each of the coefficients in discriminating between the groups.

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[1] George W. Ladd in his article "Linear Probability Functions and Discriminant Functions" (*Econometrica*, 34 [1966], 873–885), argues that the choice of method depends on the assumed distributional characteristics of the dependent variable, yet again the eventual outcome of the analysis is clearly the same regardless of the technique used.

Let me conclude by reiterating that I do not find fault with Professor Kort's essential theorem, nor with his discussion of its usefulness in political analysis. However, I believe that he recommends a methodological procedure which is needlessly time-consuming, which provides no additional information beyond that available from ordinary multiple regression, and which is no more effective in fulfilling the goal of classifying units of observation into dichotomous groups.

PETER H. LEMIEUX
Massachusetts Institute of Technology

TO THE EDITOR:

The comments by Mr. Lemieux on my article in the June 1973 issue of the *APSR* provide a welcome additional emphasis on the relationship between multiple regression analysis and two-group discriminant analysis, which—in view of the continuing and increasing importance of multivariate analysis in political science—is especially useful. All his statements pertaining to the mathematical exposition of this relationship and to the applicable statistical tests are correct. His conclusion, however, that the conversion of regression coefficients to discriminant function coefficients and the corresponding computation of the discriminant function values are unnecessary requires further comment.

Although regression analysis—if performed under the conditions stated in my article—and two-group discriminant analysis offer the same results, the respective underlying assumptions of these methods are quite different.[1] Regression analysis is based on the assumption that there is an observed dependent variable which can be regarded as continuous. The object of the analysis is to minimize the sum of squares of differences between the observed dependent variable Y and estimated Y^*, to be obtained from the analysis. Accordingly, the function

$$f = \sum_{i=1}^{N} (Y - Y^*)^2 \qquad (1)$$

is formed, where N is the number of observations and

$$Y^* = a + b_1 X_{i1} + b_2 X_{i2} + \cdots + b_j X_{ij} + \cdots + b_n X_{in},$$

consisting of the linear combination of the independent variables X_j $(j = 1, 2, \ldots, n)$, their coefficients b_j, and the constant a. The values of a and the b's are obtained as results of the analysis.

By taking the partial derivatives of (1) with respect to a and each b and setting them equal to 0, one obtains the system of normal equations, which—in matrix and vector form—can be written as

$$(X'X)b = X'Y. \qquad (2)$$

This system of equations is solved for the unknown a and b's, the regression coefficients. It readily can be seen that the observed dependent variable Y is incorporated in the equations from the solution of which the values of the regression coefficients are obtained. The obtained values of the regression coefficients clearly depend, therefore, on the values of observed Y. The initial assumption regarding the existence of such a variable and its particular values affect the entire analysis. To be specific, if the values of Y are 0 or 1, the values of the coefficients resulting from the regression analysis reflect these values of Y. It is in view of this consideration that the limitations of multiple regression analysis were stated in my article (last paragraph on p. 555 and first paragraph on p. 556). Regression analysis becomes "robust against these pathologies" (the expression used by Lemieux) as a result of its conversion to discriminant analysis, but not on the basis of its underlying assumptions.

In contrast to regression analysis, discriminant analysis does not assume the existence of an observed continuous dependent variable. In the two-group case, it assumes only that various observations of a phenomenon can be placed in one of two categories. To the extent that one can refer to an observed dichotomous dependent variable in this case, one should note that no assumption is made that this variable can be measured either on an interval scale or on an ordinal scale. To be specific, there is no assumption, for example, that such a variable has values of 0 or 1. The object of the analysis is to find values for a function

$$Z_i = c_1 X_{i1} + c_2 X_{i2} + \cdots + c_j X_{ij} + \cdots + c_n X_{in}, \qquad (3)$$

such that

$$\frac{A^2}{Q} \qquad (4)$$

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[1] For differences between these methods other than those presented in this discussion, see George W. Ladd, "Linear Probability and Discriminant Functions," *Econometrica*, 34 (October, 1966), 873–885.

is maximized, where

$$A^2 = (c_1d_1 + c_2d_2 + \cdots + c_jd_j + \cdots + c_nd_n)^2,$$

$$d_j = \sum_{i=N_1+1}^{N} X_{ij}/N_2 - \sum_{i=1}^{N_1} X_{ij}/N_1,$$

i.e., the difference of the respective means of X_j in the two groups, and

$$Q = \sum_{j=1}^{n} \sum_{k=1}^{n} c_j c_k \sum_{i=1}^{N} x_{ij}x_{ik} \quad (x_{ij} = X_{ij} - \overline{X}_j),$$

i.e., a quantity proportional to the variance of (3).[2]

By taking the partial derivatives of (4) with respect to each c and setting them equal to 0, one obtains the system of normal equations which—in matrix and vector form—can be written as

$$(x'x)c = d. \tag{5}$$

This system of equations is solved for the unknown c's, the discriminant function coefficients. In contrast to system of equations (2), it can be seen that no observed dependent variable is incorporated in the equations from the solution of which the values of the discriminant function coefficients are obtained. Consequently, the values of these coefficients do not depend on the values of such a variable, and the entire analysis is independent of the assumption of the existence of such a variable.

Lemieux states that the "recommended transformation is unnecessary." The transformation in question is the conversion of regression coefficients based on a dependent variable $Y = 0,1$ to discriminant function coefficients and the corresponding computation of discriminant function values Z_i, in accordance with the theorem that establishes the relationship

$$c_j = b_j \frac{N}{N_1N_2}. \tag{6}$$

With appropriate substitutions, this relationship can be derived from the solutions of the respective normal equations of regression analysis and discriminant analysis, as was shown in the outline of the proof of the theorem in the appendix to my article. Furthermore, in his conclusion, Lemieux refers to "a methodological procedure which is needlessly time-consuming, which provides no additional information beyond that available from ordinary multiple regression, and which is no more effective in fulfilling the goal of classifying units of observation into dichotomous groups."

Apparently, "methodological procedure" as employed by Lemieux in this context does not pertain to the use of multiple regression with a dependent variable $Y = 0,1$ for reaching the results of discriminant analysis—the major part of the method proposed in my article, for he cogently argues that this approach is proper. It appears, therefore, that, in using the term "methodological procedure," Lemieux is referring to the conversion of the regression coefficients to the discriminant function coefficients in accordance with (6) and to the corresponding computation of the discriminant function values. A minor point in this connection is that, in view of the computation facilities which are available at the present time, the procedure in question actually cannot be regarded as "time-consuming."

With regard to the important points in his comments, Mr. Lemieux states correctly that the results of multiple regression, if performed under the stated conditions of $Y = 0,1$, classifies units of observation into dichotomous groups as well as discriminant analysis does and provides the same information. For this reason, the method proposed in my article was recommended. Furthermore, R^2 and F as obtained from regression, to which Lemieux also properly refers, provide measures of significance for discriminant analysis. The argument, however, that the conversion of regression coefficients to discriminant function coefficients in accordance with (6) is unnecessary cannot be sustained if a proper conceptual framework for discriminant analysis is to be employed. There is a decisive difference between performing multiple regression with $Y = 0,1$ and then converting the regression coefficients to discriminant function coefficients on one hand and accepting the results of such regression without converting the coefficients on the other. From equation (1) and system of equations (2), it can be seen that the values of the coefficients obtained from regression analysis clearly depend on the particular values of Y. In the case under consideration, they reflect values of 0 or 1 for Y. If other values for Y were chosen, the regression coefficients would be correspondingly different. It should be noted, therefore, that the values of regression coefficients are based not only on the assumption of the existence of an observed dependent variable, but also on the particular values of such a variable. This characteristic of the coefficients is improper for discriminant analysis. On the other hand, from equations (3) and (4) and system of equations (5), it can be seen that the values of the coefficients obtained directly from discriminant analysis do not depend on the values of an observed dependent variable and are not based on the assumption of the existence of such a variable. Precisely these values of the discriminant function coefficients are obtained by the conversion presented in my article. Consequently, if multiple regression analysis with $Y = 0{,}1$ is used for attaining the results of discriminant analysis—a perfectly proper method, then the conversion of the obtained regression coefficients to discriminant function coefficients is indeed necessary, in order to maintain the proper underlying assumptions of discriminant analysis.

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[2] See R. A. Fisher, "The Use of Multiple Measurements in Taxonomic Problems," *Annals of Eugenics*, 7 (1936), 179–188. Gerhard Tintner obtains the same normal equations as given by Fisher by maximizing $A^2 - \lambda(Q - K)$, where λ is a Lagrange multiplier and K is a constant; see his *Econometrics* (New York: John Wiley & Sons, Inc., 1952), pp. 96–102. The notations used by Fisher and Tintner, respectively, have been modified in this presentation.

FRED KORT
University of Connecticut

TO THE EDITOR:

The fact that articles exhibiting the misuse of multiple regression analysis appear so frequently in political science leads me to make the following observations with reference to the article by Philip Stewart et al., "Political Mobility and the Soviet Political Process: A Partial Test of Two Models," which appeared in the December 1972 issue of the *Review*.

My basic criticism is this: a major portion of Stewart's article is concerned with interpreting multiple regressions run on very small samples; this is a highly questionable procedure. Multiple regression analysis is generally inappropriate for small samples because of the fact that the least squares solution maximizes the correlation of the independent variables with the dependent variable. In small samples this characteristic is compounded by the fact that the least squares estimate capitalizes on chance variation. As a result, the estimates of R^2 become more and more distorted (i.e., inflated) as sample size is reduced because of the loss of degrees of freedom. The Stewart article provides a good example of a political scientist who is evidently unacquainted with this aspect of multiple regression and who thus spends a great deal of time "interpreting" what is in large part a statistical artifact of applying multiple regression to very small samples.

J. P. Guilford provides a standard formula which Stewart et al. might have used to deflate the artificially high estimates which they obtained from their small samples:

$$c^{R^2} = 1 - (1 - R^2)\left(\frac{N-1}{N-m}\right)$$

where:

c^{R^2} = corrected R^2
N = sample size
m = number of variables

(J. P. Guilford, *Fundamental Statistics in Psychology and Education,* 4th edition, McGraw-Hill Book Company, 1965, pp. 400–401.)

Utilizing Guilford's formula, I have recomputed the figures which Stewart and his associates provide for R^2 in Tables 3, 5, and 6. The results are presented below.

Table 3. Per Cent of Variance Explained

Year	Reported	Corrected
1955–57	36	22
1958–60	31	19
1961–63	72	58
1964	64	40
	29	15
1965–67	13	0*

Table 5. Per Cent of Variance Explained

Oblasti	Reported	Corrected
Unimportant	19	1
Agricultural	76	28
Industrial	13	0*
Mixed	30	19

Table 6. Per Cent of Variance Explained

Political Mobility	Reported	Corrected
Pre-1938	28	16
Post-1938	50	35

* In Tables 3 and 5 Guilford's correction for inflated R^2 yields a negative value for per cent of variance explained; this is reported here as a corrected figure of 0.

These computations do not mean that Stewart's findings are meaningless, but rather that they are far less striking than the picture conveyed in the article. One cannot use multiple regression with 7 cases and 5 variables (as was done in one instance in Table 5) and expect to generate stable estimates.

Until political scientists can rid themselves of their seemingly insatiable desire to employ high-powered statistical techniques on inadequate data bases, what Abraham Kaplan has called "The Law of the Hammer" will be characteristic of the discipline. Just as a child with a hammer soon finds that everything needs hammering, there is an alarming tendency for social scientists to feel the same way about their statistical techniques. This is something that we should all try to avoid.

DAVID PFOTENHAUER
U.S. Department of State

TO THE EDITOR:

In responding to the Pfotenhauer letter concerning the use of multiple regression in my December, 1972 *APSR* article (pp. 1269–1290), first, I agree with Mr. Pfotenhauer that multiple regression techniques are often misused in political science. Rarely do authors employing these techniques utilize corrections for small samples. Where small samples must be utilized, some correction is appropriate, such as Guilford's R^2 or the R^2 reported by Ezekiel and Fox (*Methods of Correlation and Regression Analysis,* 3rd edition [New York: John Wiley & Sons, Inc., 1954] pp. 300–305).

Second, although most of Mr. Pfotenhauer's comments on the use of regression in my article are either inaccurate or inappropriate, I accept the spirit of his critique to the effect that I should have reported some form of a corrected R^2 in the three tables he cites. This would have been more consistent with the general approach taken in the body of the text in the interpretations of the statistical tables. As a careful reading of this text would show, the multiple R is referred to in only two places, once on page 1277 and once on page 1284. In both cases the intent was to summarize the impact of "Regime." Utilizing the somewhat more adequate ${}_cR^2$ in no way alters the conclusions advanced, however. Conscious of the inverse relationship between (1) the ratio of N to the number of variables and (2) R^2 when N is small, I restricted my analysis, with these two exceptions, to the bivariate regression coefficients.

Third, while applauding Mr. Pfotenhauer's insistence upon greater methodological rigor, let me comment briefly on some of his remarks which, in the context of my article, are either inappropriate or inaccurate. Mr. Pfotenhauer states that his "basic criticism" is that "a major portion of Stewart's article is concerned with interpreting multiple regressions run on very small samples." This statement contains two inaccuracies. As noted above, in only two places in nine pages of data analysis and conclusions are multiple regressions used as the basis of my interpretations. Where they were used it was to contrast their relative size from one regime to the next. These same relative sizes appear in the corrected R^2. Moreover, the author refers to the varying N's in my study as "samples." As this is not only inaccurate, but also makes much of his critique inappropriate to my article, this point deserves some elaboration.

As I noted in the statement of my research design, the group of 224 persons-*oblast*-tenures in this study is "analytically defined as a population." The small N (7 in the agricultural *oblasti,* for example) are still populations and not samples. Now, what are the implications of this for Mr. Pfotenhauer's critique? There are three factors associated with small N size that may lead to a biased R^2, only 2 of which apply to populations. The first is sampling error which is clearly not a source of error in an R^2 based on a population. A second source of bias is loss of degrees of freedom. The problem arises from the fact that "a straight line fitted to two observations will pass precisely through both," as noted by Ezekiel and Fox (p. 300). This problem is associated with regression whether it is used inferentially or descriptively. It was awareness of this source of error that led me, while reporting R^2 in the normal manner, to base my analysis on the bivariate relationships. The third source of error is what Mr. Pfotenhauer, almost quoting Guilford, refers to as "chance variation," but which is more generally referred to as measurement error. Again, awareness that the multiple R does capitalize on those measurement errors *that favor high multiple correlations* also led me to adopt the more cautious approach of restricting interpretation to the bivariate relationships.

Two additional comments seem appropriate here. First, Mr. Pfotenhauer apparently misinterprets his source (Guilford) when he argues that "multiple regression is generally inappropriate for small samples (and, generally speaking, statisticians refer to anything under 100 as a small sample with respect to regression)." What Guilford does say is that "for small samples—and for multiple-correlation problems this means anything less than an N of 100—*degrees of fredom should be considered*" (italics mine). This is far different from branding multiple regression "inappropriate"!

Finally, what it seems to me that all this adds up to is not Pfotenhauer's appeal against the "Law of the Hammer," but two rather different pleas: (1) that critics carefully read the text of the works they "critique"; and (2) that scholars instead of abandoning sophisticated techniques, seek an even greater level of understanding of both the limits and potentials of those methods available to us.

PHILIP R. STEWART
The Ohio State University

TO THE EDITOR:

Professor Edward Tufte's article, "The Relationship Between Seats and Votes in Two Party Systems," *American Political Science Review*, 67 (June, 1973), 550–554, it should be said at the outset, is extremely well done technically. It is also—for me—very *thought-provoking* about matters of great substantive importance in the longitudinal development of American electoral politics. I was thus prompted (largely for my own ongoing work files) to draft a series of rather extensive reflections on this article. But such disagreements as I have with its analysis should be viewed in terms of the intellectual stimulation which the discussion provided; and not as reflecting on the quality of the work done.

(1) It is very obvious that the so-called "cube law" does not fit most empirical cases mentioned by the author.

(a) The most extreme cases of lack of fit, in order, are New Jersey (32 elections); US, 1868–1970; New York (20 elections) and Michigan (10 elections). It would be useful to note that bias (deviations from equality) may arise from any of the following factors, some of which the author discusses below:

(i) Malapportionment (quasi-universal in American states before 1965 or so), or nonapportionment (as in German Reichstag, 1871–1918). New York State and Michigan are excellent examples of this with (largely) single-member constituencies; in New York, deliberate malapportionment was one of the chief political goals of the Constitutional Convention of 1894. In New Jersey, one has both malapportionment and (after 1894, again!) conversion from single-member districts to county-wide at-large elections. (But there are still problems with the extraordinary N.J. chart, to which we will allude later.)

(ii) Heavy regional concentrations of one-party (uncontested) elections: especially if these are concentrated asymmetrically in favor of one party rather than the other. Such has been the case in the vast majority of American congressional elections from 1878 to the present time: the South the region, the Democrats the favored party. (It is this, by the way, which—in conjunction with other attributes of voting behavior outside the South—produces the weird results for 1924–1928, cf. p. 550.)

(iii) Behavioral change in the electorate, skewed systematically in some fashion. This may involve such phenomena as found in the N.J. case after 1930, or the much more politically significant (and recent) development of bimodal partisan distribution of district outcomes since about 1964–66 (the latter, of course, contributes heavily to a drop in the swing ratio; the former to the extreme scatter in the New Jersey plot). Similarly, one recent pattern of behaviorally related bias mentioned by Tufte (p. 549) is the monotonic relationship between size of district and relative Republican success. As to the latter, it would be *most* desirable to obtain some *longitudinal* data: to the extent that it may be a permanent bias, it of course washes out as an explanation of the post-1964 shift in the swing ratio. There is reason to suspect that, in fact, this pattern was *even more extreme prior to* reapportionment after *Wesberry* v. *Sanders* (1964). As to the post-1964 behavioral change itself, there is excellent reason to believe it to be linked to such things as decay of party as a cue for voting, the rise of uses of congressional franks, size of staffs, etc., by incumbents; see David Mayhew. "Congressional Elections: The Case of the Disappearing Marginals" (1973 NEPSA paper).

I turn briefly to one case before proceeding further with the discussion. The first is the strange case of New Jersey (any presidential year from 1932 through 1944 will do). Here we have the following structural issues. (a) The lower house of the legislature was elected annually in November, which means that it was on the same "ticket"—better, was voted for on the same day—as President every fourth year. (b) The elections were at large within county units after 1894. As a piece of contextual information, it should be noted that after the national critical realignment of 1896 a very large gap opened between presidential and state (especially governor) outcomes, with the Democrats favored in the latter. Decomposition of an initially very tight longitudinal partisan pattern was a chief reality in N.J.

County	% of 2-Party Pres. Vote		Seats won by Party		% of Seats won by Party	
	D	R	D	R	D	R
Atlantic	54.2	45.8	·	2	0	100.0
Bergen	36.8	63.2	·	5	0	100.0
Burlington	55.7	44.3	·	1	0	100.0
Camden	66.1	33.9	3	·	100.0	0
Cape May	47.4	52.6	·	1	0	100.0
Cumberland	54.1	45.9	·	1	0	100.0
Essex	45.9	54.1	·	12	0	100.0
Gloucester	53.4	46.6	·	1	0	100.0
Hudson	66.0	34.0	10	·	100.0	0
Hunterdon	43.4	56.6	·	1	0	100.0
Mercer	57.4	42.6	2	1	66.7	33.3
Middlesex	61.7	38.3	3	·	100.0	0
Monmouth	42.2	57.8	·	2	0	100.0
Morris	38.3	61.7	·	2	0	100.0
Ocean	39.5	60.5	·	1	0	100.0
Passaic	51.6	48.4	·	4	0	100.0
Salem	60.1	39.9	1	·	100.0	0
Somerset	45.0	55.0	·	1	0	100.0
Sussex	42.2	57.8	·	1	0	100.0
Union	46.9	53.1	·	4	0	100.0
Warren	50.8	49.2	·	1	0	100.0
NEW JERSEY	51.8	48.2				
"Seats" (incl. elect. coll.)	16	0	19	41	31.7	68.3

politics, beginning very shortly after 1896.

We turn to the New Jersey results for a single year, 1940 (unfortunately, I haven't processed the actual vote for state representatives; so we must link the presidential two-party vote with the legislative *outcomes* by county). We can now make a few counterfactual statements. First, *if* New Jersey had had P.R. *and* no split-ticket voting, the lower house (N = 60) would have been 31D, 29R. Second, *if* under the winner-take-all system all counties with a Democratic majority had elected a solid slate of Democratic legislators, the result would have been a tie: 30D, 30R. Precisely this is what did not happen; and, as an empirical statement for 1940, we can say that the Democrats needed to win between 57.4 per cent and 60.1 per cent of the vote in order to elect a "solid" slate, while the Republicans could win as little as 42.6 per cent of the two-party presidential vote in a county and still pick up a representative, and as little as between 44.3 per cent and 45.8 per cent and elect a solid Republican legislative slate. It is obvious that a *chief* reason for the discrepancy between statewide partisan vote for President and legislative outcomes is that a substantial minority of the electorate—in strategic places—*voted a split ticket* with a net and quite strong Republican benefit at the "grass roots" legislative level. Moreover, the whole history of the period from the early 1930s to the late 1940s is replete with such "pied" electoral outcomes; they stand in the strongest possible contrast with linkages before 1894. Nor can there be doubt that the "winner-take-all" feature of state legislative elections, coupled with this split-ticket voting, helps to account for this result. As the result on pure "winner-take-all" feature of state legislative elec-would have been 30D, 30R, so the result on a similar basis but with proportionality in multimember counties would have yielded D 33, R 27.

All of this is but to make the central point that *both* behavior and rules of the electoral game may significantly alter outcomes in any legislature from those which might be expected either through the cube-law approximation or through Tufte's rough rule of thumb, a transformational ratio of 1:2.5.

(b) It is equally obvious that, as we have suggested, a relationship between votes and seats in the U.S. House will necessarily deviate more or less extensively from a cube law approximation if and to the extent that large numbers of seats are won uncontested, and these seats are heavily differentiated by party. Such is the case for most of U.S. history—not infrequently we find 70 to 75 Democratic seats uncontested (virtually all in the South), to between 3 and 5 for the Republicans. As one could not expect anything like a cube law ratio to obtain in plotting *electoral college* outcomes because of the lumpiness of "winner-take-all" (analogous to the New Jersey case), so we could not expect any close fit under these circumstancs either.

As I have developed elsewhere in some detail, exclusion of the South and border states yields very different results across the whole time from those suggested by Tufte. With the greater South excluded, the data file suggests that *in the very long run* American congressional elections have closely corresponded to an approximation of the cube law formulation:

Period	*N*	*Y*=	$r^2\times100$
1874–1972	50	$-0.920+2.810X$	85.8
Cube Law	—	$-0.904+2.808X$	—

To summarize: I am no particular devotee of the cube-law formulation, especially as it purports to constitute a description of—and generalization from—empirical data. (Even at that, it does seem to me that Tufte's arguments that the formula is hard to remember and compute are rather strained.) But it remains quite possible that the cube law is actually a "model," or a valid *unbiased estimate* which is approximated more and more closely as one adds *N*s of cases, *to the extent that ceteris paribus assumptions are not violated by systematic biases in either behavior or electoral-rule structure*. In other words, the "law of large numbers" may have something to do with the cube law, as with other artifacts of statistical method. To the extent that this *may* be so, and to the extent that procedural and behavioral factors of prime importance may work toward producing a systematic bias, it would follow that Tufte's rejection of the cube law may be premature and less well grounded than the article suggests.

(2) Clearly the most suggestive part of the whole paper is that which deals with long-term changes in "swing ratios." If one were to retain the cube law as a heuristic, such changes represent more or less extensive deviations from the "expected"—and there is a very strong (though hardly linear) longitudinal pattern of such changes. Whether or not one does retain it, my disagreements with this section of Tufte's article are very few. The only major one deals with his attempted structural (bipartisan gerrymander) explanation for the steep decline in congressional swing ratios during the post decade. Otherwise, the author's long-term analyses are both extremely interesting and persuasive.

Probably the most interesting point of all, so far as understanding certain peculiarities of American politics is concerned, is developed by the author on p. 547:

> ". . . the more nationally oriented the politics of a country or the more nationalized the forces prevailing in a given election, the greater the swing ratio—other things being equal."

I agree. But as applied to *House* elections, the very steep decay in the amplitude of swing ratios then bears the inference that *a progressive denationalization of congressional elections and congressional politics has unfolded during the past century*. This proposition is not only counterintuitive (can one think of a single comparative case where this could plausibly be said?) but intersects strangely with Donald E. Stokes's argument in his article on congressional elections in Chambers & Burnham (eds.), *The American Party Systems.* Yet there is very strong evidence to support the existence of long-term decay in congressional seat swing; and there is more than a little collateral evidence to support the view that *state* elections were much more nationally oriented, say, in 1866 or in 1893 than they are today. (See articles on Conn. state election of 1866, *Appleton's Annual Cyclopedia,* 1866; and Ohio state election of 1893 in *Ibid.,* 1893).

Lest one think that this may simply have been an artifact of the one-partyism which developed decisively in the South, after 1900, we again advert to our series of vote-seat calculations for the three-quarters of the country not "Southern" in a broad sense. The size of the slope in the regression tells much the story Tufte points to—though we have selected five rather than three elections at a time.

Of course, these *b*'s are in no sense identical with the swing ratios the author uses. The story is essentially the same, however, as that in Tufte's argument. One may add here a point. The argument *neither confirms nor denies the*

Period	N	$b=$	$r^2\times 100=$
1874–1882	5	4.383	88.4
1884–1892	5	8.619	88.2
1894–1902	5	3.295	96.8
1904–1912	5	3.234	97.2
1914–1922	5	2.510	99.4
1924–1932	5	2.763	97.4
1934–1942	5	3.709	99.7
1944–1952	5	3.044	98.6
1954–1962	5	2.328	70.2
1964–1972	5	1.528	92.0

existence of critical realignments: it simply ignores the problem, since its analytical concern is elsewhere. (Personally, I think the argument—especially the periods selected—would have been improved by thinking about these changes in terms of realignment theory, but that's another issue. . . .) One may nevertheless hazard some generalizations. First, before the realignment of 1893–96, the transformational ratio was extremely high (*e.g.*, for the 14 elections, 1866–1892, $b = 5.595$). Subsequently, it was depressed (unevenly but monotonically) until the trough was reached in 1914–22 (another arrangement would probably concentrate this in the mid-1920s, as Tufte suggests); then a resurgence, followed by a movement toward the all-time lows of the recent period. Second, it appears very probable that these changes were associated very intimately with the transformations in mass voting behavior which accompanied these realignments, *i.e.*, those of the 1890s and 1930s. Thirdly, we still have to explain the steep drop in both b's and Tufte's swing ratio in the post-1960 period.

Here Tufte's argument about the effects of bipartisan gerrymandering of districts is ingenious but not ultimately convincing. For there is a host of evidence—some from states like West Virginia and New Hampshire, where one party controlled everything and no bipartisanship (or even gerrymandering) was involved—to support the view that *the most important single factor has been systematic change in mass voting behavior since 1960.* Very bluntly put, party as a referent for voting decisions has disintegrated at a very rapid rate during the past decade; and, "liberated" from such cue-giving constraints, voters have increasingly turned to other cues. At the level of the HR, for a variety of reasons of *saliency* and *access to mass communications,* among others, this has meant that critically large minorities of the electorate *vote for incumbents* whatever is happening elsewhere on the national or state ticket (see Miller & Stokes, 1958, on this—reprinted in *Elections and the Political Order*).

We do not try here to propound a theory of mass voting behavior which accounts for these phenomena. It is enough to say that the very high b's and swing ratios of the late 19th century were associated with a period in which party ID and party voting were extremely salient, by all aggregate indicators. Similarly, the very low b's and swing ratios of the post-1964 period are associated with a period of "final" (?) decadence of party as a major symbolic and cue-giving intermediary between voters and the choices they make at the polls. Longitudinal analysis of the relationship between votes in CDs for congressman and for other offices, when performed, will duly reveal this to have been the case in the 1920s, and most of all, in the period beginning around 1960 to 1964, depending on the locality.

(3) The broader theoretical issues which this paper presents are of major importance to scholars in this field. Granted the discussions of mass voting behavior now in the literature, and covering the whole period from the Civil War to the present, it is in the highest degree anomalous to suggest that the US has been marked by progressive *denationalization* of electoral forces, in a wave-like pattern which is closely linked in time with realignment sequences and their consequences. But such must be the verdict, so long as we associate transformational ratios, parties and elections together. This of course must be done if any intelligent arraying of 435 outcomes per election is to be attempted, and especially the partisan component must be included. But if the reality is that we are "nationalizing political forces" along lines which *cross-cut* increasingly soluble parties, then we need quite novel nomenclature and analytic concepts to deal with so momentous a change. In any event, it should be obvious that reliance on bipartisan gerrymanders as a simple structural intervenor is reliance upon a frail reed (which in itself requires explication, one may add, in terms of changing elite norm standards). The overwhelmingly important change has been *behavioral,* not structural; and if this is not fully clear to our professional colleagues now, it soon will be.

I will conclude with a prediction. It may be that Watergate and other issues will produce a 60 per cent D, 40 per cent R result in the 1974 congressional elections, as Gallup now suggests. If so, this will cut deeply enough into the Republican "mode" of our now bimodal distribution to result in a significant Democratic party swing—perhaps 25 to 30 seats. Even so, I predict two things. First, the distribution will *continue* to be bimodal for the foreseeable future; and second, most of the Democratic "new men" will be able to entrench themselves without much difficulty, even with a "national" Republican swing in 1976. To believe otherwise is to believe that powerful forces for reintegrating partisan-oriented voting behavior are developing in the American electoral system. I see no evidence which supports any such view as of 1973.

WALTER DEAN BURNHAM
Massachusetts Institute of Technology

TO THE EDITOR:

Professor Burnham's comments, while far from proven findings, contain several interesting speculations and represent a useful extension of my paper.[1] Here I wish to develop some of his remarks with a bit of new data and, in addition, consider several other recent studies bearing on my results.[2]

The Cube Law

Several recent discussions have found great virtues in the cube law and Burnham makes a brief defense of the law. Although most of the case against the cube law was made in my paper, perhaps some additional comments are in order. The cube law makes a specific prediction about the relationship between seats and votes—that the elasticity of the seat odds with respect to the vote odds is 3.0, and that the bias of the electoral system is zero. In the hundreds of such elasticities and biases that I have examined in many different electoral systems, perhaps 80 per cent are inconsistent with the predictions of the law. I find this very disenchanting and the citation of the few particular times the law fits unconvincing. The two remaining defences of the cube law in the most recent papers seem to be: (1) "It's the only law political science has got"; and (2) "Most laws (such as physical laws) don't work very well either." Neither line of argument has much appeal in the face of the lack of theory leading to a sound and specific derivation of the law and the law's poor fit to the data. One way to view the whole matter is that the questions of any *political interest* in my study, in Burnham's comments, and in Mayhew's analysis, would not be raised if one really believed in the cube law—because the law simply assumes away important political questions. So why is it, then, that the literature on electoral systems is cluttered with respectful references to the cube law, and why do some find it hard to kick the habit? It is partly because the mathematical format of the cube law gives it, for some, an undeserved credibility; but, in addition, the cube law has had appeal because it is one of those charming, harmless story-explanations that so often embellish perennial celebrations—like Santa Claus or the Easter Bunny. It should be left at that.

The Longrun Votes-Seats Relationship for the U.S. House of Representatives

Far more important than the cube law is the question of the longrun decline in the swing ratio and the increasing job security of incumbent representatives in the last few elections. The table shows the postwar trend in the incumbency of incumbents and the decline of competitive seats. (The swing ratio is estimated through a formalization of the method displayed in Figure 4 of my previous paper. This method yields an estimate of both the bias and the swing ratio for *each* election over any given set of districts.) The table indicates a substantial recent decline in the swing ratio, in the competitiveness of congressional seats, and in the number of incumbents defeated by nonincumbents.

In my paper I suggested, without a great deal of supporting evidence, that redistricting—particularly "bipartisan" redistricting—was making a substantial contribution to the recent decline in political competition and increase in the tenure of the incumbents evident in the table. Mayhew's recent paper has considered a variety of other factors, particularly the increasing advantage of incumbents compared to nonincumbents in the resources necessary to win and keep office. And Burnham, in this communication, suggests that "behavioral change" of voters is key: "the most important

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[1] Edward R. Tufte, "The Relationship Between Seats and Votes in Two Party Systems," *American Political Science Review*, 67 (June, 1973), 550–554.

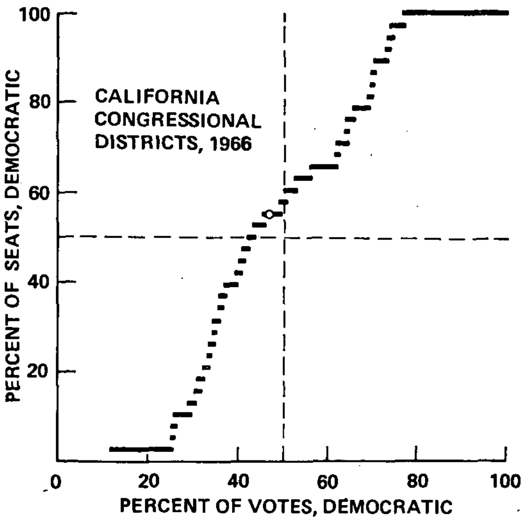
[2] David Mayhew, "Congressional Elections: The Case of the Vanishing Marginals," *Polity*, forthcoming; Thomas W. Casstevens and William D. Morris, "The Cube Law and the Decomposed System," *Canadian Journal of Political Science*, 5 (December, 1972), 521–532; David Sankoff and Koula Mellos, "La regionalisation électorale et l'amplification des proportions," *Canadian Journal of Political Science*, 6 (September, 1973), 380–398; Rein Taagepera, "Seats and Votes: A Generalization of the Cube Law of Elections," *Social Science Research*, 2 (September, 1973); and Rein Taagepera, "A Generalization of the Cube Law of Elections," paper prepared for delivery at the 1973 annual meeting of the American Political Science Association, September, 1973. See also Henri Theil, *Statistical Decomposition Analysis* (New York: Elsevier, 1972), pp. 204–217, which finds that the cube law fails in the American case.

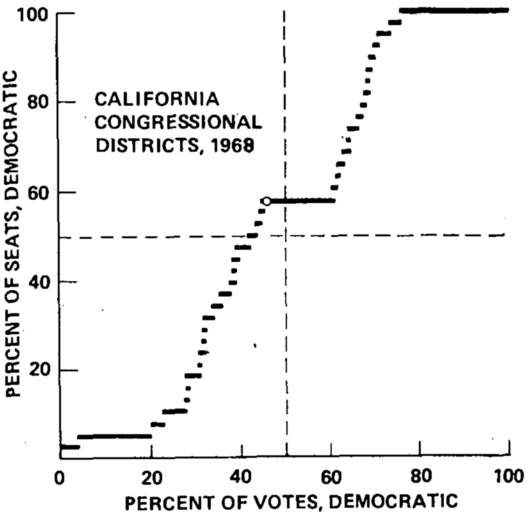
U.S. HOUSE OF REPRESENTATIVES, 1946–1972

Year	Swing ratio: per cent change in seats for a 1 per cent change in votes	Percentage of seats in which winning candidate received less than 53 per cent of votes	Number of incumbents defeated by nonincumbent opponents
1946	2.11	13	52
1948	2.64	17	68
1950	2.09	13	33
1952	1.89	11	22
1954	1.98	11	22
1956	2.05	11	15
1958	2.37	15	35
1960	1.98	11	26
1962	1.76	11	15
1964	2.51	15	45
1966	1.73	11	40
1968	1.29	6	5
1970	1.26	8	12
1972	1.16	8	10

single factor has been systematic change in voting behavior since 1960." This might possibly be the case, although the highly aggregated evidence presented in support of that view can not provide any inferential leverage on explanations based on individual behavior: it is simply too much af an inferential leap to go from changes (often not statistically secure) in nationwide swing ratios computed by decade to inferences about the behavior of individual voters in particular decades. For more recent times, survey evidence might provide some evidence bearing on Burnham's hypothesis.

The more important task, it seems to me, is in allocating the effects on political competition of redistricting on the one hand and the increase in incumbent resources on the other. Mayhew's paper suggests several tests and, although some of the recommended evidence bears very indirectly on the two explanations, some of the tests seem quite helpful. One further test useful for separating out the trend effects from the redistricting effects on incumbency is to examine the swing ratio and the bias in a state before and after redistricting. The swing ratio and the bias can be estimated from the plotted votes-seats curve for each election. Consider, for example, the two votes-seats curves for California in 1966 and then in 1968. The 1966 curve displays a good deal of bias, but at least the curve is moderately steep in the area in which elections take place—that is, between 40 and 60 per cent of the vote divided between each party—indicating that for reasonable changes in votes there will be reasonable changes in seats. Note, however, the development of the flat spot in the middle of the 1968 curve, indicating that even very large changes in the partisan division of the vote will not produce any change in the partisan division of the seats at all. If the Democrats in 1968 had gained an additional 15 percentage points in every congressional district in the state, they would not have gained a single additional seat. (The system is, however, biased in favor of the Democrats—note that they gain a majority of the congressional seats with about





45 per cent of the votes in both 1966 and 1968.)

The plateau in the 1968 curve is in that part of the curve where the statewide vote division is between 40 to 60 per cent Democratic—that is, in the area where almost all electoral outcomes fall. Why did such a flat spot in the central part of the curve develop between 1966 and 1968? There was a court-ordered redistricting that, as one study dryly put it, "did not materially affect the political fortunes of any incumbent."[3]

A more general point: such seats-votes curves can be used to estimate both the bias and the swing ratio of the electoral system in each election. These two descriptions of the electoral system can then be studied over time, for each state and for the nation as a whole, and changes in the performance of the electoral system can then be related to other factors, such as redistricting and shifts in the balance of electoral resources between incumbents and nonincumbents.

Burnham's letter raises a good many other points, several of which are speculative and not yet supported by evidence. The letter does indicate, however, the intriguing aspects of the seats-votes relationship and how the study of that relationship opens a broad range of political, historical, and even statistical questions.

EDWARD R. TUFTE

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[3] John F. Gallagher and Louis F. Weschler, "California" in Eleanore Bushnell, ed., *Impact of Reapportionment on the Thirteen Western States* (Salt Lake City: University of Utah Press, 1970), p. 86.

EDITORIAL COMMENT

Rejection. The worst chore that falls to the managing editor of this *Review* is rejecting manuscripts for publication. We are only too painfully aware that scholarly careers depend upon the ability of scholars to get their thoughts into print, and that intellectual discourse cannot be shaped except by what readers are given an opportunity to see. We have commented before on our relief that there are other journals to which authors can turn with their manuscripts rejected here. At the very least, this provides an appeal from inevitable mistakes. Mistakes, however, either of omission or commission, sooner or later mount up and fall into a pattern, and define the limitations of a managing editor in the exercise of his stewardship. A fortunate editor is one who discerns this pattern before most of his readers do, and who forthwith musters up the good grace to pass the job along to fresh hands.

In the meantime, of course, he busies himself shaping the scholarly conversation of his colleagues, and willy nilly messing about with their careers. One observer of this process in his own discipline is a distinguished historian of early modern England, Professor J. H. Hexter. In the preface to his *Reappraisals in History* (New York: Harper Torchbooks, 1963) Hexter says:

> The rejection of their early efforts sometimes unduly discourages young historians. They may perhaps be raised from unwarranted despair by the contents of this book and by its publication. Three of the essays in it were rejected by scholarly journals in good standing . . . All of the rejections came when the author was over forty. A letter of rejection is not a divine decree; it is neither an immutable nor an eternal judgment, but the decision of one or two fallible men, subject to reversal by other men equally fallible. So let young historians take heart; and in this matter may all historians be young at heart.

The Uncertain Wages of Virtue. A political scientist boggled our minds recently by saying, "I know—or at least can figure out—what good it does the *Review* to have editorial interns slaving away checking footnotes and quotations. What I can't figure out is what good it does the editorial interns."

We had to assure our interrogator that service as an editorial intern was purely voluntary. Some graduate students have become editorial interns by asking to join at a time when there was a vacancy. Others have been asked and have declined. Mostly, when students have been invited to become interns, they have accepted, and they seldom drop out before leaving the Berkeley graduate program altogether. Starting in the fall of 1973 several Stanford graduate students also elected to join the program.

This constitutes a weak prima facie case for the proposition that the interns like to be interns, however dubious the status, and despite the annoyance of the work and the downright rottenness of the pay.

For what it's worth, interns do get something back. We have a seminar, once a month, not for credit, at which we generally go over an article that has been accepted for *Review* publication. Sometimes, the author joins the group for the evening. Sometimes, we invite a political scientist in whom the group has an interest to assign us an article he has written, and we devote a session to discussing that. In this fashion the interns were able to get a preview of Heinz Eulau's Presidential Address, and meet with such diverse and interesting political scientists (among others) as Michael Walzer, Alexander George, Austin Ranney, Ali Mazrui, and Frederick Frey.

These evening meetings are held in the managing editor's living room, and sometimes they can get pretty raucous. This, needless to say, increases the morale of the group. Social scientists have always noted the correlation between noise level and group solidarity, but what they have failed to understand until now is that the one causes the other.

This is one answer to the question what do editorial interns get? Another answer is that once a year we have dinner at the expense of the Association. Any pleasure that interns receive from this act of modest largesse is somewhat mitigated by the fact that they have to listen to a short speech by the managing editor.

Last year, his mind still boggled by the question with which we began this comment, he said:

> I've been wondering whether the experience of working side by side together on the *Review* has helped in any way, and whether we can make the experience any better for the people who will be coming along to take your places.
>
> What my thoughts have led me to is this: In my view, going through graduate education is an incredibly challenging experience. It entails the learning of a complex array of skills. It means the mastery of large and difficult bodies of written material. It means focusing your energy and intelli-

gence in such a way as to produce new and original combinations of ideas. It means learning new ways to think and talk about things. It means adopting a new identity.

Amidst all these heavy demands for focused intelligence, for mastery in depth, your work for the *Review*—our monthly seminars on random subjects, your occasional job of footnoting and nit-picking—can only come to you as a distraction.

What I'm hoping is that for at least some of you, some of the time, it has been a life-giving distraction. In order to stay sane, and intellectually healthy, we all of us need peripheral vision as well as focused concentration. We need to let our minds play a little as well as plod a lot.

For those of you who have been able to use the distraction of the *Review* as professional play—and I hope it's most of you—I suggest the possibility that it will help you become better political scientists. By giving you a chance to exercise your judgment, and your wits, and, if I may insert an empirical finding, your voices, free of evaluation by your professors and just for fun, I hope the *Review* has widened your experience a little, and cultivated your taste a little, and improved your minds—just a little.

Every year, tempered—or distempered—by the experience, a few editorial interns leave the nest and go out to seek their fortunes. Former interns are now teaching at Dartmouth, M.I.T., Pittsburgh, Princeton, Illinois, C.U.N.Y., William and Mary, Minnesota, Southern Methodist, and one or two have even made it onto the faculty at the mother church in Berkeley. Not bad.

Erratum: In "The Attribution of Variance in Electoral Returns: An Alternative Measurement Technique" by Richard S. Katz in the September 1973 issue, Table 4 was deleted, and Table 5 renumbered in its place. Table 4 should have read as follows:

Table 4. Correlations Between District National Variance Components and Support of Party Majority on Party Opposition Roll Calls

	Democrats	Republicans
All Roll Calls	.17	.18
Foreign Policy Roll Calls	.16	.04
Domestic Policy Roll Calls	.20	.17

The table appearing on page 827 is correct, but should have been numbered as Table 5.

Articles Accepted for Future Publication

Craig Neal Andrews, Politecon Research and Consulting, Detroit, "Integration and Community in Communist Theory"

William I. Bacchus, Commission on the Organization of the Government for the Conduct of Foreign Policy, "Diplomacy for the '70s: An Afterview and Appraisal"

Paul Allen Beck, University of Pittsburgh, "Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior"

Samuel H. Beer, Harvard University, "Tradition and Nationality: A Review Essay"

Robert A. Bernstein and William W. Anthony, Texas A&M University, "The ABM Issue in the Senate, 1968–1970: The Importance of Ideology"

Gordon S. Black, University of Rochester, "Conflict in the Community: A Theory of the Effects of Community Size"

Walter Dean Burnham, Massachusetts Institute of Technology, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate' "

Edward G. Carmines, State University of New York, Buffalo, "The Mediating Influence of State Legislatures on the Interparty Competition-Welfare Expenditures Linkage"

John Chamberlin, University of Michigan, "Provision of Collective Goods as a Function of Group Size"

Richard Allen Chapman, University of Montana, "*Leviathan* Writ Small: Thomas Hobbes on the Family"

Claude S. Colantoni, Terrence J. Levesque and Peter C. Ordeshook, Carnegie-Mellon University, "Campaign Resource Allocations Under the Electoral College"

Wayne A. Cornelius, Massachusetts Institute of Technology, "Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities"

Bruce Bueno de Mesquita, University of Rochester, "Need for Achievement and Competitiveness as Determinants of Party Success in Elections and Coalitions"

Lawrence C. Dodd, University of Texas, "Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis"

Dennis L. Dresang, University of Wisconsin, Madison, "Ethnic Politics, Representative Bureaucracy, and Development Administration: The Zambian Case"

Peter Eisinger, University of Wisconsin, Madison, "Racial Differences in Protest Participation"

David J. Elkins, University of British Columbia, "The Measurement of Party Competition in Multi-Party Systems"

John A. Ferejohn and Morris Fiorina, California Institute of Technology, "The Paradox of Not Voting: A Decision Theoretic Analysis"

Ada Finifter, Michigan State University, "The Friendship Group as a Protective Environment for Political Deviants"

Peter C. Fishburn, Pennsylvania State University, "Paradoxes of Voting"

Ronald P. Formisano, Clark University, "Deferential-Participant Politics: U.S. Political Culture, 1789–1840"

Mark Gavre, University of California, Los Angeles, "Hobbes and His Audience: The Dynamics of Theorizing"

Ted Robert Gurr, Northwestern University, "Persistence and Change in Political Systems, 1800–1971"

Larry B. Hill, University of Oklahoma, "Institutionalization, the Ombudsman, and Bureaucracy"

Richard Child Hill, Michigan State University, "Separate and Unequal: Governmental Inequality in the Metropolis"

Kenneth Jowitt, University of California, Berkeley, "An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems"

Donald R. Kelley, Monmouth College, "Toward a Model of Soviet Decision Making: A Research Note"

Stanley Kelley, Jr., Princeton University and Thad W. Mirer, University of Wisconsin, Madison, "The Simple Act of Voting"

Jae-On Kim, University of Iowa, John R. Petrocik, University of Chicago and Stephen N. Enokson, University of Iowa, "Voter Turnout Among the American States: Systemic and Individualistic Components"

Walter Korpi, University of Stockholm, "Conflict, Power and Relative Deprivation"

Jesse F. Marquette, University of Akron, "Social Change and Political Mobilization in the United States: 1870–1960"

Arthur H. Miller, Ohio State University, "Political Issues and Trust in Government: 1864–1970"

Thomas C. Nowak and Kay A. Snyder, Michigan State University, "Clientelist Politics in the Philippines: Integration or Instability?"

Stanton Peele, Harvard University and Stanley J. Morse, Pontifica Universidade Catolica de Sao Paulo, "Ethnic Voting and Political Change in South Africa"

John C. Pierce, Washington State University and Douglas D. Rose, Tulane University, "Non Attitudes and American Public Opinion: The Examination of a Thesis"

David E. Price, Duke University, "Community and Control: Critical Democratic Theory in the Progressive Period"

Kenneth A. Shepsle, Washington University, "On the Size of Winning Coalitions"

Brian D. Silver, Florida State University, "Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis"

Arthur G. Stevens, Jr., University of Virginia, Arthur H. Miller, Ohio State University and Thomas E. Mann, American Political Science Association, "Mobilization of Liberal Strength in the House, 1955–1970: The Democratic Study Group"

Peter G. Stillman, Vassar College, "Hegel's Critique of Liberal Rights"

C. Neal Tate, North Texas State University, "Individual and Contextual Variables in British Voting Behavior: An Exploratory Note"

Kent L. Tedin, College of William and Mary, "The Influence of Parents on the Political Attitudes of New Voters"

Timothy A. Tilton, Indiana University, "The Social Origins of Liberal Democracy: The Swedish Case"

Glenn Tinder, University of Massachusetts, Boston, "Beyond Tragedy: The Idea of Civility"

John Wanat, University of Kentucky, "Bases of Budgetary Incrementalism"

Meredith W. Watts, University of Wisconsin, Milwaukee, "B. F. Skinner and the Language of Technological Control"

Herbert Weisberg, University of Michigan, "Models of Statistical Relationship"

Louis P. Westefield, Southern Illinois University, Edwardsville, "Majority Party Leadership and the Committee System in the House of Representatives"

Roger E. Wyman, Rutgers University, "Middle-Class Voters and Progressive Reform: The Conflict of Class and Culture"

The Study of International Politics Makes Strange Bedfellows: Theories of the Radical Right and the Radical Left*

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The political spectrum has often been viewed as a linear continuum on which the extremes of the left and right define not only the end points, but also the most antithetical positions. The alternative hypothesis is that there are some dimensions on which the extremes may resemble each other more than the center. Although the issue has informed studies by psychologists, political scientists, and others, the evidence does not yield a clear cut verdict for either position. For example, authoritarianism has been identified with the extreme right, but these studies have been criticized for using indicators of authoritarianism that are more sensitive to rightwing than to leftwing political attitudes. Partial support for the critics emerged from Rokeach's finding that "closed-mindedness," existing independently of political orientation, can be found on both the right and left. Bunzel has suggested that such rightwing organizations as the John Birch Society share a distrust of politics with organizations of the radical left. As these examples indicate, the question of similarities between extremes on the political spectrum has yet to receive a definitive answer.[1]

This essay examines the theories of international politics and foreign policy espoused by scholars of the radical right and left. They are, to be sure, marked by manifest differences in *content*. The right and left differ, for example in their assessments of American, Soviet, or Chinese foreign policy; on the sources of the cold war or specific conflicts such as those in Korea, Indo-China, or the Middle East; and in the normative goals they espouse. But if we look at the *structure* of the theories there appear to be some striking similarities in their portrayals of the nature of international politics and foreign policy. Because this essay is focused on studies of international politics since the outbreak of World War II, we shall devote considerable attention to the parallels between rightwing theories of the U.S.S.R. and Soviet foreign policy, and leftwing explanations of the United States and American foreign policy.[2] In the pages that follow some two dozen points of possible convergence are grouped under five major headings: *Understanding history and politics, the causes of war, the nature of the enemy, the conditions of peace, and ends and means in politics.*

Before proceeding it is well to point to several important limitations in the present essay. Restrictions of space and time preclude a comprehensive survey of the literature produced by the two radical groups. Explicitly excluded from considerations are works that make no pretensions of being scholarly.[3] Thus observations about current international affairs by Robert Welch of the John Birch Society remain outside the scope of the present essay, as do the writings of what one critic has called the "bomber left."[4] Much of the discussion focuses on several prominent groups of historians and social scientists.

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* For helpful criticisms and comments on earlier drafts, I am grateful to Gar Alperovitz, Barton J. Bernstein, Donald Blake, David Green, K. J. Holsti, Robert Jervis William R. Kintner, Jean Laponce, David McLellan, William Moul, Philip Resnick, Dwight James Simpson, Michael D. Wallace, and six anonymous reviewers for the *American Political Science Review*. This study, part of a larger project on belief systems and foreign policy, was prepared while I held fellowships from the Center for Advanced Study in the Behavioral Sciences and the Ford Foundation; it was written to honor James T. Watkins IV on the occasion of his retirement from the Political Science Department, Stanford University. Acknowledgment of my gratitude to all of the above does not imply that they agree with all or any part of this essay.

[1] T. W. Adorno, Else Frenkel-Brunswick, D. J. Levinson, and R. N. Sanford, *The Authoritarian Personality* (New York: Harper, 1950); R. Christie and Marie Jahoda, eds., *Studies in the Scope and Method of "The Authoritarian Personality"* (New York: Free Press, 1954); Milton Rokeach, *The Open and Closed Mind* (New York: Basic Books, 1960); John H. Bunzel, *Anti-Politics in America* (New York: Knopf, Inc., 1967). For a summary, see Roger Brown, *Social Psychology* (New York: Free Press, 1965), pp. 542–544.

[2] Owing to this emphasis on recent international politics, works focused on earlier periods—for example, W. A. Williams, *American-Russian Relations, 1781–1947* (New York: Rinehart, 1952); or W. A. Williams, *The Roots of the Modern American Empire* (New York: Random House, 1969)—receive little attention in this essay.

[3] In a private communication Barton Bernstein suggested that some of the authors included in this essay do not merit the label "scholar." By some definitions (e.g. that scholarship involves original research in primary materials) Professor Bernstein is quite correct, but perhaps one should not apply excessively restrictive criteria to works focused on such recent events as the Cold War. There is also a danger that dismissing a work or an author as "unscholarly" is merely an invidious form of academic one-upmanship by which the reviewer excuses himself from having to deal seriously with some ideas. The reader who feels that my criteria have been too loose in this matter

The "protracted conflict" school includes several rightwing historians and political scientists who share an interest in Communist affairs and contemporary international conflict. Their work is a response to alleged failures of most Western scholars and statesmen to understand the true nature of Soviet and Chinese communism. Fearing a growing tide of appeasement in the West, they have warned since the early 1950s that accommodation with communism is too high a price to pay for what would, at best, be the temporary comfort of détente in the cold war. These writers have published prolifically, not only in their own journal,[5] but also in several books on contemporary international affairs.

Revisionist historians of the "New Left" share with members of the protracted conflict school a commitment to scholarly endeavors not only for their own sake but, more importantly, to change the course of American foreign policy. Many of them seek a "usable past" to inform and guide a fundamental restructuring of American society.[6] In a series of books published during the past decade, they have challenged both the conventional explanations of recent diplomatic history and the traditional view of America as a pluralistic society. Revisionist historians are not, of course, a homogeneous group. Sidney B. Fay and A. J. P. Taylor, whose interpretations of World War I and World War II represented serious challenges to the conventional assessments of those events, share little with revisionists of the left. Our focus is largely on those who approach recent history from Marxist or neo-Marxist premises. A much smaller literature produced by a radical faction among "peace researchers" is also considered.

These writers are not unrepresentative of the radical right and left,[7] but the present analysis is far from exhaustive. A more comprehensive study might have included not only other scholars of the radical right, but also other groups of social scientists on the left, such as economists of the *Monthly Review* school. Space limitations also make it necessary to cast the discussion in rather broad outlines. Subtle —and perhaps not-so-subtle—variations within the two schools are no doubt blurred, but it is hoped that any distortion is relatively small.

Some may find that the text is marred by an overabundance of quotations and references. Perhaps in an essay of this type there is some value in permitting the authors under review to speak for themselves on many crucial points. This raises another problem: quotations out of context can be used to mislead as well as to instruct. References permit the reader to examine the original works, and to judge whether the effort to avoid distortion was in fact successful.

Understanding History and Politics

1. *Contemporary international politics can be understood only if one begins with an integrated, scientific theory.*

Radicals of both persuasions share the view that conventional analysts, like the chained prisoners in Plato's "parable of the cave," can only see dim shadows cast by a reality that lies beyond their field of vision. The philosopher kings in this analogy are those armed with an integrated, scientific theory that illuminates the

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should determine for himself whether the basic thesis of my essay would require substantial modification were some of the authors under review dropped from consideration.

This survey has "oversampled" books and undersampled articles, on the premise that the former represent the more considered views of their authors.

[4] Arnold Beichman, *Nine Lies About America* (New York: Library Press, 1972), pp. 77–99.

[5] *Orbis,* published by the Foreign Policy Research Institute, University of Pennsylvania. In recent years, however, *Orbis* has opened its pages to a much more representative group of scholars.

[6] See, for example, Barton J. Bernstein, ed., *Toward a New Past* (New York: Pantheon, 1968). For excellent reviews of the revisionist literature on the cold war, see Henry Pachter, "Revisionist Historians and the Cold War," *Dissent,* 15 (Nov.-Dec. 1968), 505–518; Charles S. Maier, "Revisionism and the Interpretation of Cold War Origins," *Perspectives in American History,* 4 (1970), 313–347; and Robert W. Tucker, *The Radical Left and American Foreign Policy* (Baltimore: Johns Hopkins Press, 1971). Other review articles that came to my attention only after early drafts of this essay were completed include: J. L. Richardson, "Cold War Revisionism: A Critique," *World Politics,* 24 (July, 1972), 579–612; Walter Laqueur, "Rewriting History," *Commentary,* 55 (March, 1973), 53–69; and Robert Stover, "Responsibility for the Cold War—A Case Study in Historical Responsibility," *History and Theory,* 11 (No. 2, 1972), 145–178.

[7] The term "radical" presents serious difficulties as it means different things to different people, but it seems preferable to such alternatives as "extremist," which has rather odious connotations. "Radical" is not used here in a pejorative sense. It does indicate that both groups of writers are intensely critical of American liberalism—both as reflected in scholarship and in the conduct of foreign policy—and seek fundamental rather than incremental changes in American society and diplomacy. It does *not* follow, of course, that all who are dissatisfied with some aspect of the status quo would fall into one of these two groups.

essence of foreign policy and international politics.

By itself the stress on theory is not particularly unique to the radical theorists. Few would argue that we can approach complex social phenomena with a *tabula rasa*. Nor does a sufficient compilation of raw data result in understanding; rarely if ever is it true that "the facts speak for themselves." But analysts of the radical right and left are not merely asserting these truisms. They charge that others have failed to come to grips with the realities of contemporary international politics because their studies and prescriptions proceed from theories that, among other deficiencies, neglect the institutional and ideological bases of war; misinterpret the fundamental nature of the enemy; and, failing to understand the conditions of peace, lack any relevance to contemporary problems of international politics. Thus, where others may find puzzles, paradoxes, anomalies, and inconsistencies, the radical insists that history reveals a coherent pattern if viewed through properly ground theoretical lenses.

Writings from the radical right constitute an endless lament about "the general lack of awareness among Western thinkers and analysts about the present-day Communist theory and practice, and the true nature of Soviet long-range objectives, strategy, and tactics."[8] Conventional thinking fails to see international conflict as part of a coherent pattern. The liberal[9] is especially guilty of looking at each conflict in isolation, rather than as an "organic whole."[10] As a result, when Communist tactics change from one situation to another, he confuses tactical maneuvers with a change in goals. All too often the liberal is beguiled by naive notions such as "Communist liberalization" or "peaceful co-existence."

The key to understanding the Communist phenomenon, according to analysts of the radical right, is to view it as a method "of conflict in space over a sustained period of time"—that is, a method of "protracted conflict."[11] This perspective lays open what all too many Westerners have assumed was a "riddle wrapped in a mystery inside an enigma." "The facts which matter today yield their meaning to the theory of 'protracted conflict.' Without such an informing theory, the crisis of our times is meaningless."[12] The theory of protracted conflict is adduced primarily from the Marxist classics, especially the writings of Lenin. Proper reading of these materials is not a task for the amateur or the dilettante, for much of the writing is in an esoteric and mystical code ("Aesopian language") that only the cognoscenti can decipher.[13] Those who have mastered the code will, however, reap rich rewards. Given an understanding of the true nature of communism, they will not fall prey to the euphoric belief that liberalization is taking place within Communist states, a widely-believed myth (especially among intellectuals) that Communists exploit.[14] They will avoid equating changes in leadership with a shift in fundamental Communist goals, as well as other dangerous errors of interpretation. Informed by the theory of protracted conflict, one will recognize that periods of consolidation or even retrenchment are as much a part of the Communist war plan against the Free World as the periods of aggressive expansion. It will be clear that to Communists "peace" means "war," "peaceful coexistence" means war by other methods, and promises are merely made to be broken whenever convenient. Most important, the protracted-conflict approach enables one to see Communist political, military, psychological, economic and technological efforts as merely different facets of a comprehensive war plan, and to discern the intimate relationship between seemingly disparate events in various parts of the world.

Radicals on the left are equally insistent about the importance of an integrated theory, not only for social analysis, but also for defining a moral basis for action: for "without some coherent analysis and a theory based upon it there are no clearly defined limits to action, no restraints to capitulations or moral failures, and no definite assumptions upon which one bases one's response to an evil society and its conduct."[15] Like their counterparts on the right, they are intensely critical of the more conventional interpretations of international politics and foreign policy. Special targets are students of American foreign policy who take seriously the false illusions of "pluralism," while overlooking the institutional structures from which arise the motivations for policy making in capitalist societies. The liberal analyst, even when criticizing such American policies as the support of South Vietnam, is likely to give some consideration to idiosyncracies of personality, organizational inertia, inadequate information, errors of human judgment, misperception, historical accidents, unanticipated consequences, bureaucratic politics, good intentions gone astray, bad luck, or sound policy applied in inappropriate circumstances. He therefore mistakenly assumes that the "system" can be reformed to produce better decisions.[16]

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[8] William R. Kintner and Joseph Z. Kornfeder, *The New Frontier of War: Political Warfare, Present and Future* (Chicago: Regnery, 1962), p. 161. See also, Robert Strausz-Hupé, William R. Kintner, James E. Dougherty, and Alvin J. Cottrell, *Protracted Conflict* (New York: Harper, 1959), pp. 10–11. In a private communication Professor Kintner indicated that his present views are not necessarily the same as those expressed in earlier publications.

[9] Although liberals are not explicitly charged with treason in this literature, one is left with little doubt that their naiveté and ignorance (or worse) have been instrumental in ensuring Communist successes since 1945. Liberals are linked with pacifists, socialists, and neutralists in a single category. Other authors remind us that liberals "use American power to contrive evanescence of American power," and the "enemy whose power is growing throughout the globe is also in our midst." Strausz-Hupé et al., 19–20; M. Stanton Evans, *The Politics of Surrender* (New York: Devin-Adair, 1966), pp. 12–13; and Kintner and Kornfeder, p. 82.

[10] Strausz-Hupé *et al.*, pp. 109, 118. See also William R. Kintner and Stefan T. Possony, "Strategic Asymmetries," *Orbis*, 9 (Spring, 1965), pp. 40–41.

[11] Strausz-Hupé et al., pp. 7–8.

[12] Strausz-Hupé et al., p. 6.

[13] Kintner and Kornfeder, p. xvi. See also William R. Kintner and Harriet Fast Scott, trans. and eds., *The Nuclear Revolution in Soviet Military Affairs* (Norman: University of Oklahoma Press, 1968), pp. 5, 395.

[14] Strausz-Hupé et al., p. 67; Gerhart Niemeyer, *Deceitful Peace: A New Look at the Soviet Threat* (New Rochelle, N.Y.: Arlington, 1971), p. 133.

The key to understanding such diverse events as the origins of the cold war or the American intervention in Vietnam, according to the radical left, is a coherent theory of capitalist society and of the institutional imperatives that guide its actions. Guided by a proper perspective on the inherent nature of capitalism, relating "power and class, domestic and foreign policy,"[17] the analyst will understand that the domestic failures of capitalism are inexorably linked to imperialism, and that violence against the oppressed in America and in the Third World are part of a web of capitalist exploitation. Injustice and repression are inherent in capitalism, and evil policies are structural and systematic, not accidental and episodic.

In structure the theory of capitalist imperialism is thus the mirror image of the theory of protracted conflict. To make the point in a somewhat different way, radicals of both persuasions are strong advocates of Allison's Model I (the unitary rational actor), and are critical of Allison's Models II and III, which regard foreign policy decisions as importantly affected by "organizational processes" and "bureaucratic politics."[18] Model I advocates assume that policies emerge in unalloyed form from a calculated consideration of goals, strategies and tactics, as well as a close correspondence between motives and actions. Models II and III are rejected because they imply that bureaucratic organizations may come to constitute a semi-autonomous force and that inter- and intrabureaucratic political processes may involve serious debate over "real" alternatives, thereby significantly affecting foreign policy decisions.

Radicals of the right do not deny the existence of factional disputes within the Kremlin or in Peking, but they argue that differences of viewpoint are circumscribed within the narrow confines of Marxist-Leninist orthodoxy—that is, the strategy of protracted conflict against the Free World. Models II and III are irrelevant to understanding Soviet or Chinese policies because there is no deviation from basic tenets of Communist goals and strategy. In a similar vein those on the left insist that the limits within which even top ranking bureaucrats and military personnel consider policy options are strictly set by the ruling class and the interests of corporate capitalism they represent.[19] Events that may appear to others as accidental or random, when viewed through the prism of the appropriate theory, emerge as instances of planned, rational action. Policies of the enemy are especially subject to comprehensive explanation. The keys to the motivations and calculations are to be found in theories that link the enemy's institutions and ideology to foreign-policy decisions. In the conclusion we will have another look at these theories to consider whether, as employed by their advocates, they satisfy a basic requirement of any explanation—falsifiability.

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[15] Gabriel Kolko, *The Roots of American Foreign Policy: An Analysis of Power and Purpose* (Boston: Beacon, 1969), p. 135.

"If the United States' foreign policy is set in its historical framework and analyzed in terms of the Marxist theory of imperialism, however, these developments hardly appear obscure and unintelligible, or even of recent origin." David Horowitz, *Empire and Revolution* (New York: Random House, 1969), p. 54.

[16] "The notion of American leadership's errors or misperceptions is reassuring to those who believe the society can be redeemed by abler, superior men. But that species of liberal theory ignores the reasons for the constancy [of American policy] and also the justifications and explanations for action . . ." Joyce and Gabriel Kolko, *The Limits of Power: The World and United States Foreign Policy, 1945–1954* (New York: Harper & Row 1972), p. 7. This point receives a somewhat different emphasis from Gar Alperovitz. "Men make mistakes in deciding issues—but the 'system' sets the terms of the issues they decide; and, as we shall ultimately have to conclude, it is the 'system' and its traditions which must therefore be challenged." *Cold War Essays* (Garden City, New York: Anchor, 1970), p. 91.

[17] Kolko, p. xvii. See also Kolko and Kolko, pp. 7, 19.

[18] These models are described in Graham Allison, *Essence of Decision: Explaining the Cuban Missile Crisis* (Boston: Little Brown, 1971). Many nonradical scholars also prefer Model I to Models II and III.

[19] "A society's goals, in the last analysis, reflect its objective needs—economic, strategic, and political—in the light of the requirements of its very specific structure of power. Since this power structure in America has existed over many decades in a capitalist form, its demands are the common premises for the application of American power—one that theorists attribute to social consensus and sanction, but which in reality has always reflected the class structure and class needs. With time, such structural imperatives and limits appear to take on independent characteristics, so that whether academics or businessmen administer it, the state invariably responds in nearly identical ways to

2. *Nothing important happens by chance.*

A central element in the cosmology of both the radical right and left is the belief that important events do not "just happen." Chance is rejected as a valid explanation. This view is not unique, of course; most scientific enterprises are biased against chance as an explanation. But whereas many social scientists consider patterns and regularities in probabilistic terms, radical theorists tend to view events in a deterministic fashion, and to assume that a few independent variables can account for all of the variance in important matters of international politics. This point is, of course, related to the belief that there exists a comprehensive theory that will reveal the forces and motivations underlying each event.

High on the list of sins attributed to liberals by the radical right is the liberals' presumed belief that "The communization of half the world over the past 20 years is nobody's fault in particular."[20] The origin of the cold war, its expansion, and the resulting arms race, as well as specific crises and conflicts, cannot be attributed to misunderstandings, miscalculations of vital interests, the unintended escalation of minor events, or the rivalries that have so frequently marked relations among great powers. They are, on the contrary, all manifestations of a sophisticated Communist master plan of political-military-psychological-economic-technological warfare against the Free World. The cold war, in short, is neither more nor less than an international offensive initiated by the Communists and aimed at total world domination.

Nor are conflicts in former colonial areas merely the results of clashing local interests. Like his counterpart on the left, the radical rightwing analyst finds intimate links between seemingly disparate events in widely separated parts of the world: "Our opponents have a larger design into which the Vietnam conflict fits."[21] An assessment of events in 1965, for instance, led Possony to the conclusion that the war in Vietnam, the attempted coup in Indonesia, and disorders in Bolivia, Colombia, Ecuador, and Guatemala were all part of a plan by Mao Tse-tung and Ho Chi Minh for a "tri-continental war" against the Free World.[22]

The radical left also insists that all important events can be traced to ultimate causes. Liberal analysts are charged with intellectual myopia for suggesting that American foreign policy reflects anything other than the structure of American institutions—specifically, the "expansive interests of American capitalism."[23] There is little patience with propositions that Washington may sometimes be responding to rather than initiating international undertakings, or that its policy-making processes may sometimes best be described as "muddling through." Indictments by Chomsky and Kolko reflect the widespread unwillingness on the left to contemplate such explanations. "It is conceivable that American actions in Vietnam are simply a single outburst of criminal insanity, of no general or long-range significance. . . It is difficult, however, to put much credence in this possibility."[24] "The logical, deliberative aspects of American power at home and its interests abroad show how fully irrelevant are notions of accident and innocence in explaining the diverse applications of American power today, not only in Vietnam but throughout the Third World."[25]

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similar challenges. Apart from the fact that no bureaucrat, however chosen, could rise to a position of responsibility without continuous and proven conformity to norms of conduct and goals defined by the society's economic power structure which is, in the last analysis, the source of national goals, there is little intrinsic significance in the nature of the bureaucratic selection process and administrative elite." Kolko and Kolko, p. 19.

[20] Evans, p. 17. See also pp. 521–522.

[21] Franz Michael, "The Stakes in Vietnam," *Orbis*, 12 (Spring, 1968), 129.

[22] Stefan T. Possony, "Mao's Strategic Initiative of 1965 and the U.S. Response," *Orbis*, 11 (Spring, 1967), 159.

[23] Kolko and Kolko, p. 8. "That [American] policy and practice has been deliberative and quite consistent because the goals, structure, and requirements of the United States' social system have remained durable throughout the period" (Kolko and Kolko, p. 7).

[24] Noam Chomsky, *American Power and the New Mandarins* (New York: Pantheon, 1967), p. 311. Radical left analysts concentrate their fire less on the fully committed supporters of American policy in Vietnam—for example, the radical right theorists whose views are examined in this essay—than on the liberal critics of that policy, such as Arthur Schlesinger and Ronald Steel. See, for example, Chomsky, pp. 207, 220, and 298.

[25] Kolko, p. xi-xii. See also Kolko, *The Politics of War: The World and United States Foreign Policy, 1943–1945* (New York: Random House, 1968), p. 625; and Horowitz, p. 71.

The radical left critique is not limited to Vietnam policy, or even to the period since 1945. Some critics view American diplomatic history as a record lacking consistency: periods of deep isolationism are followed by spasms of intense international involvement. Radical left historians reject this interpretation. They tend to regard America as a nation born with imperialist ambitions that, rather than abating, have expanded linearly—if not geometrically—to the present.[26] This school is thus highly critical of analyses that attribute the expansion of American influence since 1945 to a complex interaction between a genuine threat from the U.S.S.R., American reluctance, and humanitarian ideals on the one hand, and misplaced sentimentality, bureaucratic stupidity, and serious errors of intellect on the other. On the contrary, asserts the analyst of the radical left, the growth of the American empire arose from a grand design of the international environment necessary to preserve capitalism at home, and of rationally calculated policies aimed at achieving that end.

Although radicals attribute widespread use of conspiratorial methods to their adversaries, neither of the theories is, strictly speaking, a "conspiracy theory of history." The radical right argues that the Communist nations are merely pursuing policies that derive naturally and inescapably from the nature of Communist totalitarianism. Similarly, the radical left analyst argues that in pursuing a policy of violence, repression, and imperialist expansion, capitalist America is simply doing what comes naturally. "Imperialism is not a matter of choice for a capitalist society; it is the way of life of such a society."[27]

3. *Contemporary international politics is basically a moral conflict of the forces of freedom and justice versus those of aggression and repression.*

The radical right and left share a Manichean view of history and politics. They agree that contemporary international conflicts are not merely the result of irreconcilable demands for territory or spheres of influence, clashing national interests, or historic rivalries. Soviet-American relations cannot be properly understood as just another conflict between two great powers. These nations are the most powerful representatives of diametrically opposed social and ethical systems. Special emphasis is usually placed on the moral difference between opposing forces, but heroes and villains are rarely defined by their individual attributes. The left and right tend to agree that the personal qualities of Stalin, Truman, Acheson, Molotov, and others are less important than their roles as representatives of certain institutions, classes, or movements. Radical right analysts almost invariably describe the postwar era as a life-or-death struggle between Western civilization—representing law, democracy, freedom, and peace—and communism, a system of violence, repression, and imperialism.[28] The fate of civilization depends on the destruction of communism.[29] To those on the radical left, the conflict pits the irredeemable evils of capitalism, and its by-products of imperialism, repression, and war, against revolutionary movements of social transformation, whose triumph will ensure an era of peace and justice.

The Causes of War

4. *The causes of conflict and war can be traced to the nature of certain polities.*

4a. (Radical right version). *The cause of contemporary international conflict is the inherent aggressiveness of international communism.*[30]

4b. (Radical left version). *The cause of contemporary international conflict is the inherent aggressiveness of capitalist imperialism.*[31]

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[26] "Indeed, even if the Soviet Union had not existed, the condition of the Third World and America's response toward it after 1945 would scarcely have been different—for Washington's goals predated the war and even 1917 itself." Kolko and Kolko, p. 714. See also, William Appleman Williams, *The Contours of American History* (Cleveland: World, 1961); Carl Oglesby and Richard Shaull, *Containment and Change* (New York: Macmillan, 1967); and Sidney Lens, *The Forging of the American Empire* (New York: Crowell, 1971).

[27] Harry Magdoff, *The Age of Imperialism: The Economics of U.S. Foreign Policy* (New York: Monthly Review, 1969), p. 26.

[28] Kintner and Kornfeder, p. 295.

[29] Arleigh Burke, "Power and Peace," *Orbis,* 6 (Summer, 1962), 198. See also, Robert Strausz-Hupé, William R. Kintner, and Stefan T. Possony, *A Forward Strategy for America* (New York: Harper, 1961), p. 265.

[30] "The 'cold war,' which is really pure political warfare, results from the expansionist techniques which the USSR initiated and with which the United States has been more and more compelled to cope." Kintner and Kornfeder, p. 1.

"So long as [Communist] totalitarianism survives, so long the danger of total war will persist. It is as simple as that." Stefan T. Possony, "The Challenge of Soviet Totalitarianism" *Orbis,* 8 (Winter, 1965), 788.

[31] "The contemporary world crisis, in brief, is a by-product of United States' response to Third World change and its own definitions of what it must do to

Kenneth Waltz has shown that political theorists generally attribute the existence of war to one of three causes: the nature of man, the state, or the international system.[32] Analysts of the radical right and left are rarely plagued by doubts about the causes of contemporary international conflict. Although they disagree on the identity of the source, the structure of their analyses is identical. The etiology of conflict and war is in the nature of the state, or more precisely, in the malignant institutions of certain states. For reasons that we shall explore later, they overlook or deny the possibility that either attributes of individuals—for example, limits on rationality, overweening ambition, pride, stubbornness, and the like—or of the international system may shape or constrain policies in ways that make periodic clashes among nations probable.

The radical right traces contemporary international conflict to a protracted, multifaceted Communist assault on the Free World. Soviet communism is a system based on violence. Whatever humanitarian instincts and values may have motivated Karl Marx have long since been discarded in the policies of the Soviet ruling elite. Because Lenin, Stalin, and their successors have proved neither willing nor capable of carrying out any genuine reforms, totalitarian violence and repression have been the instruments for maintaining power over a discontented Russian people. Having been institutionalized and bureaucratized, totalitarianism no longer depends on the whims of a dictator; this explains its survival beyond the death of Stalin. Nor is its hostility toward the West subject to change, for it is inherent in the Soviet system.[33]

The messianic drive to international expansion and destruction of all non-Communist systems is an inescapable part of Communist theory and practice. "Conflict to the bitter end is the stuff from which communism draws its very sustenance."[34] Moreover, abysmal domestic failures of Soviet and Chinese communism require their leaders to seek compensating victories in the international arena. It follows, according to the radical right line of reasoning, that if and when the Free World comes to understand the inherent nature of communism, and adopts a "forward strategy" aimed at victory rather than containment or coexistence, the fundamental defects of the Communist system will become manifest. The ultimate defeat of communism will usher in an era of international harmony. In the words of one writer, the world will then move from a period of conflict management to one of conflict resolution.[35]

The radical left explanation of contemporary international conflict is structurally similar but, of course, the role of the villain is assumed by different actors and institutions. War and conflict are rooted in the structure and ideology of capitalism and its most powerful contemporary representative, the United States.[36] Capitalism is inherently violent, but even internal repression is not sufficient to resolve its fundamental contradictions and inability to provide domestic justice and prosperity. Hence the inescapable drive for empire. External expansion and exploitation of peoples abroad in a never-ending search for markets and raw materials make it possible for a time to conceal the weakness of capitalism behind a precarious facade of prosperity at home. A capitalist state also tries to repress all movements of the revolutionary left. Examples of progress and development in socialist nations are a threat because they demonstrate that there is a just alternative to the ethical and practical failures of capitalism. As we shall see later, the corollary to the theory of capitalist imperialism is the premise that socialist states are inherently peaceful.

In summary, then, the radical right and left share the beliefs that the causes of war are to be found in the existence of certain malignant states, and that the condition of peace is the adoption by all states of preferred institutions and ideologies.

At the same time advocates of both theories agree that neither flaws in man's nature nor the imperatives of an international system com-

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preserve and expand its vital national interests." Kolko, *Roots*, p. 85. See also Kolko and Kolko, pp. 7, 714.

The cold war is "a calculated response to the *global* crisis in the capitalist system itself." Horowitz, p. 71. Emphasis in the original.

[32] Kenneth Waltz, *Man, The State, and War* (New York: Columbia University Press, 1959).

[33] Kintner and Kornfeder, p. 82.

[34] Strausz-Hupé et al., *Protracted Conflict*, p. 19.

[35] Robert D. Crane, "The Cuban Crisis: A Strategic Analysis of American and Soviet Policy," *Orbis*, 6 (Winter, 1963), 549.

[36] "It is only in this context [of global revolution] that Soviet-American relations should be placed, and it is here that the limits of the notion of the Cold War become most apparent. For that static concept conditions us not to probe further the real character of the forces of intervention and expansion—and therefore violence—in our times. It minimizes the nature and causes of mankind's fate today, leading us to believe that conflict and violence are accidental rather than inevitable consequences of the objectives of American foreign policy and the imperatives it has imposed on movements of social transformation throughout the world." Kolko and Kolko, pp. 6–7.

Oglesby and Schaull, p. 72, define imperialism as free enterprise applied to international politics.

posed of sovereign states are a major factor in explaining war. Accepting either of these arguments would tend to cast doubt on the absolute moral superiority of one side in contemporary international conflicts. A tragic view of man's condition is difficult to reconcile with what Kenneth Boulding has called the "cowboy theory" of international conflict[37]—that it is essentially the good guys versus the bad guys. At the least it alerts the analyst to the possibility that war is too complex a matter to be disposed of by assigning all guilt to one party, no matter how morally or emotionally satisfying that may be. Those on the right repeatedly attack the "liberal" notion that one might look dispassionately at international politics without constantly making moral and other distinctions between the Communists and the Free World. We are told that the Communists are "different," not only ideologically, but in virtually every important sense.[38] As one writer puts it, communism is a "disease of the heart and intellect."[39]

A mirror image of this view can be found on the radical left. To suggest that both parties to a conflict might share some responsibility for it is simply inconceivable. A principal at the Stockholm "war crime" hearings was asked why no one was invited to present the South Vietnamese or American case. The question was dismissed as absurd because, by definition, the South Vietnamese and Americans were no less guilty than Hitler was in attacking the Warsaw Ghetto.

Similarly, if one assigns some importance to systemic considerations—for example, Rousseau's thesis that in a system of sovereign states, pursuit of even limited security will result in periodic wars—one may become a bit more sympathetic to the genuine security dilemmas that few states, and none of the great powers, have totally escaped. But as we shall see later, in the view of the radical right, only Free World nations not Communist ones, have security problems. It follows that Western arms serve the cause of peace, whereas those acquired by Communist nations threaten it. Radical left analysts similarly deny that the United States and other capitalist nations have had any genuine reasons for concern about security. "The bogey of Soviet imperialism" was merely a manufactured pretext for expanding the American empire.[40] On the other hand, genuine and valid Soviet concerns about *its* security are sufficient to explain and perhaps excuse Soviet policies in Czechoslovakia, East Germany, and elsewhere. And whereas Western professions of peaceful intent are consistently dismissed, Soviet assertions are apparently taken at face value. For example, the 1949 "peace offensive" is described as having been sufficiently genuine to force Washington "to conjure up new alleged Soviet threats."[41]

On one point the two theories differ in emphasis. The radical right tends to attribute primary causation to ideology. Marxist-Leninist ideology gives rise to institutions which, like the messianic doctrine that spawned them, are inherently expansive and violent. The radical left usually places ideology and institutions into a somewhat different relationship. At the root is capitalism, from which derive other institutions, including a characteristic class structure. Political structures and processes reflect faithfully the needs of the economic system, both in domestic and foreign policy. A liberal ideology then emerges as the justification for capitalist institutions and policies.

This point aside, the radical analyses are virtually identical in structure. They agree that we must look to the attributes of certain nations as the prime source of contemporary international conflict, and that once these cancers are eliminated mankind will enter into an unprecedented era of peace.

5. *All international conflicts are linked.*

Analysts of the radical right and left further share the belief that war, conflict, and injustice, wherever they may occur, can ultimately be traced to a single source. Those on the right find the source in international communism, whereas those on the left point to capitalist imperialism.

Radical right analysts view Soviet-American relations as only the most visible contemporary expression of "pervasive conflict that encompasses all lands, all peoples, and all levels of society."[42] Civil as well as international conflicts are part of the same gigantic struggle. Thus, there is no difficulty in linking the war in Indo-China, racial conflict in Africa, or civil disorders in many nations of Latin America as components of an integrated offensive by the forces of communism against the Free World.[43]

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[37] Kenneth Boulding, William O. Douglas, Harry V. Jaffa, Clinton Rossiter, William V. Shannon, and Harvey Wheeler, *The U.S. and Revolution* (Santa Barbara: Center for the Study of Democratic Institutions, 1961), p. 5.

[38] Evans, p. 16; Strausz-Hupé et al., *Protracted Conflict*, p. 143; and William R. Kintner, *Peace and the Strategy Conflict* (New York: Praeger Publishers, 1967), p. 11.

[39] Evans, p. 133; Anthony Trawick Bouscaren, *Soviet Foreign Policy: A Pattern of Persistence* (New York: Fordham University Press, 1962), p. 4; cf. Niemeyer, p. 39. For a counterpart of this argument on the left, see Theodore R. Roszak, "The Disease Called Politics," *Seeds of Liberation*, ed. Paul Goodman (New York: Brazilier, 1964).

[40] Philip Resnick, "Canadian Defense Policy and the American Empire," *Critical Issues in Canadian Society*, eds. Craig L. Boydell, Carl F. Grindstaff, and Paul C. Whitehead, (Toronto: Holt, 1971), p. 379. Resnick, p. 388, establishes his credentials as a student of imperialism with his observation that Canada's role in "the American empire" is equal to that of Czechoslovakia vis-à-vis the U.S.S.R.

[41] Kolko and Kolko, pp. 398, 703, and 503.

Radicals on the left can similarly find direct causal linkages between the structure of American capitalism and such diverse events as the pace of modernization in Peru, Brazil, Ecuador and the Dominican Republic;[44] the Arab-Israeli conflict; the Indo-Pakistani War of 1971; or even tension between the French and English communities in Canada.[45] As Kolko puts it, the United States has transformed "the American crisis into a global one."[46] The insistence that any international conflict or injustice can usually be traced to American imperialism is not without irony, for it is *de rigueur* among many on the left to assert that the origins and expansion of the cold war can be attributed in part to Washington's proclivity for linking all revolutionary forces to Moscow and a phantom international Communist conspiracy. Others argue that American officials have been aware all along that revolutionary movements of the left are really nationalist in character, but that they have deliberately depicted them as Soviet puppets in order to mobilize support for anti-revolutionary policies.[47]

6. *Domestic and foreign policy are two sides of the same coin.*

Theorists of both the radical right and left view domestic and foreign policy as a seamless web, a position that is closely related to, and quite consistent with, the view that the causes of war are to be found in the character of certain states. This thesis can be seen most clearly in their analyses of the enemy and in their prescriptive statements.

Those on the right view Soviet policy not only as a reflection of the inherent aggressiveness of Communist states, but also as a result of the abysmal failure to fulfill the domestic promises of the Marxist utopia.[48] External adventures have provided the Soviet ruling class with sufficient success—aided in no small measure by Western defeatism and its sabotage of such anti-Communist governments as that of Nationalist China—to "satisfy national pride,"[49] and thereby avert serious internal upheavals. Were the West to abandon its defeatist policies in favor of a "forward strategy," not only inhibiting further Soviet expansion but carrying the cold war into the Communist zone, Soviet domestic failures could no longer be hidden.

Radical left analyses of American foreign policy are guided by a similar line of reasoning. American imperialism is viewed as a necessary and inseparable product of its capitalist institutions. Three basic motivations, all linked to domestic necessities, are seen as the driving force behind American foreign policy. An insatiable search for markets and cheap raw materials inevitably results in exploitation of large parts of the world, especially within the Third World.[50] The putative failures of capitalism at home provide a supplementary motive for imperialist expansion. Washington's aggressive policies triggered the cold war, not in response to any genuine external threat, but as a means to buy a "temporary respite to the system's internal economic problems of poverty, automation, and the like."[51] Equally important, the imperatives of capitalism have given rise to a messianic antirevolutionary ideology that has outlived its economic progenitors.[52] This is but one reason for America's antagonism to revolutionary forces. The other is that movements of the left, although not a "real" challenge to American security, offer an alternative to the contradictions and failures of a capitalist society. Just as the radical right believes that Soviet totalitarianism cannot long survive at home if it is thwarted abroad, so the radical left argues that once American imperialism suffers external defeat, rot in the foundations of the capitalist society will be exposed. Defeat for America signifies not only the triumph of progressive forces abroad, but is also a necessary condition for a revolutionary restructuring of American society into a socialist (i.e., peaceful) state.

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[42] Strausz-Hupé et al., *Protracted Conflict,* p. 10.

[43] Possony, "Mao's Strategic Initiative," p. 171.

[44] Fred Goff and Michael Locker, "The Violence of Domination: U.S. Power and the Dominican Republic," in *Latin American Radicalism,* ed. Irving L. Horowitz, Josué de Castro, and John Gerassi (New York: Random House, 1969). For a useful corrective to this article, see Jerome Slater, *Intervention and Negotiation: The United States and the Dominican Revolution* (New York: Harper & Row, 1970).

[45] Aijaz Ahmad, "The United States Responsibility in Bengal," *Canadian Dimension,* 8 (Nov. 1971), 17 ff. See also the Canadian left publication, *Our Generation.*

[46] Kolko, *Roots,* p. 49. See also Kolko and Kolko, p. 7.

[47] See, Kolko, *Politics of War,* p. 446; David Horowitz, *The Free World Colossus* (New York: Hill & Wang, 1965); Alperovitz; Resnick; and Kolko and Kolko, p. 332.

[48] The protracted conflict school is somewhat less than consistent on this point. It is argued, on the one hand, that the more internal stability Communist societies achieve, the more aggressive their foreign policies. On the other hand, troubles within Communist nations are supposed to spur them into seeking diversionary adventures abroad. Compare Robert Strausz-Hupé, "The Sino-Soviet Tangle and U.S. Policy," *Orbis,* 6 (Spring, 1962), 37; and Stefan T. Possony, "The Chinese Communist Cauldron," *Orbis,* 13 (Fall, 1969), 788.

[49] Kintner and Kornfeder, p. 318. American complicity in the defeat of the Nationalist government in China is a standard part of radical right recounting of history. OSS assistance to Mao during World War II, the critical role of the American Communist Party, the machinations of Harry Dexter White and his Communist colleagues in the Treasury Department, and the UNRRA program are described as "indispensable" to Mao's victory. Possony, "The Chinese Communist Cauldron," p. 817; Possony, *A Century of Conflict: Communist Techniques of World Revolution* (Chicago: Regnery, 1953), p. 317; and Kintner and Kornfeder, p. 79. One wonders what role if any, Mao played in the defeat of the Nationalists.

Needless to say, the radical left interpretation of the same events is that America was guilty of intervention in support of the Nationalists. Indeed, Kolko and Kolko assert, p. 549, that American backing of the Kuomintang government was "the major cause of the prolonged civil war." See also Kolko, *Politics,* pp. 611–617.

[50] "[R]aw materials guided every aspect of American policy toward the Third World, and the keystone of

The Nature of the Enemy

Beliefs about the "enemy" are at the core of radical theories of international relations and foreign policy. To the radical right the opponent is Communist totalitarianism and those nations that form part of the Communist empire, especially China and the U.S.S.R. On the left the adversary is capitalist imperialism, the most powerful contemporary manifestation of which is the United States. Although the two schools differ on the identity of the enemy, they do share a number of important beliefs about its nature.

7. *The driving force behind the enemy's quest of empire stems from institutional imperatives.*

8. *Institutionalized violence is inherent in the enemy's sociopolitical system.*

9. *The enemy's sociopolitical system is a static structure; whatever changes occur (for exple in personnel within the ruling elite) take place within such narrowly circumscribed limits as to have no substantial effect on foreign policy.*[53]

10. *The enemy's foreign policy reflects only the interests of the ruling class, and runs diametrically counter to genuine interests of the population.*

These points have already been discussed in some detail in other contexts and we shall not dwell further on them.

11. *In making policy decisions, the enemy is free from external constraints.*

12. *In making policy decisions, the enemy is constrained only by the needs of the ruling class, its institutions and ideology, and is free from external constraints.*

Analysts of both the radical left and right assume that policy makers in the enemy nation are free from any external and democratic internal constraints that might shape the main contours of foreign policies and restrict policy choices on critical issues. Within limits established by the needs of the ruling class, their options are virtually unlimited. They behave as they do—pursue policies of aggression and repression—by deliberate and calculated choice, not in response to international pressures or domestic constraints that lie beyond their control. This belief has important implications for the remainder of their analyses, being intimately related to some of the others we touched upon earlier: for example, the belief that nothing important happens by accident, and that every foreign policy undertaking is part of a carefully developed master plan.

According to the radical right interpretation of international politics Soviet leaders are free to pursue the expansive goals set out in Marxist-Leninist doctrine, whereas statesmen in the Free World democracies—especially the United States—have continually been restricted and hampered (if not subverted) by the demands of domestic politics. Executive freedom has been inhibited by inadequate public understanding and opposition critics "who do not agree with the country's goals."[54] As a result

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the Point Four program from 1950 onward was the accelerated expansion of world raw materials supplies and sources." Kolko, *Roots*, p. 64. See also, Magdoff; and Kolko and Kolko, p. 8.

For a partial corrective to these arguments, consult Tucker, pp. 121–131; and Kenneth N. Waltz, "The Myth of National Interdependence," *The International Corporation*, ed. Charles P. Kindleberger (Cambridge: M.I.T. Press), 1970.

[51] Kolko, *Roots*, p. xvii. See also, Richard J. Barnet, *Roots of War* (New York: Atheneum, 1972), p. 337, and Kolko and Kolko, p. 5.

[52] Christopher Lasch, "Introduction" to Alperovitz, p. 15.

[53] Kolko, *Roots*, p. 9. Alperovitz, p. 120, writes of the "situational logic" created by American institutions that "traps Presidents, whatever their personal views, repeatedly into antirevolutionary decisions." See also, Kolko and Kolko, p. 19.

[54] American Security Council, *USSR vs. USA: The ABM and the Changed Strategic Military Balance* (Washington: Acropolis, 1969), p. 30.

the executive must face "the monolithic challenges of the Communists with one or both hands tied behind its back."[55] In contrast, Communist rulers, masters of a totalitarian political system, encounter no internal dissent. Whatever differences exist among Soviet elite groups center on relatively minor questions of tactics and timing, rather than on the basic policy of waging a protracted war against the Free World. The contrast between the political imperatives that constrain Western leaders and the freedom of Communist leaders emerges clearly in an evaluation of summit meetings. Because "Soviet negotiators are free agents; their Western counterparts are not," thus summit conferences are a dangerous ruse. Western leaders, who must pander to the "ill-founded" hopes of the masses, are under intense pressures to seek accommodation, whereas Soviet leaders are free from such concerns.[56]

Radical right analysts also argue that the only international pressures and constraints faced by the Communists are those of their own making; for example, the sullen and discontented peoples in the Eastern European sector of the Soviet Empire. There are no legitimate concerns for security that might impel Russia, China, or other Communist nations to seek allies, undertake vast arms expenditures, and the like. Indeed, the Free World has been disgracefully lax in meeting the multifaceted challenge of communism. For example, it is claimed that the United States has never even joined the race for strategic weapon systems.[57] Soviet foreign policy is rooted in the nature of Communist totalitarianism, not in the external challenges with which most nations must cope. Whatever activities the Kremlin leadership undertakes can therefore be understood only as an integral part of an implacable drive to world domination.

Theories of American policy making espoused by radicals of the left are structurally similar. The limits within which policymakers operate are established by the ruling class and the imperatives of capitalism. Within these boundaries, however, neither internal nor external pressures constrain American foreign policy options. According to this view the domestic political process is the facade rather than the substance of democracy. Genuine pluralism and freedom await the emergence of class opposition to constituted power by an anticapitalist mass movement.[58] Evidence of diversity in the social or educational background of bureaucrats and politicians is dismissed as irrelevant, for the combination of institutional imperatives and ideological consensus transcends the processes and outcomes of political recruitment: "What is ultimately of greatest significance is that whether leadership comes from Exeter-Harvard or Kansas, the results have been the same."[59] Should any member of the ruling class stray beyond the limits established by the requirements of capitalism, he would quickly be purged.

The radical left view of public opinion and its role in American politics centers on the apathy and manipulability of the masses. Even though there is no "genuine" (i.e. radical)

Radicals of the right repeatedly profess a devotion to democratic institutions, but at the same time they propose: limiting criticism of the executive (even with respect to the Watergate scandal, according to Possony), greater responsibility for military experts, creation of "cold war academies," vastly greater arms spending, and the development of sufficient "political maturity" to stop playing into Communist hands. These steps would seem to represent a reasonably important leap toward a garrison state, if not 1984.

[55] Strausz-Hupé et al., *Protracted Conflict*, p. 123.

[56] *Ibid.*, p. 103. As evidence on this point the authors cite an interview with Strausz-Hupé published in *U.S. News and World Report*, April 11, 1958. Writings of the protracted conflict school abound with citations to earlier assertions by the same authors. One has the illusion of documented evidence, but often the citations merely lead to an undocumented assertion which, presumably, gains the status of evidence with the passage of time. Confusing citations may also be found in the works of Gabriel Kolko.

[57] Kintner, p. 7. Explanations of the arms race by those on the left are scarcely more convincing. One theory is that sometime in 1951 or 1952 Washington triggered the arms race in order to "overload" the Soviet economy, to "delay" its recovery from World War II, and ultimately to force its collapse. Horowitz, *Empire and Revolution*, p. 78. Kolko and Kolko, pp. 508–509, assert that Washington started the arms race with the NSC-68 study in 1950.

[58] Kolko, *Roots*, p. 10. New Left historians generally take a harsh view of American reform movements, not excluding the muckraker and "Progressive" periods or the New Deal. Some of the literature is effectively summarized in Irwin Unger, "The 'New Left' and American History: Some Recent Trends in United States Historiography," *American Historical Review*, 72 (July, 1967), 1237–1263.

[59] Kolko, *Roots*, p. 7. This argument can, of course, sustain more than a single interpretation. It is not incompatible, for example, with the view that the pressures of Soviet imperialism have left American leaders with few options, and that any reasonable person would have resisted the more overt signs of Soviet hostility toward the West. Indeed, radicals of the right would accept Kolko's thesis, arguing that irrespective of who has formed the government, leaders in Washington have followed the same policy of appeasing Moscow. The latter interpretation is quite similar to that of Evans.

Given his premise that social background is irrelevant, it is odd that the only systematic evidence in Kolko's work consists of data on the background of American foreign policy elites, showing that they have been recruited from a fairly narrow stratum of society. *Roots*, p. 18.

opposition in America, public opinion is fickle and has often been less than enthusiastic about the internationalist ideology and policies of the ruling class. In these cases leaders have been able to manipulate events (e.g. by creating or exacerbating international crises) in a manner that undercuts any potential opposition. Moreover, because the public is usually apathetic, the facade of a free press and freedom of dissent is maintained. "For this reason more rigid and totalitarian means of handling mass opposition to ruling policy was uncalled for and liberal rhetoric was possible in a situation where a society neither endorsed nor opposed the action of leaders. In a society without serious opposition, manipulation replaces the knout."[60] In case of conflict, however, public disapproval would not deter the ruling elite from pursuing policies that serve its class interests. In short, neither public opinion nor the exigencies of democratic politics constrain the ruling class.

The radical left analysis is not always consistent, however. For example, the "entirely monolithic"[61] character of American leadership is often asserted, even if rarely demonstrated. Yet time and again the same authors claim that the Truman administration embarked upon the cold war and sustained an atmosphere of hysterical anticommunism in order to bludgeon an unresponsive Congress into supporting and paying for expansionary American policies. For this reason, international crises were a godsend to Truman and Acheson: "Had the Korean crisis not occurred, it would have been necessary to manufacture an equivalent or to accept a major defeat for the White House's efforts to reassert American interests in a world slipping even further from beyond its grasp."[62] The recalcitrance of congressmen and senators, whose devotion to capitalism presumably matches that of presidents and secretaries of state, remains unexplained.

Nor, according to the radical left view, can we look to the imperatives of international politics to explain American foreign policy. This point emerges most clearly in the standard left explanation of the cold war and its origins. The argument, in brief, is that long before the collapse of the axis powers, American leaders came to fear the replacement of the "old capitalist order" in Europe and elsewhere by forces from the left. These posed no challenge to American security, but by presenting a viable alternative social system, they did threaten Washington's design to make the world safe for capitalism. Overwhelming American military power in 1945 ensured that no one, certainly not an exhausted Russia, threatened any "legitimate" definition of American security.[63] On the other hand, Russian security needs are deemed quite genuine and legitimate, especially given the U.S.S.R.'s exhausted condition in 1945. Moreover, according to the radical left version of these events, Soviet leaders defined their requirements in a remarkably conservative fashion.[64] The "circumspect" men in the Kremlin not only failed to take advantage of the power vacuum that the failures of capitalism had left throughout the world, they also imposed tight controls over Communist parties in Western Europe to prevent genuine revolutions of the left, even when doing so permitted the conservative propertied classes to regain their dominant position. In part to signal his cooperative postwar intentions, Stalin saved capitalism in Western Europe. Stalin is thus accused by the left not of belligerency, but of naiveté in believing that there were effective constraints on American imperialist expansion. This version of postwar history goes on to argue that America responded to Soviet exhaustion and conservatism with bellicosity. The minimal Soviet definition of its security needs and Stalin's restraint of Communist parties in Western Europe were met with military and diplomatic threats, born of a traditional opposition to forces of the left, and buttressed by the American nuclear monopoly of 1945–1949. The origins and development of the cold war, then, are not to be found in the expansion of Soviet influence and a genuine threat to Western security; nor in a complex combination of mutual suspicions, incompatible goals, and arms races feeding upon and fueling each other: nor in the historically common falling out of wartime allies following collapse of a defeated foe. Rather, the answer is to be found in Washington's determination to make the world safe for American capitalism, and doing so by opposing the political left wherever in the world it might appear.[65]

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[60] Kolko and Kolko, p. 333. See also, Kolko, *Roots*, pp. 15, 12. W. A. Williams, *The Contours of American History* (Cleveland: World Publishing Co., 1961), provides an alternative explanation—that all strata of American society have supported imperialism because they believe that all benefit from it.

[61] Kolko and Kolko, pp. 50–51. This quotation refers specifically to the Truman Administration in 1946!

[62] *Ibid.*, p. 651.

[63] *Ibid.*, pp. 4, 31, 56, 477–478. The radical right agrees with this diagnosis of relative capabilities, but uses it to argue that American "waffling" served to invite Communist advances during the immediate postwar period. See, for example, Evans, pp. 522–523.

[64] "Ultimately, the United States' greatest protection in Europe was the Soviet Union's intentions and capabilities, which post-war history had repeatedly proved were both quite modest." Kolko and Kolko, p. 670; see also pp. 4, 55, 58, 330, 358, 482, and 694; and Kolko, *Politics*, p. 450.

The linchpin of this explanation of the cold war is America's military superiority in 1945. This is alleged to have given Washington a virtually free hand in charting the course of postwar developments, and to have rendered Stalin the hapless object of American power diplomacy, giving him no alternative but to follow Washington's bellicose lead.[66] Soviet behavior, however repressive, is thus explained by a lack of options. This remarkable solicitude for Stalin's position is not matched by a comparable sensitivity to the hypothesis that leaders in London and Washington may also have perceived genuine security problems. This point aside, an anomaly exists in the radical left accounts of the postwar years. If American military power and Soviet exhaustion were so important, and if Americans were not only villains but purposeful villains, then why didn't Washington simply undertake a pre-emptive attack on the Soviet Union while it enjoyed a nuclear monopoly, or at least attempt to roll back the Soviet empire to its 1939 frontiers? Either the U.S.S.R. was powerful enough to deter any attack, or the Americans refused to play the international bully. Perhaps there is a hint of an answer to this apparent contradiction in the argument that the foreign policy of the Truman administration was based on the need for international tensions to permit integration and mobilization of the public, Congress, and allied nations under Washington's hegemony.[67] Did American leaders restrain themselves from eliminating Soviet power out of a perceived need to maintain a plausible external enemy? We cannot be certain because the argument is not fully developed.

In summary, then, the radical left and right view the United States and the Soviet Union, respectively, as quite exceptional nations, perhaps unique in history. Their leaders exist in international environments that neither pose genuine challenges to their security, nor constrain their policy options. And, within the limits established by the needs of the ruling class, they are also free from domestic pressures. Having established these points, largely by definition rather than with evidence, it is then easy to attribute all policy decisions to rational and unencumbered choices, made by men who represent structurally evil institutions and morally abhorrent ideologies.

13. *The military is not an autonomous force in the enemy's political system; it is subordinated to and dominated by the civilian ruling class.*

Neither the radical right nor the radical left perceives the enemy as a traditional military dictatorship. Those on the right regard the Soviet military as but one branch of the Communist apparatus for protracted warfare against the Free World. They point, with more than a touch of envy, to the serious hearing that military advisers are given in the Kremlin, in contrast to the shabby treatment they purportedly receive in Washington.[68] They are not, however, inclined to doubt that the army loyally follows the lead of the party in the U.S.S.R.

Radical left analysts regard the role of the military in the United States similarly, stoutly rejecting interpretations of American foreign policy that assign it a central and autonomous role in policy formation. The professional soldier is regarded as a subservient and compliant servant of the capitalist ruling class—the effect rather than the cause of American imperialism. Even in specific decisions involving foreign and defense policies, military men only follow the martial lead of the civilian rulers.[69] The soldier who questions the policies of his civilian superiors is quickly purged; Kolko cites as typical the cases of Generals Douglas MacArthur and Edwin Walker. Even as renowned a critic of American society as C. Wright Mills is chided for having described the military as an autonomous force in American politics, and for slighting the pervasive power of the economic elite and the class interests they serve.[70] Those who regard civilian control of the military as an important issue in a democratic society are therefore deluded; the answer is not to ensure civilian control of the military, but to eliminate the evil "system" the soldiers serve.

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[65] Alperovitz, p. 101; Kolko, *Politics,* p. 622; Horowitz, *Free World Colossus.*

[66] Oglesby and Shaull, p. 42; Horowitz, *Empire and Revolution,* p. 50; and Kolko and Kolko, pp. 4, 54–58, 482–483.

[67] Kolko and Kolko, p. 671. "Indeed, to American leaders the value of the Soviet Union was precisely in the carefully contrived appearance of bolshevik ferociousness which they might employ to mobilize Congress and the public to sustain expansive policies for other areas of the world where Russia or its allied parties were docile or irrelevant." Kolko and Kolko, p. 483.

[68] "In the Soviet Union, the strategy recommended to the Politburo is made by military professionals and not by a comparable cortege of economists, sociologists, psychiatrists, and comptrollers, the groups that have sought to monopolize strategic thinking in the United States." Kintner, p. 51. Presumably the protracted conflict group, which has never been reticent about offering strategic advice, falls into another category.

[69] "In the spring of 1950, NSC-68 revealed that the civilians were far more martial than the generals when containment doctrine was failing to turn the political tide in Asia and sustain military mastery in Europe. The desire also to spend money as a tool of foreign economic policy as well was scarcely comprehensible to the docile military men. But now, galvanized by its own fears and needs, frustrations and ambitions, seeking tools for politically and economically managing Europe and the world, Washington had only to find the excuse [Korea] to embark on that tortured, endless, and infinitely costly and dangerous arms race which terrorizes our civilization." Kolko and Kolko, pp. 508–509.

Thus to radicals of both persuasions the enemy is not an example of a conventional military dictatorship; it is, rather, a "garrison state" in which specialists in violence faithfully serve the interest of the ruling class.

The Conditions of Peace

14a. (Radical right version). *The elimination of communism and the establishment of institutions reflecting the values of Western civilization will usher in an era of peace.*

14b. (Radical left version). *The elimination of capitalism and its replacement by socialism will ensure an era of peace.*

The radical right and radical left are united not only in their belief that the cause of conflict is readily identifiable. They also assume that the replacement of malignant social systems with a system of their choice will result in peace.

Those on the right argue that peace awaits the day when all nations create an international political-legal order under leadership of the Western nations. The only alternative to a system of nations modeled on the American values is global war. With the destruction of Communist totalitarianism the peoples of the world can establish a peaceful international order.[71]

The first and most important step to peace and justice, according to the radical left, is elimination of the American empire, "the essential precondition for the emergence of a nation and a world in which mass hunger, suppression, and war are no longer the inevitable and continuous characteristics of modern civilization."[72] This, as we shall show later, allegedly requires the revolutionary transformation of the political, corporate, and military structures of American society—the destruction of a system of class rule dominated by the interests of an unchallenged business elite.

The corollary is the premise that socialist states are by nature peaceful.[73] Because a just and humane political economy does not depend on exploitation, even a nation as malevolent as America would be peaceful if socialist. Only such states are committed to and capable of resolving any of the social and economic problems—poverty, racism, injustice, and the like—that might breed future wars; all others are inherently unable to "terminate the vast misery of their masses."[74] It therefore follows that a world of socialist nations will be a peaceful one. Presumably such a global system would be marked by the absence of great power conflicts, incompatible efforts to construct a preferred international order, and status differentials, to say nothing of competing territorial and other interests that might prove irreconcilable.

The record of conflict *between* socialist states—for example, the Soviet invasions of Hungary and Czechoslovakia, or its bitter disputes with Yugoslavia and China—might seem to call this premise into serious question. Radical left analysts usually rely on several rebuttals. An aggressive socialist state is one that has strayed from the faith; this is true because by definition "genuine" socialist states are peaceful. Thus, the Soviet invasion of Czechoslovakia is sometimes explained by the fact that the U.S.S.R. has fallen from a state of grace. An especially ingenious solution is to assert that the Soviet Union practices "state capitalism"! Alternatively, it is argued that as long as any capitalist states exist, the socialist ones cannot always act according to their inherently peaceful nature.[75] Advocates of this line of reasoning argue that the Czech invasion was made necessary by the threat of capitalist subversion, for example, from West Germany. A third approach to this seeming contradiction is to deny that ordinary standards of ethical judgment can be applied to violence perpetrated by the U.S.S.R. and other socialist states. As Sartre puts it, "Russia is not comparable to other countries. It is only permissible to judge it when one has accepted its undertaking, and then only in the name of that undertaking."[76] In the conclusion we shall consider further the double standard style of interpreting history.

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[70] Kolko, *Roots*, pp. 27–37. See also Alperovitz, p. 76.

[71] "The United States, which alone among the great nations was founded upon a rational set of political and legal ideals, must bear the prime responsibility for the creation of such an order . . . in which the rule of liberty under law enters into the possession of a great majority of mankind, instead of remaining the exclusive heritage of the Western peoples." Strausz-Hupé et al., *Protracted Conflict*, p. 150. See also Crane, p. 549; and Burke, p. 203.

[72] Kolko, *Roots*, p. 87.

[73] In his monumental study of modern war, Quincy Wright found that socialist economies "have produced the most warlike states of history." He goes on to point out that, "States at war have tended to become socialistic, and socialistic states have tended to be at war." Quincy Wright, *A Study of War*, 2nd ed. (Chicago: University of Chicago Press, 1965), pp. 831, 1172. Whether or not Wright's conclusions also apply to contemporary socialist states, they at least suggest that a simple equation between socialism and peace can scarcely be assumed.

[74] Kolko, *Roots*, p. 58.

[75] Thus, Stalin did not enjoy "dooming the Poles or

Having identified the causes of war, analysts of both persuasions thus assume that when the forces of aggression and repression have been eliminated, the international system will undergo a fundamental change. The path to peace is not one of painstaking, piecemeal resolution of the many conflict-creating problems hanging over from World War II, from the colonial era, or from vast disparities in wealth, resources, and standards of living. Requiring some degree of accommodation with the existing international system, at least in the short run, this illusory approach to conflict-resolution is viewed as a typical "liberal" solution—both practically ineffective and morally offensive. It fails to come to grips with the "real" sources of conflict in the world, and it implies a willingness to strive for some form of compromise and accommodation with an evil adversary. It suggests abandonment of crusades to eliminate, once and for all, the enemy's malignant social system.

15. *There are fundamental weaknesses and contradictions in the enemy's system.*

16. *The forces of aggression and repression can be defeated, but this is conditional upon appropriate actions.*

17. *Destruction of the enemy's system is dependent upon external defeat.*

Analysts of both the radical right and left are conditionally optimistic about the ultimate defeat of the enemy, provided that the forces of peace persevere in the pursuit of appropriate policies. This belief stems from the premise that basic contradictions within the enemy nation can be exploited. But elimination of the adversary's social system will not occur solely as a result of domestic pressures. Radicals of both schools view internal dissent as an inadequate vehicle for restructuring a repressive society that is essentially beyond reform. External defeat is the necessary catalyst.

Although analysts of the radical right cannot be charged with insufficient attention to or underestimating the power of Soviet military capabilities,[77] they insist that behind the facade of great strength there are inherent defects that can be exploited by the West. These include a discontented and sullen population, an empire of repressed nations, a record of failure to achieve any of the goals promised by Marxist theology, and an inability to resolve such basic political problems as the rules of leadership succession.[78] That the U.S.S.R. has gained spectacular international victories can more properly be attributed to intellectual and moral failures of Western statesmanship than to Soviet capabilities: "The Communists are likely to win World War III because they know they are in it."[79] Were Free World leaders to understand the essential nature of communism, and were they to adopt a "forward strategy" aimed at its eventual elimination, the evident failures of Communist totalitarianism would soon become apparent. Frustration of external adventures would illuminate and magnify the failures at home, and encourage dissidents to work against the regime. The Soviet empire must either grow or it will recede, and eventually collapse or be destroyed. The key is Western determination, and on this score the protracted conflict school is at best conditionally optimistic: "At the present juncture of the conflict, however, paralysis of the will is increasing faster in the West than is the rate of decay within the communist orbit."[80]

The radical left argument is similar in structure. The Great Depression and World War II demonstrated the bankruptcy of capitalism. Yet the contradictions in capitalist America have remained hidden—except, of course, to those with special insight and moral sensitivity—under a cloak of superficial prosperity, sus-

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Czechs to servitude." He did so because "(1) he wanted to secure the Soviet Union from collapse, and (2) because the peculiar configuration of Soviet and Western weaknesses and strengths limited his possibilities to that one course." Oglesby and Shaull, p. 42. See also Kolko and Kolko, pp. 56, 58.

[76] Jean-Paul Sartre, quoted in Beichman, *Nine Lies*, p. 208. Herbert Marcuse is another member in good standing of the double-standard school of ethics. See footnote 95 below.

[77] An exception is Evans, pp. 197–198, 209, who argues that our overestimates of Soviet military strength stem from unverified Russian claims and unjustified Western assumptions.

[78] See, for example, Possony, *Century*, p. 419.

[79] Bouscaren, p. 4. The same sentence appears in a number of other books, including Strausz-Hupé et al., *Protracted Conflict*.

[80] *Protracted Conflict*, p. 132; and Niemeyer.

tained by economic imperialism. Put most simply, the radical left thesis is that the exploited peoples of the American empire have paid the costs of maintaining capitalism in the United States. Because the American people themselves have been unwilling or unable to transform their society into a peaceful (i.e., socialist) state, the impetus must come from without. Fortunately, imperialist expansion contains the seeds of its own destruction. Once defeated abroad, even in peripheral areas such as Vietnam, America's ruling class will be seen in its true light—as the naked emperors of a decaying empire. "The rest of the world is forced to do for the American people what they are only tangentially presently able and willing to do for themselves. The victims of American executioners, ironically, thereby work not only for their own integrity and freedom but indirectly for that of the American people themselves." The catalyst of external defeat will set in motion a series of events leading to the ultimate transformation of American society: "the failure of America's global strategy will open up new options and tensions at home."[81] The American people will then be able to carry out the necessary task of destroying the old order and replacing it with a peaceful socialist one.

Radical analysts at both ends of the spectrum thus take an organic view of imperialism. Once the enemy's empire stops growing, it will begin to recede. Both further agree that the process of attenuation must begin with external defeat.

18. *The war in Vietnam represents a probable turning point in history.*

Analysts of both schools subscribe to a rather extended form of the so-called "domino theory" in which the war in Vietnam is seen as a likely turning point in history.

Radicals of the right regard Vietnam as the critical testing ground for the Communist plan of "protracted conflict" against the Free World. It represents not only blatant aggression against an integral part of the Free World but, more important, it is a test of the West's ability to survive political-psychological-subversive warfare against its will, determination, and morale. Only firm military action by the United States in 1965 thwarted a worldwide Communist assault against the Free World.[82] If, despite this commitment, Communist forces prevail in Indo-China, it will confirm their ability to win World War III without risking a direct nuclear attack against the Free World. Conversely, a Communist defeat in Indo-China would prove to all peoples that resolute and timely action by the Free World can defeat Moscow's strategy of protracted conflict.

The mirror image of this thesis can be found on the radical left. American intervention in Indo-China is viewed not as an aberration or the result of miscalculation, but as a logical and inescapable outgrowth of the drive to empire by the capitalist establishment.[83] A defeat for imperial America in the jungles and rice paddies of South Vietnam would be even more consequential—and salutary—than predicted by the standard version of the domino theory. It would represent the beginning of the end for the American empire. Revolutionaries elsewhere in Asia, Africa, and Latin America would receive encouragement from a demonstration that the emperor's suit of technological and nuclear armor is highly vulnerable, and that America is indeed a helpless giant when faced with guerrilla warfare.[84] Most important, defeat in Vietnam would mark the beginning of the end for an American domestic prosperity based on exploitation both in the Third World and at home. Failure would make possible a revolutionary restructuring of American society, a necessary and sufficient condition for world peace: "On the outcome of this epic contest [in Vietnam] rests the future of peace and social progress in the world for the remainder of the twentieth century, not just for those who struggle to overcome the legacy of colonialism and oppression to build new lives, but for the people of the United States themselves."[85]

In summary, then, the arguments of the right and left are virtually identical in structure (Figure 1). Both adhere to an extreme version of the domino theory, placing great emphasis on the outcome of the war in Indo-China. Defeat of the enemy would not be an isolated episode, but rather the first link in a causal chain of events that ultimately holds the promise of genuine peace.

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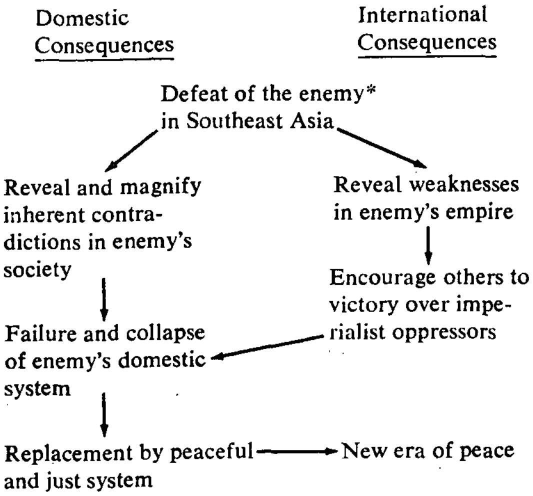
[81] Kolko, *Roots*, pp. 137, xviii.

[82] Possony, "Mao's Strategic Initiative," *Orbis* 11 (Spring, 1967).

[83] ". . . Vietnam is the most important single embodiment of the power and purposes of American foreign policy since the Second World War. . . ." Kolko, *Roots*, p. 88. See also Oglesby and Shaull, pp. 112–113.

[84] "For the United States to fail in Vietnam would be to make the point that even the massive intervention of the most powerful nation in the history of the world was insufficient to stem profoundly popular social and national revolutions throughout the world." Kolko, *Roots*, p. 90.

[85] Kolko, *Roots*, p. 132.


Domestic Consequences
International Consequences
Defeat of the enemy* in Southeast Asia
Reveal and magnify inherent contradictions in enemy's society
Reveal weaknesses in enemy's empire
Encourage others to victory over imperialist oppressors
Failure and collapse of enemy's domestic system
Replacement by peaceful and just system
New era of peace

* For radical right = communist imperialism (U.S.S.R., China)

For radical left = capitalist imperialism (U.S.)

Figure 1

Ends and Means in Politics

19. *The goal of politics is total victory over the enemy.*

The belief that the source of war and injustice in the world is readily identifiable provides substantial rationale for the argument that nothing short of total victory over and elimination of the offending social system is acceptable. Politics is viewed as a "zero-sum game" in which the object is to eliminate the adversary.[86] The politics of coexistence and compromise carry a number of implications that neither the radical right nor left accepts. It suggests that all parties to a conflict may share some responsibility for its existence as well as for its resolution. It also implies a willingness to hold moral judgment in abeyance: once political issues are cloaked in ethical terms, the probabilities of successful bargaining and compromise are materially reduced. As shown earlier, neither the radical right nor radical left is burdened with doubts about the moral superiority of its cause or about the immorality of its adversaries. Both hold steadfastly to "inherent bad faith" models of their opponents, and to the belief that angels—and Clio, the muse of history—have the wisdom to be on their side.

Thus the radicals on the right call for a "forward strategy" aimed at elimination of the Communist system, while attacking the liberals for being beguiled by the sirens of coexistence. "The first condition for successful psycho-political warfare directed against communism is a determination to fight to win, eventually to destroy this totalitarian phenomenon which endangers the very existence of Western civilization. A part of this determination is abandonment once and for all of all the notions of lasting compromise, accommodation, or coming to terms with present-day communism."[87]

On the left there is a consistent call for revolutionary transformation of capitalist America as a necessary and sufficient condition for solving mankind's most pressing moral and social problems. It is scarcely surprising that having thus posed the issue, the radical on the left is no more eager to accept the notion of coexistence with "evil" than is his counterpart on the right.

20. *Means can only be judged in conjunction with the character of those who use them.*

21. *Force and violence are acceptable when used for just ends.*

22. *Peace is an end state, not a process.*

Although radicals of the right and left are moral absolutists with respect to goals, they take a relativist position regarding the instruments of politics. This is most evident in their prescriptive assertions about war and violence. Having established to their own satisfaction that the causes of war, injustice and other social evils are readily identifiable, advocates of both radical positions adhere to a contemporary version of the "just war" doctrine to sanction violent action against their respective enemies. The two sets of arguments are remarkably similar in structure although, of course, the intended targets of violence differ.

The radical right thesis is that because communism is an institutionalized form of violence, it is sheer folly—or worse—to believe that its expansion can be countered by "pious homilies on the blessings of peace."[88] A quarter century or more of liberal delusions about "liberalization" and the prospects for coexistence has merely eased the way for Communist enslavement of a large part of the world's population. To negotiate with the adversary is at best imprudent, for negotiation is "merely one technique, albeit a highly effective one, prescribed by the doctrine of protracted conflict."[89] The best guarantee of peace is therefore the possession and willingness to use superior military power in the hands of the forces of peace—that is, those opposing communism. Concepts such as "mutual deterrence," "balance of power," or "sufficiency" are dangerously inadequate to guarantee peace, for they imply a symmetry of forces. Communist deception, secrecy, and the possibilities of a technological breakthrough are reasons enough to justify whatever effort is required to achieve military superiority.[90] That any prospects for détente might be lost is regarded with less than a tragic sense of loss. Coexistence, an "Aesopian" term signifying war by other means, is a snare best avoided by the West.

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[86] "The defeat of the communist purpose will be our victory; their success will be our defeat. . . . There is no middle ground between communist takeover and communist defeat." Michael, "The Stakes in Vietnam," p. 122.

"We cannot be secure until freedom reigns in all of the decisive areas of the globe. Nor can Khrushchev and Mao end their warfare against us until freedom is obliterated from this earth." Kintner and Kornfeder, p. 348.

[87] Kintner and Kornfeder, p. 345.

[88] Strausz-Hupé et al., *Protracted Conflict*, p. 5. "Violence is the focus of bolshevik operational thinking." Possony, *Century*, pp. xvii–xviii.

In line with this reasoning analysts of the right interpret the history of the cold war as a series of defeats and lost opportunities brought on by the lack of intestinal fortitude in the Free World. Unwillingness in 1945 to take a tough stand against Soviet expansion triggered the cold war. In Korea, the West failed to gain the military initiative owing to wholly unfounded fears of general war; the U.S.S.R. was not prepared in 1950–51 to risk World War III. The West's "supine" acceptance of Soviet penetration of the Middle East—Czech arms shipments to Egypt in 1955 should have been seized—was followed by even worse folly. The United States forced the British and French to withdraw from the Suez operation, and the Soviets won the "credit" for rattling their missiles at London and Paris. Hungary was simultaneously sacrificed by the West's unwillingness to take "modest risk." Tough talk followed by retreat in Laos, Cuba, and elsewhere have merely fueled the engines of Communist aggression. Deployment of American troops in Vietnam stands out as one of the few bright spots in an otherwise disastrous record. The necessary "forward strategy" may seem to entail risks, but any resulting conflict would be a just war, for "The defense of civilization is tantamount to the destruction of the communist movement throughout the world."[91]

Radical left support for a just war doctrine emerges most clearly in the recent debate among "peace researchers," that is, social scientists who study violence. Until recently there was a widespread consensus that control and reduction of violence were the practical goals of peace research. A fairly sizeable group on the left now argues that a false equation of peace with the absence of violence merely supports an intolerable status quo.[92] A few even assert that their "science" permits them to identify the appropriate targets of violence.[93] Moreover, by emphasizing the concept of "structural violence" the term violence has been stretched to encompass relationships within virtually any institution or nation which falls short of absolute egalitarianism. This would seem to support a program of abolishing all institutions, or at least opposing the status quo everywhere, by violence if need be, as none yet created by man meets this condition. In practice, however, the concept is usually limited to describing deficiencies in Western institutions, especially American ones, and to justifying revolutionary action against them. If "structural violence" is to be more than a slogan it should also encompass such institutionalized policies as the systematic suppression of ethnic or religious minorities, harassment and imprisonment of nonconforming artists and intellectuals, long-term sentences to "labor camps" where all but the hardiest are doomed, or use of capital punishment even for such relatively minor offenses as theft of state property or black market operations. Rarely, if ever, has a detailed analysis of structural violence been used to condemn, much less justify the use of force against the U.S.S.R., China, Cuba or other socialist states in which these are common practices.[94] The status quo there is more commonly celebrated than condemned. A double standard is thus used to judge violence of the left and right. "The violence of revolutionary terror, for example, is very different from that of the White terror, because revolutionary terror as terror implies its own abolition in the process of creating a free society, which is not the case for the White terror. The terror employed in the defense of North Vietnam is essentially different from the terror employed in the aggression."[95] The belief that revolutionary violence and terror are self-arresting processes terminating in freedom must surely be taken as a metaphysical article of faith for, beyond a few examples such as the American Revolution, few propositions run counter to such impressive historical evidence.

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[89] Strausz-Hupé et al., *Protracted Conflict,* p. 19.

[90] *Ibid.,* p. 125; Edward Teller, "Planning for Peace," *Orbis,* 10 (Summer, 1966); Evans, p. 247; and Crane, p. 549. Teller used this argument to attack the Nuclear Test Ban Treaty of 1963.

[91] Burke, 198. See also, Evans, pp. 522–523, 409; Strausz-Hupé et al., *Protracted Conflict,* pp. 17, 48, 89 and 137; Possony, "Mao's Strategic Initiative."

[92] See, for example, writings by Lars Dencik, Martin Jarvad, Ole Jess Olsen, and Herman Schmid. This literature is reviewed in Hanna Newcombe and Alan G. Newcombe, "Approaches to Peace Research," *Peace Research Reviews,* 4 (No. 4, 1972), 1–23; and Michael D. Wallace, "The Radical Critique of Peace Research: An Exposition and Interpretation," *Peace Research Reviews,* 4 (No. 4, 1972), 24–51. On "the rising mystique of violence on the left," see *American Violence: A Documentary History,* ed. Richard Hofstadter and Michael Wallace (New York: Macmillan, 1967), pp. 29–43; and Richard Lowenthal, "Unreason and Revolution," *Encounter,* 23 (Nov. 1969), pp. 22–34.

[93] An especially ill-conceived effort of this type may be found in the recent work of William Eckhardt. His research instruments are well-suited to "discover" that favorable qualities (personal choice, freedom, insight, and conscience) are associated with the radical left, whereas bad ones ("unfreedom" and "inequality") are attributed to conservatism and positivism. This effort would merely be bad social science were it not for Eckhardt's claim of policy relevance for his findings. Arguing that peace researchers must "rethink" their position on nonviolence, he goes on to assert that we now know those against whom the use of violence is justified. Comments to the Western Region meeting of the Peace Research Society, Vancouver, Canada, February 14, 1972. See also his "Research and Education as Approaches to Peace and Justice," *Peace Research Reviews,* 4 (No. 4, 1972), pp. 70–101.

Having defined capitalist institutions as violent, structurally or otherwise, the radical on the left has no more difficulty in sanctioning the "just revolution" against them than does the radical on the right have in encouraging the use of whatever means might eliminate the scourge of communism. The argument is made all the more attractive by the assumption that successful revolutionary warfare is a necessary and sufficient condition for the ultimate elimination of violence, and is buttressed by the premise that the capitalist ruling class will never permit "real" change through electoral or other nonviolent means. Few on the radical left would quarrel, for instance, with Senator Barry Goldwater's charge that the American electoral process has produced only "an echo," not "a choice."

There is, finally, one other similarity in the arguments of the radical right and left. Both tend to regard peace as an end state. It is a *condition* that will exist in the future when the institutional and ideological bases of war have been destroyed, rather than a *process* or a precondition for the resolution of critical issues. There is scant recognition of the alternative proposition that means have a way of becoming ends, or at least of stamping their character on the goals that are ultimately achieved. This shared conception of peace has important normative implications, because it provides one justification for employment of violence against selected targets.

It might seem unfair to conclude that the radical right and left share a crude "ends justify the means" philosophy, and it would certainly be inaccurate to suggest that these versions of the just war doctrine are the monopoly of the radical right and left. Justifications for the use of violence are, after all, as old as mankind itself. Certainly those who have embarked on "wars to end all wars" have not escaped this type of reasoning. Finally, all but unconditional pacifists would probably accept the proposition that under some circumstances a lesser degree of force may be employed if there is a high probability that doing so will avert a greater degree of violence. These important qualifications aside, there are several qualities of the radical arguments that seem especially pernicious: the often undisguised double standard used to judge means; the absence of doubt about the sources of evil in the world; and the messianic belief that once the forces of darkness have been swept aside mankind will enter an unprecedented era of peace and justice.

Conclusion

The fact that the radical right and left explanations of international politics and foreign policy are similar in structure does not, *and logically cannot*, tell us very much about the extent to which one or both may be valid, either as theoretical bases for extending our understanding or as guides for action. It would, moreover, be illogical to assume that whatever weaknesses exist in theories at the extremes demonstrate the validity and utility of more conventional analyses. For one thing, the radical explanations share at least some points with more conventional approaches; we pointed out earlier, for example, that the idea of "a war to end all wars" can scarcely be called an invention of either school of radicals. More important, other theories and modes of explanation deserve critical analyses on their own merits. To assume that flaws in radical theories can also serve as evidence in support of competing explanations is to fall prey to "straw man" reasoning. Nor can we use a "golden mean" argument to suggest that more conventional theories, representing a midpoint between the radical ones, share the best of both and are saddled with the liabilities of neither. Indeed, if the argument presented in this essay is valid, it strongly suggests that the radical theories do *not* represent the opposite ends of a linear continuum; therefore the notion of a "golden mean" between them is meaningless.

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[94] The work of Johan Galtung is an important exception in this respect.

[95] Herbert Marcuse, *Five Lectures: Psychoanalysis, Politics and Utopia* (Boston: Beacon, 1970), p. 103.

Some qualities of the theories reviewed here have considerable merit. For example, scholars of both schools are committed to an examination of the institutional and ideological bases of foreign policy. The premise of linkages between domestic and external policies is a reasonable working hypothesis, although it can hardly claim the status of an empirically-based "iron law" with which it is endowed by the writers under review. Nor can one quarrel with a multidisciplinary perspective on the study of foreign policy, integrating rather than compartmentalizing political, economic, military, and other facets of external relations. But this too can be pushed to absurd lengths, as it is when foreign policy is reduced by definition to the external manifestation of economic imperatives.

A comprehensive critical assessment of the theories and research under consideration would extend this essay into a book, or even a series of books. This conclusion is therefore limited to a consideration of two points on which the radical right and left explanations seem especially vulnerable. First, *both theories appear to be essentially nonfalsifiable*. That is, they are used by their proponents in a way that any data, even those that appear quite inconsistent with the theory, are interpreted as supporting evidence. Second, there are some *methodological problems* as well, difficulties that seem to be of a fundamental rather than trivial nature.

At the core of the radical right analysis of contemporary politics is a set of assumptions about international communism and its implacable drive to world power. The nature of that movement, according to the authors we have considered, can be discovered by the theory of "protracted conflict," which postulates a multifaceted, long-range war against the Free World, as set forth in the writings of Marx, Lenin, and others. There is, of course, no denying the importance of Marxist-Leninist writings. Nor need one question those aspects of that doctrine singled out for special emphasis by these authors: the tactics of pushing forward, retreating or zig-zagging, depending on the situation; the arcane language of Marxist dialectics ("Aesopian language"); and the importance of a messianic belief in the Marxist New Jerusalem. Specialists on Marxist-Leninism of almost all political persuasions would probably accord each of these points a prominent place.

The difficulty arises in their use by the "protracted conflict" school. Virtually any Soviet policy or action is deemed consistent with the theory. Put somewhat differently, it is hard to see what evidence would be required to raise serious doubts about the protracted war framework. Thus, when the Soviets threaten Berlin, invade Finland, Hungary or Czechoslovakia, or provide logistical support for the North Korean invasion of South Korea, one can quite properly cite these as evidence of Soviet aggressiveness. On the other hand, Soviet participation in international negotiation on some issue or another, trade with the West, or other normal forms of diplomatic intercourse are also cited as evidence in support of the theory.[96] These actions are interpreted as a temporary period of cooperation and consolidation ("the zig-zag" theory) before the next phase of aggression, or as an indication that because the more naked forms of aggression may create too many risks at that point in time, the Soviets are switching emphasis to other instruments in their kit-bag of aggressive tools. The theory is irrefutable.

Consider a more specific example, the vehement opposition by the radical right to the 1963 Nuclear Test Ban Treaty. A key reason was that either the terms of the treaty favored the Soviets, or else they planned to cheat on its terms. Why? Because otherwise the Soviets wouldn't have been foolish enough to sign it. And how do we know that? Because the Soviets are unalterably committed to victory over the Free World, and they would certainly not agree to an undertaking that might hamper achievement of that goal. Q.E.D. In short, we have a circular argument which always returns to a set of axioms of how Communists must, by definition, behave.

The difficulty, then, is *not* that the protracted conflict school takes a dim view of Soviet goals, strategy, and tactics. Only the most committed apologist for the Soviet regime could fail to agree on many of these points. The real problem is that the protracted conflict theory does not appear capable of falsification.

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[96] To be more precise, the radical right tends to argue that *no activity* undertaken by Communist nations can be considered "normal," owing to the nature of Communist ideology and practice. This line of reasoning underlies the radical right argument against policies that contain any suggestion of détente. See, for example, Strausz-Hupé et al., *Protracted Conflict*, p. 119.

What evidence would be required to cast doubt on some core elements of the theory? Probably nothing short of a series of Soviet or Chinese actions that would be unprecedented in the history of great power diplomacy—for example, unilateral disarmament, followed by invitations to inspectors for verification of the facts. It is not clear that even such utterly implausible actions would suffice to convince all those on the radical right. It is predictable that at least some of the following interpretations and prescriptions would be offered: this is a sign that the Soviet system was too weak to support a large arms budget, hence the action reflects a change of tactics rather than of fundamental goals; this is a trick to make us drop our guard; or, this is a spectacular opportunity to end the Communist menace once and for all. In any case, there will always be trouble spots somewhere in the world that can be attributed to Communist machinations. Possony's analysis of events (and a spectacular nonevent, a "gigantic tricontinental people's war") in 1965 demonstrates the ease with which one may do so.[97]

Radical left theory/epistemology is equally vulnerable on the point of non-falsifiability. Starting from Marxist or neo-Marxist theoretical bases, analysts of this school argue that the imperatives of capitalist institutions constitute a necessary and sufficient explanation of American foreign policy. The search for markets and cheap raw materials are the international manifestations of the implacable drive for profit. No one would argue that a capitalist or mixed economy can operate without markets, raw materials, and profits (are these considerations any less important for state-owned enterprises?). But when it can be shown that an external undertaking has little relationship to markets or raw materials, and when it is undeniably clear that it is generating massive deficits rather than profits—the war in Vietnam is a prime example—then the radical argument takes another tack. In these circumstances, it is argued, the capitalist ruling class is willing to accept a loss in the short run to protect the integrity of the empire (presumably pursuing a domino-theory line of reasoning), and therefore, profits, for the long run. How do we know this? Because capitalists are by definition driven by the profit motive and because the ruling elite acts on the basis of rational calculation. Hence, the seemingly uncharacteristic policy undertaking is actually an integral part of a longer run plan.[98] Q.E.D.

The treatment of American policy on colonialism is especially instructive. How does one explain imperialist America's policies when they seem to support nationalist movements against colonialism? The United States allegedly eased the British out of the Middle East in order to gain control of the oil resources there. But how does this square with the consistent support Washington has given Israel, a negligible factor in the oil industry and the common enemy of virtually all petroleum-exporting nations in the area, since 1948? By asserting (but not taking the trouble to demonstrate) that Washington has skillfully maintained a tension-ridden status quo in the area by alternating support for the Arab nations and Israel. That this answer bypasses many inconvenient questions is apparently no source of embarrassment. Other cases are simply passed off by neglect. Beyond a single brief paragraph charging that Washington sought to push the British out of India in order to gain a privileged position for itself,[99] the issue of Indian independence is neglected. Not even a paragraph is devoted to the question of Indonesian independence and American policy toward Dutch colonialism. Admittedly a single book cannot cover all events in full detail, but the omissions are startling in a work that purports to be so concerned with problems of imperialism. The rule of inference appears to be: If Washington opposes colonialism, it is because of a selfish desire to replace European imperialism with an American variety; if it does not, it is evidence of the linkage between capitalism and colonialism or neocolonialism.

It is also interesting to speculate on possible explanations for American failure to take action against expropriations of utilities or major oil and mining firms in Mexico, Peru, Chile, Libya and other countries. It is clear that the same line of reasoning could apply. Sophisticated capitalists are taking a longer-run view of their interests, or they are altering tactics to maintain and extend the empire by other than direct means. How do we know this? Because this is how, by their nature, capitalists must operate, and there are no competing factions whose interests might be reflected by those who make American foreign policy. Again, it is very difficult to imagine the types of policies that would cause the radical left analysts to question and perhaps discard the theory of capitalist imperialism. Thus, what its advocates would claim as a major virtue of the theory—that it is able to encompass and integrate all foreign policy behavior into a coherent pattern—must be regarded by less passionately committed observers as a fatal flaw. A theory that is impervious to apparently contradictory evidence and is, therefore, incapable of falsification simply fails to meet a basic requirement of explanation.

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[97] Possony, "Mao's Strategic Initiative," p. 181.

[98] For further development of this point, see Tucker, *The Radical Left and American Foreign Policy*, pp. 75–82.

[99] Kolko and Kolko, pp. 76, 427.

Buttressing my doubts about the radical theorists are some of their methodological practices.[100] One of the standard works of New Left revisionism, Gar Alperovitz's *Atomic Diplomacy,* purports to show that atomic bombs were used in 1945, not to defeat a collapsing Japan, but to threaten the Soviet Union with a display of American military superiority.[101] It has recently been demonstrated, however, that *Atomic Diplomacy* is marred by such questionable uses of quotations (e.g. ellipses to alter meanings, quotations out of context, etc.) as to cast serious doubt on the merits of the entire book.[102] Other works also make heavy use of quotations, but the methods and criteria of selection are scarcely systematic or even visible. There is no indication, for example, of what quotations are not used.[103] Nor is much attention given to the context in which policy pronouncements are made. Kolko, for example, repeatedly quotes statements assuring congressional committees that American aid and trade policies are intended to serve the vital interests of the United States.[104] This should scarcely occasion surprise or outrage, except by the naive or by those who are preparing an indictment. It would be astonishing indeed, if not practically suicidal, for an American Secretary of State to inform the House Foreign Affairs Committee that a foreign aid bill was intended to serve only humanitarian ends, perhaps even to the detriment of national interests. To assure skeptical congressmen that aid and trade programs contribute to American goals is one of the rhetorical rituals in which every Secretary of State must engage annually, especially when foreign aid allocations include funds for Communist nations such as Yugoslavia, or for persistent critics of American society and diplomacy—for example, India. In any case, were assertions of pristine humanitarianism unalloyed by statements of national interest, their authors would surely be open to legitimate criticism on the grounds of hypocracy.

Consider one other example. Henry Wallace is quoted in 1946 as stating, ". . . we conceive our primary function . . . to be to represent the United States' trade interests. . . ."[105] This is offered as evidence of a monolithic capitalist ideology in Washington. In fact, because Wallace was Secretary of Commerce at the time his assertion was the functional equivalent of a union leader paying homage to the working man on Labor Day. It might support an indictment for banality, but not much else.

When events that do not quite fit the theory are encountered, there is a disturbing tendency to explain them away. Kolko's analysis of events in Poland during World War II provides some interesting examples. The massacre of some 10,000 Polish officers in the Katyn Forest is dismissed as peripheral to the Polish question and its impact on relations between the Allies. Even if the Soviets were guilty, certain "larger facts" must be considered. British and American intransigence, along with the xenophobia of the London Poles, purportedly would have made Poland a source of Soviet-American tensions whether or not the massacre had taken place.[106] The Soviet decision to halt its advancing armies on the outskirts of Warsaw while the Nazis wiped out the Polish uprising in that city is also explained away—it was the fatigue of the Russian troops and the foolish plans of the Poles that permitted the Nazis to eliminate a significant part of the non-Communist Polish elite.[107] Although the questions are hypothetical ones that radical historians have not been forced to confront, it seems not wholly unfair to wonder whether comparable actions, had they been undertaken by British or American troops, would have been treated with similar solicitude. Had the Western allies slaughtered several thousand Communist leaders of the French underground, would the matter have been dismissed as quite irrelevant to post-war relations with Moscow? Had the Anglo-American offensive in 1944 been stopped at precisely the moment that would have permitted the Nazis to wipe out the entire Communist leadership in France, would the fatigue of advancing armies have been accepted as a satisfactory explanation? Or would there have been a temptation to cite such events as further evidence of wartime machinations by the forces of capitalism?

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[100] In order to explore these problems in some detail, my comment will focus on just a few works, with special attention to those of the most prolific writer on recent American diplomacy, Gabriel Kolko.

Two historians have criticized their New Left colleagues for being methodologically "reactionary." Unger, pp. 1241, 1262–63; and David Hackett Fischer, *Historians' Fallacies: Toward a Logic of Historical Thought* (New York: Harper and Row, 1970), p. 314. The former suggests that leftist hostility toward more systemic research methods such as quantification may arise from a fear that "the figures will not bear them out." Unger, p. 1241. For an analysis of other methodological deficiencies, see Ole R. Holsti, "The Abuse of Statistics: Examples from Studies of American Public Policy" (Center for Advanced Study in The Behavioral Sciences: Mimeo, 1972).

[101] Gar Alperovitz, *Atomic Diplomacy: Hiroshima and Potsdam: The Use of the Atomic Bomb and American Confrontation with Soviet Power* (New York: Simon and Schuster, 1965).

[102] Robert James Maddox, "Atomic Diplomacy: A Study in Creative Writing," *Journal of American History,* 59 (March, 1973), 925–934.

[103] Consult Tucker, pp. 60–61, for a further discussion of this point.

[104] Kolko, *Roots,* p. 65 and elsewhere.

[105] Kolko and Kolko, p. 23. Ellipses in the original. Two of my favorite examples are statements attributed to James F. Byrnes and William L. Clayton, respectively: "Our international policies and our domestic policies are inseparable . . . ;" and "let us admit right off that our objective [in foreign economic policy] has as its background the needs and interests of the people of the United States." Kolko and Kolko, p. 21. What is most telling about these examples is not that they have been shamelessly torn out of the context of Byrnes' and Clayton's statements, but that a historian could seriously attribute any significance to such meaningless clichés.

For a more detailed examination of some logical and methodological difficulties we can focus on one of the most recent revisionist histories by Kolko and Kolko.[108] A few of the more glaring problems have already been discussed; for example, that of an allegedly monolithic elite in which the president had to manufacture or magnify international crises in order to gain congressional support for his policies.

Consistency in judging events or adducing motives is not a virtue of this work. One need not be an apologist for the authoritarian Rhee government in Seoul to question the treatment of the Korean War. "The North Korean army moved across the 38th parallel about 4:00 A.M. on June 25, but one must not make too much of this as a causal fact. . . ."[109] The "real questions," it turns out, are of a different order; "most important," for example, was the ability of the North Korean armies to advance rapidly into the South, allegedly owing to a plan by Syngman Rhee to gain American military support. But the subsequent movement of South Korean, American, and other United Nations forces into the North is a different matter. Whereas the North Koreans wished to unite with the South along democratic lines (excluding only Rhee's clique), the latter planned to establish a unified police state.[110]

American policy on postwar reconstruction provides another example. Hostile intentions were adduced from Washington's failure to grant certain loans to the Soviet Union. But a $400 million credit, granted six months after the end of the war in Europe, is attributed in part to cupidity rather than generosity; the United States did not want to be saddled with useless Lend Lease materials.[111]

The treatment of Soviet policy making also reveals some difficulties. After asserting that "general theories of the motives of Soviet conduct . . . are still contrived,"[112] the Kolkos consistently argue from a general theory of their own, often preceding major conclusions with such phrases as "It is safe to say that. . . ." That theory postulates a cautious, conservative, and highly pragmatic Russian leadership, concerned with little more than security in Europe and reconstruction of industry and morale at home. A coup bringing the Communists to power in Czechoslovakia might seem to call into question the theory of Soviet conservatism, as might repression of the East German workers' uprising in 1953. Not so. After homage is paid to legitimate Soviet concerns about its security, a large part of the responsibility for both Soviet actions is shifted to Washington.[113]

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[106] Kolko, *Politics,* pp. 104–106. By his wording Kolko suggests that perhaps the Soviets were not guilty. British documents released on July 4, 1972 reveal that during the war London had excellent evidence that the massacre was perpetrated by the Russians rather than the Nazis. For the sake of Allied unity, the British covered up the facts at the time. The number of Poles killed varies according to the source. Kolko reports the figure as 10,000.

[107] Kolko, *Politics,* pp. 115–119. It may seem unfair to attach too much importance to these examples, but the Polish question was important to the development of Anglo-American relations with the U.S.S.R. Britain had entered World War II in honor of its commitment to support Poland against a Nazi attack, and the Polish community in the United States is not a negligible political factor.

More generally, these examples might seem to raise questions about the quality of historiography in the entire study.

[108] Kolko and Kolko.

[109] *Ibid.,* p. 578.

[110] *Ibid.,* pp. 575, 599.

[111] *Ibid.,* p. 39. On the role of American aid to Russia, compare Thomas G. Patterson, "The Abortive American Loan to Russia and the Origins of the Cold War, 1943–1946," *Journal of American History,* 56 (June, 1969), 70–92; and George C. Herring, Jr., "Lend-Lease to Russia and the Origins of the Cold War, 1944–1945," *Journal of American History,* 56 (June, 1969), 93–114.

[112] Kolko and Kolko, p. 55.

[113] *Ibid.,* pp. 397–398, 692. Nor did the Soviets or Czech Communists engage in any untoward activities against the other political parties: "For the Soviet Union, the Czech bourgeois parties conveniently committed political suicide." Needless to say, Jan Masaryk's

In other respects also the treatment of Russian leadership varies to meet the needs of the Kolkos' theory. The Soviets are absolved from complicity in the Korean invasion because the Kremlin realized that an international crisis would merely help Truman mobilize Congress. That is, Stalin and his colleagues acted on an allegedly accurate and sophisticated appreciation of American politics. Yet elsewhere the Kremlin leadership is portrayed as naive and uninformed about America for believing in the existence of powerful "restraints staying the imperialists' hands."[114]

An important deficiency in this work is that the rules of evidence appear to change whenever convenient. Americans are chided for "a myopia which took the Jacobin rhetoric of the Western European Communist parties at its face value."[115] Yet the same authors are not reticent about accepting as authoritative the rhetoric of Soviet "peace offensives" as well as some of the least sophisticated assertions of midwestern bankers or business magazines.[116] And, if the data become too troublesome, there are other expedients for sustaining a theory in its virginal state—for example, disregarding the evidence. Views expressed by John Foster Dulles, including those put forth as a private citizen, are accepted as authoritative when they sustain a particular line of reasoning. But Dulles also espoused some positions that scarcely support the thesis of a monolithic American elite. In *War or Peace*, a widely-read book written within months of the Communist victory in China, Dulles argued for recognition of the new Mao regime. That fact is conveniently overlooked, even in a discussion of the allegedly uniform stand taken by American leaders on the China issue.[117] If Dulles's view were important on some issues—a quite reasonable premise—why not on others? No explanation is offered.

Methodological practices of the radical right are no less suspect. In countless treatises these writers explain that the Soviets are skilled psychological warriors who will use any lie that might prove useful to demoralize the enemy—not only in propaganda barrages, but going to the extent of deliberately falsifying economic data to mislead the West.[118] Yet when a Soviet broadcast claims that Mao Tse-tung had killed 26.4 million Chinese during the period 1949–1965, it is accepted uncritically as prime evidence of a reign of terror in China.[119] No doubt Mao's bloody purges have taken a vast number of lives—perhaps the figure of 26.4 million is accurate—but we are not even told why, on this occasion the Soviets are presumed to be telling the truth rather than engaging in psychological warfare against the Mao regime. This is but one example of what might generously be called "careless" scholarship. Misleading use of statistics, *argumenta ad hominem*,[120] and other methodological aberrations also abound in this literature.

Not the least methodological deficiency of the radical literature is the loose employment of terminology to bootleg biases into the discussion and to set the terms of debate. "Totalitarianism," "imperialism," "repression," "fascism," and the like are used as a matter of course to describe the enemy or its allies. Consider the metamorphosis of the term "imperialism," a classic case of Humpty-Dumpty's answer to Alice's query about elastic words. The first step is to assert that imperialism need not entail occupation or formal control; informal influence may be exercised through economic power. Fair enough. Next, virtually any international economic undertaking is equated with imperialism. This too is fair if it is demonstrated empirically that the activities in question are causally linked to their putative consequences, and if the concept is then applied uniformly to the activities of all nations. Finally, the term is used as if all of its components were present in all situations. Whether or not occupation and formal control exist in a given situation, the term is applied as if they did—but only to the activities of the enemy. By the time the process is complete, the Point Four program or American investment in Canada

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death is also attributed to suicide. Kolko and Kolko, pp. 396, 398.

On rare occasions the Kolkos seem to criticize the domestic policies of Communist nations as "autocratic and exploitive"; but even this criticism is tempered: "One cannot accuse the Communist of betraying the working class, for such policies and priorities were perfectly consistent with bolshevik ideology." Kolko and Kolko, p. 401. One might use a similar line of reasoning to excuse Hitler. Liquidation of Jews and the invasion of Russia were perfectly consistent with Nazi ideology.

[114] Kolko and Kolko, pp. 585, 670.

[115] *Ibid.*, p. 35.

[116] *Ibid.*, pp. 35, 490.

[117] *Ibid.*, p. 560.

[118] Strausz-Hupé et al., *Protracted Conflict*, p. 72. This school places great emphasis on Soviet use of Pavlov and Freud to create psychological disturbances in the West. Strausz-Hupé et al., p. 23, and the Appendix.

[119] Possony, "The Chinese Communist Cauldron," p. 792.

[120] The Evans book is a classic in the art of *ad hominem* reasoning. On the radical left, the Goff-Locker analysis of the Dominican intervention is equally dependent on "evidence" adduced by this method; see especially p. 281.

are indistinguishable from the Soviet occupation of Czechoslovakia.[121]

Finally, events are often described in terms that bias the subsequent discussion. For example, the North Korean invasion of South Korea is labeled as a "civil war."[122] Had Bonn invaded East Germany with the help of British or American military supplies, would the term "civil war" be equally acceptable to the same authors?

Given such manifest difficulties, what future have these theories? Even critics of the radical explanations sometimes claim that they serve as a useful counterbalance to more conventional theories of international politics.[123] But the premise that the truth must lie somewhere in between is likely to be valid only at a very superficial level. Anyone who did not already appreciate the fact might learn from the radical right that Soviet foreign policy has not always been a benign international influence, or that Communist totalitarianism bears little resemblance to an egalitarian utopia. Radical left writings are a useful reminder that in matters of foreign affairs the United States is scarcely the innocent babe-in-the-woods sometimes celebrated by the Babbitts, elementary civics texts, and even in some of the standard works on the cold war. At a more fundamental level, however, the contributions of radical explanations for either explanatory or normative theories are open to question. Certainly there are deficiencies in conventional scholarship on foreign policy or international politics, and in the implicit or explicit theoretical premises on which they are based. Each critic will come up with his preferred list, but some frequently cited ones include: nationalistic or ideological biases, insufficient theoretical rigor, invisible criteria of evidence or inference, inappropriate or inadequate methodologies, and false criteria of relevance in selecting research problems. Are these the kinds of deficiencies that are likely to be remedied by adopting, wholly or in part, the Manichean vision of political life that is so pervasive in the radical explanations? By incorporating theories that are irrefutable, owing less to their power and elegance than to their elasticity? By confronting data with methodologies that are rarely visible, much less reproducible, and that seem more useful for maintaining the pristine purity of preferred theories than for confronting all of the evidence in a relatively even-handed manner?

Irrespective of the answers to these questions, it seems a safe prediction that the radical theories will continue to have a following. They are likely to be abandoned by few of their staunchest proponents; true believers do not readily give in to heresies, especially when their paradigms can be shaped at will into an impregnable defense against any evidence that might call them into question. History can be molded into more than a single pattern; those who would reconstruct the past to meet present ideological commitments will not find it hard to do so, especially when they shun the rigorous methodologies that might require them to confront a wider range of evidence than merely that which supports their brief. No less important is the absence of archival materials relating to the foreign policies of the Soviet Union and China. This void in the record of post-1945 diplomatic history greatly facilitates the task of the radical right and left. It permits those of the right to portray Soviet leadership and policy making as a monolithic conspiracy against the non-Communist world.[124] Radicals of the left, not having to confront primary evidence about Chinese or Soviet foreign policy, have greater latitude in assuming that any distasteful event bears a "made in Washington" label.

Nor can one deny certain appeals that will ensure these explanations some following. Some will continue to find them attractive because they provide a ready-made set of normative principles, as well as sustenance for the hope that "if we can just do X, we shall be on the threshold of a new and happier era of history." Moreover, they promise order, clarity, and comprehensiveness at precisely the time when many other students of international politics have concluded that the search for an all-encompassing paradigm is at present premature—that empirical and theoretical efforts are perhaps better directed toward forming middle-range "islands of theory" that may subsequently be linked.[125]

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[121] John W. Rollins, "The Anti-Imperialist and Twentieth Century American Foreign Policy," *Studies on the Left*, 3 (No. 1, 1962), 22; and Resnick. See also, Thomas J. McCormick, "Commentaries," *Studies on the Left*, 3 (No. 1, 1962), pp. 28–33.

[122] Resnick; and Kolko and Kolko, p. 570.

[123] See, for example, Hans J. Morgenthau's review of Kolko, *Politics of War*, in *New York Review of Books*, July 10, 1969, p. 10 ff.

[124] The absence of archival materials has not, however, prevented most American Sovietologists from moving toward much more sophisticated models of Soviet politics. See, for example, William Welsh, *American Images of Soviet Foreign Policy* (New Haven: Yale University Press, 1971). For a skillful and imaginative effort to reconstruct the Soviet foreign policy process, see Allison.

[125] For a further development of this point, see K. J. Holsti, "Retreat from Utopia: International Relations Theory, 1945–1970," *Canadian Journal of Political Science*, 4 (June, 1971), 165–177; and Charles A.

Yet however much radical explanations are likely to remain in vogue, history has a way of mocking those who would attempt to define it as a struggle between the legions of virtue and the forces of darkness. A few very recent events suggest a similar fate for those we have considered. Richard Nixon is perhaps the only American president who has, at times, seemed to accept some premises of the protracted conflict school. So it is surely a delightful irony of history that Nixon has staked his political future and his place in history on the détente so feared and abhorred by the radical right. The president's opponent in the 1972 election was asking precisely some of the questions about American foreign policy that Kolko and others have claimed lie outside the bounds permitted by the monolithic American establishment. All the while, Chinese officials are reported to have expressed concern that the post-Vietnam American withdrawal from Asia will leave a dangerous power vacuum which the Kremlin may exploit to China's disadvantage, and that cutbacks of troops in Europe may permit the Soviets to deploy more of their military strength on the Chinese frontier. One could go on for pages citing events that will require considerable ingenuity and increasingly transparent sophistry to fit into the theories we have been considering. This is not to argue that history is best viewed as merely a sequence of random events that defy understanding. It is only to suggest that those seeking explanation or prescriptive guidelines rather than indictment will find little of value in the remarkably similar theories of the radical right and radical left.

McClelland, "On the Fourth Wave: Past and Future in the Study of International Systems," in *The Analysis of International Politics*, ed. James N. Rosenau, Vincent Davis, and Maurice East (New York: Free Press, 1972), pp. 15–40.

The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science*

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The first scholarly codifier of political data was Aristotle, who classified the constitutions of the ancient Greek world. By ironic coincidence he tutored a princeling named Alexander who shortly reduced his teacher's findings to the status of historical knowledge, by the expedient of dissolving and reforming all political entities within reach of his armies. The empire of Alexander the Great succumbed to a grievous lack of political institutionalization; but before it did the conqueror founded a city in his name wherein flourished a school, also in his name, which was thought to be the greatest of the ancient world. The Alexandrian school was famed for its scholars' accomplishments in gathering and systematizing vast quantities of information about the nature and ways of men, beasts, and the lands and seas wherein they dwelled. Little of that knowledge survived. The libraries of Alexandria were destroyed, partly by Julius Caesar's armies, later and more thoroughly by Christians who feared the corrupting influences of pagan thought. But no matter now, because the spirit of the Alexandrians is alive and flourishing in American social science.

Comparing the Handbooks

One of the more striking developments in political science during the past twelve years has been the compilation of large bodies of systematically recorded information which reflect, and are designed to further, the research interests of scholars in comparative politics and international relations. The publication of four handbooks summarizing new collections of "politimetric" data in the past two years provides an occasion for a preliminary assessment.[1] The focus of these volumes, and this review, is on macropolitical data: systematic information, usually but not always in quantified form, on properties and interactions of political entities. Most of the variables in these compilations refer to nations, some to subnational groups, war, or the international system.

The handbooks have various features in common. Lest there be any initial misunderstanding, none are designed primarily as references of the *Political Handbook of the World* or *United Nations Yearbook* genre, which serve mainly to provide current information on specific countries. Nor are they part of the effort to develop measures of the national social and political "product" for domestic policy purposes, though some of their contents contribute to that objective. The politimetric handbooks are concerned with presenting comparable data on theoretically useful variables for purposes of generating descriptive generalizations and hypothesis testing. That they also serve as repositories of case-specific information for the more casual user is only a side payment. A second common trait is their self-conscious concern for data quality control. The data are almost always compiled or coded from secondary sources, which gives rise to anxieties about accuracy and comparability. The compilers respond by resorting to cross-checking against multiple sources, double coding, and sometimes defensive codicils. A third common

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* *Cross-Polity Time-Series Data.* Assembled by Arthur S. Banks. (Cambridge, Mass.: The MIT Press, 1971. Pp. xxiv, 300. $30.00.)

Black Africa: A Comparative Handbook. By Donald G. Morrison, Robert C. Mitchell, John N. Paden, Hugh M. Stevenson, and others. (New York: The Free Press, 1972. Pp. xxvii, 483. $29.95.)

The Wages of War, 1816–1965: A Statistical Handbook. By J. David Singer and Melvin Small. (New York: John Wiley, 1972. Pp. xii, 419. $15.95.)

World Handbook of Political and Social Indicators, 2nd ed. by Charles Lewis Taylor, Michael C. Hudson, and others. (New Haven: Yale University Press, 1972. Pp. xiv, 443. $15.00.)

[1] Several other major collections of macropolitical information have been the subject of various research articles and monographs but have not been published in their entirety. The variables of R. J. Rummel's "Dimensionality of Nations" (DON) project are similar to those of the *World Handbook II.* They are described and analyzed, but not tabled, in R. J. Rummel, *The Dimensions of Nations* (Beverly Hills: Sage Publications, 1972). Two other important data collections are Kenneth Janda's "Comparative Political Parties Project" archives at Northwestern University and William Flanigan and Edwin Fogelman's archive of time-series data on the properties of democratic states from 1800 to 1960, at the University of Minnesota.

Table 1. Types of Indicators in the Data Handbooks

	Cross-Sectional Data		Longitudinal Data	
	World Handbook, 2nd ed.	*Black Africa*	*Cross-Polity*	*Wages of War*
No. of Indicators:[a]	114	169	102	168[b]
Percentages by Type:[a]				
Political	*38%*	*40%*	*36%*	*99%*
Structures	16	12	11	5
Processes	8	7	12	—
Economy	8	18	5	—
Conflict	6	3	9	94
Sociocultural	*26%*	*35%*	*26%*	—
Subgroup characteristics	6	12	—	
Education, literacy	11	14	11	
Communications, language	8	7	15	
Other	2	3	1	
Economic	*22%*	*24%*	*25%*	—
Productivity, consumption	18	20	18	
Other	4	4	8	
Demographic and Ecological	*12%*	*9%*	*13%*	*1%*
Other	*3%*	—	—	—
Other Traits of Indicators, Percentages				
Denote intrasocietal distribution of properties	13%	13%	10%	—
Denote intersocietal relations	17%	24%	7%	92%
Provide time-dependent comparisons[c]	18%	21%	100%	67%
Summarize coded information[d]	24%	25%	30%	99%

[a] My count of number of indicators does not necessarily agree with number of tables or variables claimed in the handbooks; I counted each differently-weighted version of every indicator for the purpose of this comparison. Percentages add to more than 100 because a few indicators are classified under two different major headings.

[b] Variables include properties of the international system, wars, and nations.

[c] "Cross-sectional" indicators of rates of change across five years or more, plus indicators on which at least two time-points are given, five or more years apart.

[d] Indicators constructed by scholars using data obtained by documentary content analysis or judgmental coding techniques. Most of these were developed by the compilers of the handbooks; a few were taken from the literature.

against multiple sources, double coding, and sometimes defensive codicils. A third common feature of the handbooks is their "institutionalized" origin. Each is the product of a relatively large-scale (for political science) research project or center, and each reflects the collaborative efforts of a number of scholars, as many as eight per volume. All were substantially funded, either by private foundations (*Black Africa*), government agencies (*World Handbook* and *Cross-Polity*), or both (*Wages of War*). Only cross-national survey research projects consume more research resources in political science than these studies. And paralleling the published data of these volumes are banks of additional data and documentation, plus computer tapes and codebooks through which full data sets are made available to other researchers.[2] So much for the basics; what of the products?

The kinds of indicators presented in these books are analyzed in the accompanying table. Explicitly "political" variables are most numerous, but only slightly more so than sociocultural ones. The more familiar aggregate data indicators of population characteristics and economic productivity comprise about a third of their contents, with the exception of *Wages*

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[2] The data sets for all of the four handbooks reviewed here are available, with relevant documentation, from the Inter-University Consortium for Political Research at Ann Arbor, Michigan.

of War. These comparisons are misleading in one important respect, though. The central concern of all these handbooks is political, and the research effort given to constructing the political indicators well outweighs the investment in the nonpolitical ones. Note, in the bottom part of the table, that one-fourth or more of the indicators in each handbook are based on coded information. These include counts of conflict events and diplomatic representatives; measures of the severity, magnitude and intensity of war; indices of political party size and fractionalization; categorization of properties of legislatures and executives; and measures of intrasocietal cultural diversity. With the exception of the last, which are especially numerous in *Black Africa,* almost all these researcher-generated indicators denote variables which are central to political analysis. The selection of nonpolitical variables, moreover, is either implicitly or explicitly guided by prevailing professional notions about the kinds of nonpolitical variables worth treating as independent or control variables when studying the political. There is some data opportunism, such as the reporting in *Black Africa* of 23 education indices, and in *Cross-Polity* of 15 internal communications series, mainly because—one suspects—"the data were there." But on the whole these books are designed primarily to provide reliable data for the analysis of "political" questions, and only incidentally do they indulge in or contribute to data fetishism.

World Handbook II. The second edition of the *World Handbook* is substantially different from and in many respects better than its predecessor published in 1964. Political entities included number 136, about the same as the first edition, but the missing-data component is reduced to 15 per cent. Some social and economic indices which proved of marginal utility to political researchers were dropped and a good many more added. The new indicators are of three types. External relations of countries receive much more attention, including measures of diplomatic representation and membership in international organization, foreign aid, and the extent of trade dominance by a single foreign partner. Distributional properties of systems are somewhat better represented, for example, in new indices of ethnic and linguistic fractionalization, party fractionalization, and a new Gini index of sectoral income distribution.

The most substantial departure is a spate of new political indices. The compilers have broken the self-imposed "aggregate data" constraints of the first edition and report a large set of indicators constructed from coded data. Some are simple, such as the age of national institutions and judgments about electoral irregularity. The single greatest addition of this sort is the inclusion of ten measures of "political protest and executive change," each reported annually for 20 years, 1948 to 1967, and each based on painstaking coding of multiple sources. Three of the indicators are counts of "civil strife" events: demonstrations, riots, and armed attacks. One conflict property is included, "deaths from domestic violence." Four other indices signify numbers of executive changes of various sorts. The two most innovative indicators denote numbers of government sanctions and external interventions. The data are displayed in tables and also in time-series graphs, one for each of 136 political entities.

The discussion and documentation of the variables in the new *World Handbook* is exceptionally good. A careful rationale is offered for almost every indicator, showing how each is related to prevailing conceptual interests in political science, describing how each was constructed, and suggesting its uses and limitations. The reliability (accuracy and comparability) of each indicator is carefully evaluated, and some are listed with accompanying error or bias estimates for each datum. Sources are specified for every indicator but one, and are exceedingly numerous. The effort is all the more impressive when it is realized that the published data and documentation represent only the top of an iceberg, with the bulk of the iceberg available on call from the Inter-University Consortium for Political Research. Consider the 10 political protest and change indicators: there are 8 additional indicators available, plus codebooks, plus a magnetic-tape file summarizing each of the 57,268 journalistic reports from which all 18 event indicators were constructed. The authors have exercised one noteworthy restraint, however. Unlike the first edition of the *World Handbook,* this one reports almost no analyses. The authors' objective here "is to make certain kinds of data readily available and interpretable to persons (including ourselves) with hypotheses to test." They have succeeded admirably.

Black Africa. This is a strikingly handsome volume whose contents are as distinctive and substantial as its graphs, maps, and typography. It differs considerably from the preceding handbook, and not only in having an area focus. One distinctive feature is that it is organized around a substantive issue, namely the problems of national integration and political stability in 32 independent states of black Africa. Hence its indicators and other information are chosen with conceptual considerations in mind: they signify aspects or possible correlates of stability and integration. It is also more self-consciously interdisciplinary, because of its subject and because its senior authors include an economist and a sociologist as well as two political scientists. And more than the other handbooks, it is a compendium of current scholarly knowledge about and research approaches to comparative study of its subject. Like the *World Handbook II,* it includes extensive discussion and documentation of its indicators, though its coverage of the reliability of specific indices is less than one might wish. Unlike any other handbook evaluated here, it devotes nearly half its length to 32 country profiles. Each includes a discussion of ethnic and language patterns; characteristics of cities; a description of the changing strengths and status of each political party; analyses of the ethnicity of cabinet members at two points in time; a summary description of internal conflict events; and a substantial country bibliography. The final section of the book comprises several excellent essays on political integration and the methodological problems of comparative analysis which are worth serious attention by the comparativist, irrespective of his interest in Africa.

Quantitative indicators of the characteristics of African states are notoriously limited and unreliable. They are also criticized by some Africans for reflecting the academic and power interests of non-Africans more than of Africans. The number and nature of indicators in *Black Africa* afford some real revelations on both counts. The data are plentiful, the indicators are often innovative by comparison with those in the other handbooks, and many of them seem to the non-Africanist reviewer to get at African essences in a way that more universal indicators fail to do. The latter generalizations apply especially to the 25 indicators of cultural and ethnic pluralism. These include measures of the numerosity, relative similarity, and preponderance of languages, for example. It comes as a surprise to find that in 22 African countries, with 73 per cent of the total population of independent black Africa, half or more of the population speaks the dominant vernacular language; and that 88 per cent of Africans live in countries where the largest language serves as a *lingua franca.* Linguistic diversity, yes, but not to the extent of paralyzing incomprehensibility. Other indicators reflect adherence not only to Christian and Islamic sects but to the independent African churches. Perhaps the most interesting indices, and certainly the most difficult to construct, tap the degree of within-country variation in the cultural characteristics of its ethnic groups. Ethnic groups were identified, then coded from the anthropological literature on traits such as economic structure, basis of social stratification, family organization, and authority patterns. From these data seven sets of indicators were constructed, signifying the "typical" cultural characteristics of each country's ethnic groups and the extent of deviation therefrom. Indicators of pluralism are not immediately political, but it is the consensus of most Africans and Africanists that they have great impact on the political. *Black Africa* also reports many by-now-familiar political indicators, for example of government finance; characteristics of legislatures and cabinets; numbers of political parties from independence to 1969, and numbers banned; size of military and internal security forces; and indices of political instability based on event counts of the kinds used in the *World Handbook II.* Special attention is given to measures of interstate relations, including trade, aid, and diplomatic indicators of the kinds reported in the *World Handbook,* and also a few novel ones, such as the number of multinational corporations active in each African country.

One of the few limiting features of *Black Africa* is an almost complete lack of pre-independence data, except for that on education. Partly this reflects a relative paucity of data, but also it is a consequence of the authors' decision to focus on post-independence conditions and trends. With this single exception—the lack of historical depth—*Black Africa* provides a model well worth emulating for other regions. It also makes for pointed speculation about why specialists on other regions have failed to codify and consolidate more of their information.[3]

Cross-Polity Time-Series Data. This is both the most comprehensive and most deficient of the four new handbooks. It comprises annual series on 102 indicators for some 150 nominally different independent states, spanning up to 152 years. Its greatest virtue is its longitudinal coverage, but that coverage varies greatly among indicators. Area, total and urban population, and coded political characteristics are provided for all independent countries, where available, 1815 through 1966. Political economy, trade, education, and communications variables are tabled, depending on availability, from 1860 to 1966. Series on productivity, mass media, conflict events, and political party characteristics are the most shallow, 1946 to 1966 at maximum. The numbers of cases decrease sharply as one moves back in time, partly because of the smaller number of nation-states in the 19th and early 20th centuries, but equally because of the lack of any data for most countries outside the Atlantic basin. These comparisons suggest how great is the attenuation of cases: "Type of regime" is a four-category nominal scale, "civilian," "military-civilian," "military," "other"; numbers of countries coded in 1820 = 32, 1900 = 49, 1966 = 127. For government revenues, converted to U.S. dollars, representative numbers of cases are 39 in 1867 and 109 in 1966. These are data enough for many analyses, but complete cross-sections of cases will not be comparable over time.

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[3] One other such handbook was published after this review essay was prepared: Ellen Mickiewicz, ed., *Handbook of Soviet Social Science Data* (Riverside, N.J.: The Free Press, 1973). Its objectives and contents are similar to those of the handbooks under review here but it relies almost solely on data from official sources.

The handbook is deficient in ways that detract from what is otherwise a noteworthy accomplishment. Its aggregate-data indicators are mostly conventional rather than theory-relevant ones, and its coded variables make only gross discriminations, *vide* the above "regime-type" categories, or the rating of legislatures as "effective/partly effective/largely ineffective." Almost none of its indicators are weighted except by population, or combined so as better to approximate conceptual variables. For post-1860 data, the handbook relies substantially on a single annual source, *Statesman's Yearbook*, whose reliability is often questionable even in recent editions. All these characteristics can be rationalized, with some justification, on grounds of the paucity of sources for historical time-series data. What is less justifiable is the failure to discuss other sources—though the introduction suggests that others were evaluated and sometimes used. Indeed, the handbook makes far too brief reference to sources, coding procedures, or questions of reliability and comparability. By comparison with the other handbooks, this one is badly underdocumented and all too uncritical of its contents.

The deficiencies of the *Cross-Polity* handbook are due partly to the fact that it was prepared by a smaller staff working with much less financial support than the other handbooks. Its data are being revised and documented by Professor Banks, which leads one to hope that a second edition will be as much better compared with the first as is *World Handbook II* compared with its predecessor.

The Wages of War, 1816–1965. This handbook has a narrower focus than the other three, provides exceptionally detailed information on its subject, and incorporates much descriptive analysis. It is the first book-length publication of J. David Singer's "Correlates of War" project, whose dimensions should be generally familiar to all numerically minded political scientists on the basis of a dozen or more published articles which have reported on its procedures and results. The central purpose of the book is to report reliable, precisely defined measures of the properties of war in the international system between 1816 and 1965. It should prove useful, even for the cognoscenti, to summarize the basic distinctions made among types of nations, and wars, because it is these distinctions which guide the aggregation of data. First, four system levels are distinguished, each of which is a subset of the preceding:

1) The *international system* comprises all political entities with populations of 10,000+.
2) The *interstate system* comprises sovereign political entities with 500,000+ populations which had diplomatic recognition from two major powers.
3) The *central system*, 1816–1919, comprises nations which were "active and influential in European-centered diplomacy" (p. 22).
4) The *major power system* includes only the world powers, five to eight in number.

The nations in sets (2), (3), and (4) are listed in the book with a detailed description of the procedures by which they were identified. Second, two kinds of wars are identified:

1) *Intrasystemic wars* are those with total battle deaths of 1000 or more which involve two or more members of the interstate system; there were 50 such wars.
2) *Extra-systemic wars*—colonial, imperial, and internationalized civil wars—are those which involve only one member of the interstate system. They are analyzed only if that state experienced 1000+ battle deaths per year during the war; there were 43 such wars.

The first of three kinds of indicators made from this information treats wars as units of analysis. Historical sources were used to estimate the beginning and end of each war, its participants and the dates of their involvement, and the battle deaths suffered by each. The basic measures constructed from this information, by war and by participating nation, are (a) war duration in months, (b) magnitude in nation months, (c) battle deaths per nation month, (d) battle deaths per million population, and (e) battle deaths per 1000 pre-war armed forces. The authors compare their information with that obtained in three other studies of the properties of war: those of Lewis F. Richardson, Pitirim A. Sorokin, and Quincy Wright.[4] The Singer and Small data are *prima facie* superior for those wars that they have studied: the inclusion rules are precise, and reported· the sources are numerous, and also reported; and the data, and indicators, are more detailed.

The second set of indicators, and analyses, uses the same basic information on wars to assess over time the variation of war in the international system. Here the interstate system, or some part of it, is the unit of analysis. Annual indicators report "amounts" of war underway at each system level, using most of the measures identified above. Three kinds of descriptive questions are thoroughly explored through these data. One is whether there has been any long-run trend in the amount—number, magnitude, severity—of international war, and the answer is clearly "no": no trends upward *or* downward. There is, however, some evidence for periodicity: the amount of war over the past 150 years peaks every 20 years or so. The third question is whether there is seasonal or monthly variation in national decisions to enter, or leave, wars. The only significant observation to emerge from 18 relevant tables is that April and October are the cruelest months.

The last set of indicators and analyses focuses on the war experiences of nations and regions. The five nations with the greatest number of months of international war prove to be France, England, Turkey, Spain, and Russia; these nations, along with Germany, China, and Japan, have also suffered the greatest war casualties. The United States has an average rank of 12th on various "war experience" indicators. The last analytic chapter examines some of the contingencies of winning and losing. The victors suffered fewer casualties, on the average, than the vanquished, but there are too many exceptions to support a law-like generalization: in 14 of 49 interstate wars, the internationally recognized "winners" suffered more severely than the nominal losers. There is no clear threshold of relative casualty levels beyond which wars are terminated. Belligerency does tend to succeed, though: war initiators won 71 per cent of the interstate wars which had unambiguous outcomes.

If one accepts the value of assessing and comparing war properties—most political scientists surely do, some diplomatic historians may not—then this book offers virtually no grounds for criticism. Its information has been gathered and reported with painstaking attention to historical nuance and to scientific reproducibility. By the same token it is a cautious book, in the sense that it reports few judgmental indicators: the principal ones are those which use judgments about central and major power status, and winning/losing. There also may be quibbles about some of the guidelines used, either in general or as applied to specific cases, but the decision rules are so explicit that critics can determine quite exactly how different rules would affect the data and the analyses. The only criticism offered here is directed at the analyses: they are exhaustive when applied to different levels of the international system, to different war properties, and to different types of war, yet few test for differences in system properties or war from one period to another. So we seldom learn whether or not the descriptive generalizations offered are time-bound. We are given every means for finding out, though, and that is accomplishment enough for one book.

Some Critical Issues

The four books under review here represent the "second generation" of politimetric data handbooks. They succeed by half a generation their predecessors, the first edition of the *World Handbook* and Arthur S. Banks and Robert B. Textor's *A Cross-Polity Survey*.[5] These two seminal volumes attracted much critical attention, and their compilers made public confession—not least in the works themselves—about their inadequacies.[6] So it is fair to ask how well the inadequacies have been met, and whether others remain to be remedied.

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[4] Lewis F. Richardson, *Statistics of Deadly Quarrels.* (Pittsburgh: Boxwood Press, 1960) Pitirim A. Sorokin, *Social and Cultural Dynamics, Vol. 3: Fluctuations of Social Relationships, War and Revolution* (New York: American Book Co., 1937) and Quincy Wright, *A Study of War,* rev. ed. (Chicago: University of Chicago Press, 1965).

[5] Bruce M. Russett, Hayward R. Alker, Jr., Karl W. Deutsch, Harold D. Lasswell, and others, *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964); Arthur S. Banks and Robert B. Textor, *A Cross-Polity Survey* (Cambridge, Mass.: The MIT Press, 1963).

[6] A "review of reviews" is to be found in Charles L. Taylor, "Further Problems: A Consideration of

Some of the early criticism focused on fundamental paradigmatic issues. In their purest form, criticisms that nations or wars are inherently incomparable or that numeric data on them cannot be used for theoretically interesting purposes are unanswerable. The issues are real enough; but if the neo-Alexandrians had taken them wholly to heart, they would have switched straightway to case studies or to some other "honest labor"—which they quite evidently have not done. We also need not take seriously at this date the critics who rejected the aggregate data approach because they associated the use of numeric information with atheoretical modes of research such as factor analytic description or unguided searches through correlation matrices for significance. We now have enough examples of serious hypothesis-testing using comparative data, and sufficient intimations of the possibility of full-fledged econometric-type theories of political phenomena, to be assured that data handbooks are not Trojan horses concealing mindless empiricists who will steal out to murder theorists while they muse. The only likely fatalities are some of their theories.

One thrust of criticism was the relative lack of what one reviewer called "teleological form" to the selection of indicators. The essence of the objection was, Why these indicators and not others? and Do we not risk letting our conceptual interests be dictated by the availability of hard data? A rather convincing answer is provided by these handbooks. Two—*Black Africa* and *Wages of War*—are organized around specific conceptual and substantive interests which dictated the search for data and the construction of new indicators. In both instances, comparative indices were constructed from nonquantitative, historical and anthropological sources which have seldom if ever been thought of as sources of systematic data. The *World Handbook* was purged of measures which proved irrelevant to current theoretical interests, and the compilers of this and the *Cross-Polity* handbooks invested much effort in developing new aggregate and coded indicators to meet their perceptions of changing theoretical interests in the discipline. Consideration of the last decade's quantitative studies in international and comparative politics suggests that the availability of the early handbook data has had some influence on our substantive interests, but that much the greater influence has been the other way: the development of interest in new kinds of variables has time and again inspired large-scale and innovative efforts to devise new measures.

Another critical theme was the predominance of nation-level measures in the early handbooks and the relative paucity of information on political entities at other levels of analysis. The international system indicators of the *Wages of War* show what can be done at that level. There are also indicators of the properties of subnational groups, including new measures of the relative size and fractionalization of cultural and religious groups, and political parties. But there are no indicators yet of economic class characteristics, or of the properties of subnational political units, and very few indicators of the distribution of values. There is one new measure of the equality of income distribution in the *World Handbook II,* and an old one of the equality of land distribution; *Black Africa* indexes the ethnic representativeness of cabinets. But with these exceptions, inequality within nations is not treated.

The problem is not lack of data or suggestive indices, either. For example, the kind of power distribution index reported in *Black Africa,* which is based on ethnic representation in cabinets, could readily be applied to regional and communal-group representation in cabinets and legislatures of other nations. There are published indices of the size and identity of social groups subject to systematic discrimination, and of the size of trade unions.[7] Conventional aggregate data are available for a number of countries on the regional allocation of government expenditures, educational enrollments, internal security forces, and health and welfare services; many useful distributional properties could be indexed thereby. The problem has been a relative lack of investment in the measurement of inequalities within nations, and one hopes that the next generation of data handbooks will help remedy it.

A third critical issue was the lack of longitudinal data in the early handbooks, and the consequent reinforcement of cross-sectional re-

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Other Views, with a bibliography of reviews of the *World Handbook,"* in *Aggregate Data Analysis: Political and Social Indicators in Cross-National Research,* C. L. Taylor, ed. (Paris and The Hague: Mouton, for the International Social Science Council, 1968.) Self-conscious theoretical and critical discussions of many issues underlying the first edition of the *World Handbook* also are presented in Richard L. Merritt and Stein Rokkan, eds., *Comparing Nations* (New Haven: Yale University Press, 1966).

[7] Ted Gurr, *New Error-Compensated Measures for Comparing Nations: Some Correlates of Civil Violence* (Princeton: Center of International Studies, Princeton University, Research Monograph No. 25, 1966), pp. 67–110.

search designs. The difficulties of describing processes and making casual inferences from cross-sectional data are too widely known to bear repeating here. All of the new handbooks provide much better data for time-dependent comparisons than their predecessors did. Two of them, *Cross-Polity* and *Wages of War*, are primarily concerned with presenting time-series data, and the *World Handbook II* provides conflict and political change data in 20-year depth. *World Handbook II* and *Black Africa* also provide a fair number of indices recorded at two or three different time-points. So ample data are now available for time-series analysis and for cross-lagged panel comparisons, both of which should and will be used increasingly to test assertions that thus far have been assessed only or mainly in cross-sectional comparisons. Indeed the compilers of these handbooks have already begun to do so.[8]

A fourth kind of criticism was aimed at the reliability of national data, especially their "notorious imprecision." There are two kinds of answers to this question embedded in the new handbooks. One is the increased reliance of the handbook compilers on data of their own making. The use of increasingly precise coding instructions, multiple sources, and coding reliability checks all contribute to a sense that (a) the indices constructed from coded data are reasonably accurate, and (b) the nature and extent of their error, or indeterminancy, is approximately known. The second is the inclusion of source information and estimates of error and bias for aggregate-data indicators, all of which helps the user decide whether and how his results might be biased by possible error. The *World Handbook* and *Wages of War* are especially helpful in this regard; *Cross-Polity* is least so. There is also a third rejoinder to the criticism, to the effect that what is "notoriously imprecise" at the microlevel often makes little difference in macropolitical comparison. The error or imprecision in those indicators that can be checked almost always entails small to moderate percentages, not very large ones; and the kinds of inferences drawn in quantitative macropolitics are not likely to be affected by anything less than gross and systematic error. That in itself says something about the nature of macropolitical comparisons, and scholars who are concerned with absolutely precise description and subtle distinctions tend therefore to reject gross nation-level comparisons out of hand. But this is an issue of paradigmatic dispute, not a criticism which the handbooks can be asked to address more carefully than the best of them already do.

Another theme of early criticism was whether nations of vastly different size, wealth, complexity, culture, and autonomy are conceptually equivalent units of analysis and, similarly, whether nominally similar properties such as "voting turnout" and "number of protest demonstrations" have similar significances from one country to another. The answer of the comparativists is usually, "Yes, for our purposes," and a telling methodological argument to this effect is offered in *Black Africa*, pp. 392–395. Moreover, the testing of hypotheses often is replicated for different, relatively homogenous clusters of nations, and for different eras, as a means of testing assumptions about conceptual equivalence. The argument is that if relationships between variables prove similar among different groups of nations, and in different time periods, then the relations can be more confidently regarded as universals. If not, then the differentiating conditions need theoretical specification. What the critic regards as a weakness in cross-national or international comparison, the empirical comparativist uses as a means to specify theory more fully.

My only serious criticisms of the new data handbooks thus far have been the lack, in one of them, of sufficient attention to reliability, and a more pervasive failure to develop indicators of within-nation differences, especially inequalities. Two others need closer attention. The first is conceptual and has to do with a common feature of all the new handbooks: the measurement of internal and external conflict.

The procedures used to assess conflict for these handbooks have several innovative characteristics. Both *World Handbook II* and *Black Africa* report indices of government acts of suppression (sanctions and banning of parties, respectively) as well as anti-regime activities. All except *Cross-Polity* use multiple sources to increase the completeness of data. But there is a pervasive confusion between conflict events and conflict properties in all these handbooks except *Wages of War*. The essential research question is whether it is more appropriate to measure the *properties* of conflict, such as its duration, intensity, scale, and impact, either in single events or at the national level; or to concentrate on the *incidence* of conflict, i.e., on the number of distinguishable events which occur. Most theoretical work on international and internal conflict, and much empirical work as well, has been concerned with conflict properties: What are the relative magnitudes of different wars? How do conflict intensities and "instability" compare among countries? It is evident that the compilers of the handbooks want to provide indicators to deal with such problems; but repeatedly—and mistakenly, I think—they treat counts of conflict events as though they were conflict properties. The nature of event counts makes this a dubious procedure, for two reasons: first, the number of events is generally an artifact of reporters and reporting (counts are inherently unreliable), and second, most "events" have incommensurate properties (counts are invalid for the more common theoretical purposes). A "demonstration," for example, may involve 50 people or 100,000, may last for an hour or for two days, and may be ignored by all but the participants or may precipitate a governmental crisis. An act of "intervention," as treated in the *World Handbook,* may be a one-day sally across a border by ten armed guerrillas or an invasion by two army divisions. The authors of *World Handbook II* are aware of the difficulties posed by their event counts. It also is true that some kinds of conflict events, like assassinations and coups d'état, are more closely equivalent than others. But only *Wages of War* follows the reasonable precedent set 35 years ago by Pitirim Sorokin: to concentrate on assessing conflict properties.[9] The *World Handbook* and *Black Africa* both report one such property—deaths reported in conflict events. Otherwise they resort to such unsatisfactory expedients as arbitrary weighting of events depending on their type (*Black Africa*) and counting riots which continue for more than one day as separate events (*World Handbook II* and, apparently, *Cross-Polity*).

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[8] Representative published studies which examine temporal variations using data from these handbooks are Arthur S. Banks, "Correlates of Democratic Performance," *Comparative Politics*, 4 (January 1972), 217–230; Michael C. Hudson, *Conditions of Political Violence and Instability: A Preliminary Test of Three Hypotheses* (Beverly Hills: Sage Professional Papers in Comparative Politics, 01–005, 1970); and J. David Singer and Melvin Small, "Alliance Aggregation and the Onset of War 1815–1945," in *Quantitative International Politics: Insights and Evidence*, ed. J. D. Singer, (New York: The Free Press, 1968), pp. 247–286.

Event counts are not only inherently unreliable and incommensurate, but in most applications they subvert the purpose of making valid comparisons of relative conflict levels among systems. Brazil, with some 70 million people, had 95 riots between 1948 and 1967; Portugal with 9 million had 36. Which had proportionally more turmoil? We cannot say unless we know something about the relative magnitudes of those riots. Conflict deaths provide a more meaningful comparison: Brazil had 2.0 conflict deaths per million population during the two decades, Portugal had 2.9. But unfortunately the handbook compilers report only an aggregate of deaths, not cross-classified by type of conflict event. This is not to imply that body counts are the best or only measurable conflict properties. But they are the only ones reported in *World Handbook* and *Black Africa.* What some others might be are suggested by *Wages of War:* duration, number and identity of participants, magnitude, outcomes. The handbook compilers do not always recognize the difference between event counts and conflict properties, either. In *Black Africa,* surprise is expressed that its event counts, unweighted for population, correlate only weakly with others' population-weighted measures of conflict properties.[10]

A second ground for critical evaluation of the handbooks is whether and how they reflect statist and status quo tendencies within the political science guild, and in the larger political world. It would be absurd to criticize the handbooks as "statist" for including measures of governmental structures and performance, inasmuch as the state has always been the core subject of political inquiry. One still has the uneasy sense that the handbooks present measures of system properties and processes that reflect mainly the "view from the top," whether the "top" is in Washington or Lagos.[11] There are two grounds for taking this issue seriously: the origin of some of the data included in the handbooks, and the evidence of what is *not* in them.

Government agencies are the original sources of, or control the dissemination of, a substantial part of the data in all the handbooks, including most demographic, economic, sociocultural, and public expenditure measures. Governments also influence the interpretation and flow of news about internal and external conflict. Some data cannot easily be distorted because there are too many means of verification, e.g., as in foreign trade estimates, membership in international organizations, and the occurrence of successful coups and revolutions. And there are few conceivable reasons for falsifying estimates of, say, urban population, or the size of the national assembly. But where opportunity and self-interest for manipulation of information are both present, data should be judged more skeptically than they now are. This is true especially of "development" indicators in the Third World, including education, health, productivity, and communications data; of governmental budget estimates in sensitive policy areas, especially military and social service categories; and of data about internal and external conflict, especially from countries in which the press is controlled. The handbook compilers maintain that they report the best cross-verified data available, and that they have little evidence of deliberate distortion of national data. There is no reason to doubt the first claim; but as for the second, the compilers are seldom in a position to know. Neither are the users. The suggestion is not that we stop using the data because some may be fudged. The point is that data from governmental sources are not immaculately conceived, and we ought to stop acting as though they were.[12]

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[9] Sorokin, *Social and Cultural Dynamics.*

[10] See pp. 125–126.

[11] Such criticisms have been made especially pointedly by scholars with a Marxist perspective, for example, Robin Jenkins, *Exploitation: The World Power Structure and the Inequality of Nations* (London: MacGibbon and Kee, 1970). The criticism is directed against data with a governmental origin and against its aggregation at the national level. Nonetheless the Marxists use the data they criticize to support their own arguments about inequalities, as does Jenkins and also Pierre Jalée, *The Pillage of the Third World* (New York: Monthly Review Press, 1968). Moreover, worldwide collections of aggregate political and social data also have been published by Marxist-oriented institutions, including a two-volume handbook issued in 1962 by the Czechoslovakian Academy of Sciences (in Czech) and another published several years earlier by the Hungarian Academy of Sciences (in German).

The second issue, which is the handbooks' collective errors of omission, seems to me both more serious and more susceptible to remedy than suspicions about unclean data. What is largely missing is the "view from the bottom" of the system, or even from the middle. We noted the relative lack of measures of subsystem properties, and especially the scarcity of inequality measures. There are aggregate measures of the extent of education, availability of health services, and distribution of income, but nothing to suggest which groups or regions are at the short end of the stick. The measurement of system change is almost exclusively focused on the incidence of structural and elite changes, not on which groups lose or gain, nor on changes in what the political system does or to whom. The measurement of conflict and intervention has a bland anonymity: there is no attempt except in *Wages of War* to develop indices which suggest the identity, motives, or relative statuses of parties to conflict. No doubt such information is difficult to compile and aggregate, but surely it is not absolutely intractable. The handbooks contain enough examples of ingenious indicator construction to suggest that lack of effort rather than sheer difficulty has been the culprit. Given the current interests of political scientists, we have every reason to ask and expect that more attention be given to such questions in the future.

By the same token one cannot reasonably accuse the neo-Alexandrians of being subservient to the interests of political and economic elites. They have, for example, been more diligent and even-handed in gathering information on international conflict and economic imperialism, political violence and repression, than any governmental agency. Their relative lack of attention to the seamy underside of social life in the nations they study is more likely a reflection of the historical interests of the political science guild than a deliberate statist bias. Those interests have expanded greatly in the past few years, but the laborious nature of fact-gathering means that the neo-Alexandrians sometimes lag a few years behind current intellectual fashions. Their attempts to catch up will be risky. To the extent that they succeed in indexing the nastier aspects of political and social systems, they risk constriction of their funds and access to domestic and foreign elites. To the extent that they fail, they will be attacked for irrelevance and worse by scholar-activists who are already profoundly suspicious of grand and numerical approaches to questions of political order, conflict, and change. So the data archivists face much the same uncertainties as their predecessors in ancient Alexandria: they are subject to the whims of Caesar and to the displeasure of the ideologists. Nonetheless they have already provided us with a vast treasury of codified political and social knowledge.

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[12] The *World Handbook II* includes a "press freedom" index which might be used in comparative analyses of data from government sources as a "control" variable—literally and statistically. It also could be used to evaluate the reliability of some kinds of measures, and to weight others—e.g., the conflict indicators. R. J. Rummel, in *Dimensions of Nations*, has used a press censorship index to test for effects of biases in some of his analyses but his example has not been much acted upon or elaborated by others.

BOOK REVIEWS

The Scientific Management of Society. By V. G. Afanasyev. (Moscow: Progress Publishers, 1971. Pp. 285. $3.00.)

V. G. Afanasyev has written an intriguing book about control, very much in the Russian sense of the term The book is interesting not only for its substantive content but also because of the political authority with which the man speaks.

Academician Afanasyev, author of numerous books on cybernetic management and frequent contributor to Pravda, is one of those, establishment scientists we refer to so pejoratively in this country. This fact is extremely critical in dealing with *The Scientific Management of Society,* his latest work. It is critical because Afanasyev's successes and failures in coming to grips with the managerial problems which confront the Soviet state are very much the successes and failures of the regime at large. In this respect, any review of Afanasyev is ultimately a review of the present state of Soviet organizational thinking, a discipline which is struggling to reconcile the sacred tenets of Marxist-Leninist science, if science can ever be "sacred," with the realities of the swiftly approaching postindustrial era.

Afanasyev's analysis begins with the concept of an integral system. Society is viewed as an integral self-controlling system. Men are seen to be both the objects and subjects of control, controlled by the system yet capable of controlling the system. This control is most effectively exercised by the individual's understanding the functioning of the system and thus being able to reshape the system itself. Historical materialism, an understanding of the "objective" laws of human development, is seen as the type of insight required of the conscious historical actor. "Conscious" action is distinguished from "spontaneous" attempts at control by its relative success and dynamic character. The conceptual framework which the author manipulates so adroitly is obviously that of conventional Marxist analysis. Afanasyev uses this analytical machinery to criticize control procedures in capitalist society.

These procedures are viewed as inadequate, essentially because of the structural characteristics of the system. Capitalist production represents the triumph of spontaneity over consciousness, that is, a situation in which the spontaneous market mechanism triumphs over the "obvious" advantages of centralized planning.

An understanding of the "objective" laws of history allows socialist society to confront ". . . the complexity and diversity of social life . . ." with a more optimal form of control (p. 82). Socialist control is distinguished from capitalist control in several important respects. According to Afanasyev it involves: elimination of the market mechanism by the Communist party and other conscious institutions, control directed toward the building of communism, and total rather than limited control of society. Only in the Socialist state do the object and subject of control merge in a social arrangement where the people themselves control and are controlled.

It is in the service of this State that Afanasyev would develop an integrated approach to the problems of scientific management. These problems are carefully identified as areas for additional study. They include: the precise application of Marxist-Leninist theory to the varied management experiences of the CPSU; examination of the structural-functional concept of society; a more exact interpretation of the relationship of subjective to objective factors in administration; an analysis of individual problems confronting the Soviet state; a reexamination of the role of the subject of control, that is, the party, the state, etc.; and an examination of the relationship of socialist society to its natural, and all too often hostile, environment. In coming to grips with this environment Afanasyev's analysis demonstrates its unique strengths and glaring weaknesses.

The strength of this work is the author's willingness to deal with information contrary to the argument he would like to make. Afanasyev questions the applicability of time worn Marxist concepts: is sensitive to qualitative-quantitative distinctions rather than preoccupied with the "inevitable" union of quality and quantity. He is prepared to confront the problem of overcentralization in dealing with democratic centralism, and considers the uncontrollability of certain economic elements in a state where control is ostensibly complete.

Like the more sterling of his counterparts in the West, Afanasyev is preoccupied with perhaps the most critical question confronting organizational theorists today, that of developing meaningful criteria of optimality, ways that we can critically evaluate the success or failure of a procedure. It is in struggling toward a theory of optimal control that he is handicapped by the conceptual baggage of Marxist-Leninist "science."

One cannot help but be suspicious of a "science" couched in the language of ascription. A science which is sacred, a science blessed by inflexible guardians of its great wisdom is less than blessed. Given his preconceived notions about the way the world must be, Afanasyev is prevented from confronting the realities of capitalist industrial success. One therefore regards with suspicion the staggeringly favorable results he attributes to the socialist economic reform. Additional problems surrounding the notion of an integral system include the standard arguments leveled at structural-functionalists, too complex to examine here.

At the risk of being inferentially arrogant, it is safe to suggest that the problems and successes of *The Scientific Management of Society* are very much those of Soviet decision makers. The uniquely interesting thing about *The Scientific Management of Society* is not so much what Afanasyev has to say about control, but what his treatment of control says about Afanasyev and the society he represents.

ARTHUR TRUEGER
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Gifts and Poison: The Politics of Reputation. Edited by F. G. Bailey. (New York: Schocken Books, 1971. Pp. vii, 318. $9.50.)

The topic of this book is micropolitics, especially the micropolitics of equality, envy, and to a lesser extent honor in the specific Latin sense; all the studies, evidently, were conducted under a general program, planned by the editor. The reports tell us, especially, how a rough equality is maintained and how innovations are resisted in a series of Italian, French, Austrian, and Spanish villages—all of them poised between the threat of depopulation and the perils and possibilities of modernization. Essentially, the emphasis on tearing down anybody who sets himself up or apart leads functionally to some degree of social equality and hampers most proposals for innovation. At several points, the insight into envy as the great equalizer provides us with insights of a sort which would do credit to Ibsen or Edwin Arlington Robinson (see especially Chapters 11 and 13). Any political scientist who has used Banfield's *The Moral Basis of a Backward Society* will have to consult Bailey and his students to see how they extend and modify Banfield's propositions.

The emphasis on "good fortune and skill in cheating" as characteristic of upward social mobility may seem characteristic even of the U.S.A.—but reading the book as a whole, one gets a sense of a difference—on this point so great as to mean that the kind of social life is different.

At several places, inevitably the effect of the micropolitical pattern upon elective politics is traced (see particularly Chapters 3, 4, 12). The major omission in the book is a failure to tell us how broadly the approach may be applicable; substantively, my own reading, participation in politics in several parts of the U.S.A., and community study in Watertown, Massachusetts would lead me to query the validity of the findings for much of this country (and for some developed areas of Western Europe). The one section of North America to which I can make a direct translation is, significantly, the economically marginal, depressed "South Shore" part of Nova Scotia around Shelburne. I strongly suspect, too, that had the writers themselves spelled out more clearly, as I wish they had, the implications of their historical perspectives, it would have become more evident to the reader that the particular pattern of equality/envy found today may have been less characteristic of these same communities in the days before the railroad. However, very large parts of the world (e.g., many Arab communities) may perhaps be analyzed in similar fashion. These caveats are important, simply because the book will be read by people unaccustomed to the tendency of anthropologists to restrict generalizations to the community under study, unless they so state.

One of the major extensions or potential extensions is the application of the sort of micropolitical analysis here sketched to cabinets, congresses, committees, councils, and so on (although presumably the substantive findings might show that envy was far less important than something else in maintaining equality in any given case). The assertion is in itself plausible; as a student of Congress and a participant in political campaigns myself, I believe it, but the text in no way adds to its credibility. It remains for these authors (or others) to study such bodies with these analytic "tools."

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Journey to America. By Alexis de Tocqueville. Translated by George Lawrence. Edited by J. P. Mayer. (New York: Anchor Books, 1971. Pp. 424. $2.50, paper.)

This book is a compilation—with revisions, corrections and explanatory notes—of all the notebooks Tocqueville kept during his journey through the United States and Canada. They contain transcriptions of his many interviews as well as his immediate impressions and reflections upon what he saw and heard. Apart from being fascinating reading, this volume has special value for both the student of Tocqueville's thought and the student of early nineteenth-century America. The Tocqueville scholar will find it interesting that many of the themes developed in *Democracy in America* can be traced to their origins in these notebooks. The editor has unfortunately been rather timid in his notes referring the reader to *Democracy*. Apart from his introduction, where he does give several interesting examples, the notes for the most part refer to George Wilson Pierson's study, *Tocqueville and Beaumont in America*. To mention two among the many examples that could have been cited, there is an interview with Lieber (p. 43) which contains the germ of the chapter on political associations in *Democracy*; there are also the interviews with Gray, senator from Massachusetts, (p. 45) and Jaret Sparks, famous editor and historian (pp. 47–48) which anticipate the opening pages of the chapter on the American system of townships.

The student of early nineteenth-century America will find especially valuable the recording of the thoughts of Americans who had been asked by Tocqueville to explain themselves and their country. As the interviews make abundantly clear, many Americans believed it to be a fact, which they were witnessing, that democracy entailed the disappearance of great men of political ability and that universal suffrage and frequent elections led to political instability. This, of course, was the period of Jacksonian democracy. It is particularly fascinating to read how they defined the underlying state of affairs which gave rise to American democracy and what they foresaw for the future. There are numerous discussions about the Negroes and the Indians, the economic effects of slavery, the advance of civilization and the future of these two races. Concerning the Negroes, speculations run the gamut from the establishing of a black nation in some portion of the American territory (p. 37) to total annihilation of the black population (p. 60).

Tocqueville's descriptions of the American scene present the reader with a panorama of diversity. "In nothing does America exemplify a system of that uniformity that delights the superficial and metaphysical minds of our age" (p. 288). Contrasts are everywhere: between the sociopolitical life of the North and the South; between the original states and the new states, and even more dramatically, the territories; and finally between the "civilized" land and the wilderness. Accentuating these contrasts is the ever-present change which characterizes America, and the underlying restlessness of the American. Poinsett, former American ambassador to Mexico, says of the American: "It would seem that the habit of changing place, of turning things upside down, of cutting, of destroying, has become a necessity of his existence" (p. 82). Tocqueville found this to be true not only of the Americans who were engaged in "clearing the wilderness" but also of the economic life of Americans on the eastern seaboard. He was very much taken by their risking what they had in hope of a better future, and their constant changing of occupations. It is with astonishment that he says, "Men do not have habits here, and what they see under their eyes prevents them from forming any . . ." (p. 185). And yet, a balance is struck. The American does emerge from the pages of these notebooks. He is the man who "confidently takes charge of the future" (p. 186). The interviews and his observations, seemingly of their own accord, converge to reveal "the only bond that unites the different parts of this huge body. *Interest*" (p. 218). It is here that all the chaotic aspects of American democracy find their ordering principle.

There are many interesting entries whose suggestive implications remain largely unexplored in *Democracy*. For example, his discussion of the importance of roads and canals possesses a richness which is lost in the rather perfunctory treatment found in his later work. Such passages as the following did not survive the transition: "The road almost always comes before those whom it is intended to serve, but it encourages them to move; we have several times seen main roads opened up literally in the middle of the wilderness" (p. 286). Whatever intellectual breadth these notebooks may lack compared to *Democracy* they more than make up in such synoptic and captivating images of a main road opened up in the middle of the wilderness.

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Economic and Social Development: A Process of Social Learning. By Edgar S. Dunn, Jr. (Baltimore: The Johns Hopkins Press, 1971. Pp. 327. $10.00.)

Economist Dunn's objective is the development of a model of social and economic change. Dunn dismisses the utility of equilibrium models for purposes of planning change and presents the idea of a stochastic "creative learning" model. He claims that only a learning system, in contrast to a machine system, can "invent a new mode of behavior independent of the influence of exogenous systems" (p. 30). Dr. Dunn argues that there is an enlightening analogy between the currently accepted theory of biological evolution and the process of creative social learning as he understands it.

Dunn has clearly reviewed a great deal of biological literature in preparation for this volume but it is not clear that biological exposition is especially useful to either the planner or the political scientist because it is used as an analogy to develop a speculative metaphor. The references to motivational states and collective strivings of evolving organisms are misleading. Phylogenetic adaptations to the environment are not individually motivated. Genetic variations occur and are selected by the environment in the sense that some survive to reproduce themselves while others do not. This is not to argue that Dunn has misunderstood the process but rather that this exposition tends to be slightly teleological (p. 56). His statement that "phylogenesis leads to homogeneous behavior of members of a species" (p. 86) is not set in sufficient context to be carefully evaluated.

Dunn presents the biological analogy in the following form. "Phylogenesis operating as a learning system produces a learning subsystem —the learning organism—that operates at a different level and by more direct means" (p. 76). Behavioral innovations which "promote a convergence between environment and social goals are reinforced" (p. 81). "Those ideas that generate maladaptive behavior (in the sense that they do not generate goal convergence) can be sifted from the idea pool without necessarily resulting in extinction of the biological organism" (p. 84).

According to Dunn, normal social reorganization and behavior change only involve modification of "goals or criteria that are instrumental to the fulfillment of primary system goals" (p. 140). Under extraordinary conditions, however, these primary system goals may not support behavior change deemed "essential" (p. 141). Then there may occur a shift in primary goals or a major reorganization of the social entity, which Dunn views as analogous to the kind of scientific paradigm shift discussed by T. S. Kuhn (*The Structure of Scientific Revolutions*, Chicago: U. of Chicago Press, 1964). Using the phylogenetic analogy, Dunn views normal problem solving in terms of adaptive specialization, and extraordinary problem solving in terms of adaptive generalization (p. 147). He claims that historically, major system reorganization (paradigm shift) may occur in two ways: first, through the cumulative effect of normal endogenous system learning which undermines the "operating context" of the system; second, through exogenous environmental change so great that system survival is threatened (p. 214). He asserts that social systems may be organized *to promote* major system reorganization and suggests that "higher order" values be used as guides in the evaluation of changes in social behavior (p. 175). These values are derived from his analysis of the evolutionary process and from selected psychological theories which emphasize "spontaneity" as contrasted with need gratification. He sees the key to such system-based social reorganization to be system-embedded dialogic communication (p. 231) because he believes it is the medium of both experimental social system transformation (p. 231) and the fulfillment of the spontaneous growth motives he values.

Finally, Dunn argues that his social learning metaphor is to become the new paradigm of the social sciences. It is explicitly value-laden (p. 242) and its goal is the enhancement of the process of human development (p. 244). Dunn's derivation of human values from genetically determined needs such as the need for oxygen illustrates a good deal of insight but probably could be improved with a more precise definition of "values." His goal of enhancing human development sounds admirable but is conceptually fuzzy. The absence of precise definitions of terms also limits the persuasiveness of other arguments. The discussion of "determined" systems suffers from this weakness. His argument that human behavior is not fully determined (p. 233) needs elaboration to make it clear. His statement that a stochastic process will not support "positive prediction" is ambiguous (pp. 70–72).

Dunn's statement that social science is self-analysis (p. 237) is not convincing. Social scientists can treat some realms of social behavior from an external vantage point. Dunn is correct that the social scientist operates within the social system if he uses the broadest possible definition of "social system." But the social scientist is not necessarily a part of a more narrowly defined social system under analysis. It is not clear that Dunn can prove his point by simple reference to the Heisenberg Principle (pp. 233, 267). The social scientist may be functionally exogenous to the system of behavior under analysis.

It is surprising that Dunn makes no reference to the work of Talcott Parsons (e.g., *The Social System,* N.Y.: Free Press of Glencoe, 1951) in his elaborate discussion of social systems and system boundaries. Although Parsons is often viewed as emphasizing equilibrium while Dunn is centrally concerned with change, reference to Parsonian literature and particularly to the problems of levels of analysis would have been useful. The concept of individual socialization, for example, is usually viewed in terms of behavior maintenance at the social system level, but it can also be viewed as behavior change at the individual level of analysis.

Modeling social change as a machine system open to information is less compatible with Dunn's view of man than the "creative social learning" model he advocates. His anthropocentric model fails to adequately define creativity, the nature of exogenous vs. endogenous variables, and "levels of analysis." Courses in the methodology and philosophy of social science may use parts of this volume to illustrate these difficulties.

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Politics as Symbolic Action: Mass Arousal and Quiescence. By Murray Edelman. (Chicago: Markham Publishing Co., 1971. Pp. 188. $3.95, paper.)

Politics as Symbolic Action is a call to political science to redirect its attention away from the formal actions of government and politics toward a concern with the forces that shape public perceptions, emotions, and behaviors, which Edelman considers a good deal less stable and persistent than is commonly assumed.

> Either we ask how actions and developments do or might change people's beliefs and behaviors or we ask how people with postulated fixed perceptions and cognitions will behave or can be coerced or repressed. To direct one's attention to one or the other questions is to predetermine both the forms of public policy that seem desirable and the kinds of empirical observations that seem pertinent to explanation and understanding (p. 173).

Government and politics are less involved in carrying out the popular will than in shaping and determining it. While Edelman makes an excellent case for the study of political symbolism, the case for Edelman's own theoretical position is not particularly clear nor without its ambiguities.

To undertake what Edelman views as a necessary redirection in empirical research, he advises paying greater attention to "the work of the symbolic interactionists, the gestaltists, and the phenomenologists, and the work of some novelists, film makers, and social critics" (p. 173). We need to pay greater attention to the "deep structure" of political myth and ritual, and be less concerned with surface events. In suggesting how this might be done, Edelman develops or introduces a number of concepts which continue the theoretical directions he began in *The Symbolic Uses of Politics.* Where his earlier study identifies the importance of symbols in politics, and in producing political quiescence, in particular, this book provides a far more elaborate discussion of symbol, myth, and ritual, and gives attention to the process of arousal as well as quiescence. In doing this Edelman introduces the concepts of ritualization and escalation as two alternative forms taken by political conflicts; the book develops these two processes in detail.

In both ritualization and escalation, concrete events, interests, and anxieties are translated into socially-cued myths, which are general, abstract, and fundamentally not empirically verifiable (although empirically identifiable). In the case of ritualization, the myths associated with prior events are not threatening and consequently evoke no anxiety and no action on the part of the public. On the contrary, the public is reassured that nothing is fundamentally wrong, which permits groups involved in a conflict to work for tangible payoffs benefiting themselves. Escalation, in contrast, invokes myths, which rather than relieving anxieties, translates them into action. Powerful myths, such as national interest, allow government or other political actors to transform ambiguous information into anxieties which require action to alleviate them. A crucial political process is the development and restructuring of expectations, rather than working for their satisfaction. The study of political symbolism requires that greater attention be placed on the shaping of political cognition. It requires distinguishing between information and meaning. The former involves complexity and disorder, while the latter is associated with order and structure and tied to selective perception, distortion and social structure.

The overall organization of the book, even after several readings, is not particularly clear, and the turgid style makes for difficult reading. The two crucial concepts—escalation and ritualization—are not well clarified. Although escalation is said to produce the involvement of the mass public in political action, including urban violence, the chapter dealing most completely with escalation considers international relations in which escalations involve governments and the mass public generally acquiesces to the wishes of its government. Thus, the full thrust of Edelman's arguments is ambiguous.

One consistent theme not systematically developed is Edelman's criticism of survey research and other empirical methods associated with contemporary political science which he says lead us to see individual perceptions as more fixed and stable than they actually are. Ironically, to support his position he cites articles like Converse's article on mass belief systems, which are themselves based on survey data. While we need to be more aware of the categories social researchers impose on data, we must recognize that this is a more general problem, not limited to the survey method.

Edelman gives us a great deal more advice about what we ought to study empirically than indications of how we should do it. While he is clearly concerned with the development of empirically-based propositions, he does no systematic hypothesis testing. Even more important, he shows no concern for how such hypothesis testing might be carried out, a considerable problem in the study of political symbolism. This omission is unfortunate because, despite all its faults, the book continues to develop a number of theoretically important, and often counterintuitive, ideas that would be well worth examining systematically. For example, the author hypothesizes that violence is more likely to occur in the absence of organization than in its presence; that beliefs which are held most unanimously in a polity are easier to alter than those over which there is a division; that public controversy helps participants accept outcomes which deviate from their beliefs about the optimum policy; that crowds and violence generate leaders and not the reverse; that ideology is a result, not a cause, of international conflicts; that behaviors lead to the development of perceptions which support them, rather than the opposite.

Edelman's work clearly has called the attention of political scientists to the study of political symbolism. It is an important area, and its theoretical payoffs in understanding the linkage between public attitudes and elite behaviors are potentially very large. At the same time, my enthusiasm for Edelman's redirection of our conceptual orientations is frustrated by his ambiguity on the one hand and by his unconcern with systematic examination of his hypotheses on the other. Edelman's major contribution, then, is in showing the value of the study of political symbolism, while leaving the task of further and more careful statement of conceptual relationships and empirical research to others.

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Plato's Phaedo, Plato's Phaedrus, Plato's Philebus (three volumes). Translated, with commentary, by R. Hackforth. (Cambridge: Cambridge University Press, 1972. Pp. 200, 172, 143. $5.50 each.)

These are reissues of the late Prof. Hackforth's translations with introduction, notes and commentary. Each dialogue is broken up into numerous small sections which are prefaced or followed by the sometimes lengthy running commentary—an infelicitous arrangement. The commentary obtrudes, interrupts the dramatic flow, and distracts the reader from Plato's words. The editor would have been better advised to have placed the entire commentary at the end of each dialogue, thus enabling the reader to confront Plato directly with his judgment as little pre-formed by others' interpretations or suppositions as possible. It is after the student has grappled with Plato for himself that a commentary can be of benefit, providing a provocative counter or challenge to the student's own interpretation.

Prof. Hackforth's reading of the Greek is competent. Although he has made use of Robin's superior apparatus, he has wisely chosen to follow Burnet's readings with but few exceptions. These rare deviations are often sound and usually justifiable. A discussion of the numerous questions that can be raised about Hackforth's readings of the *Phaedrus,* and a critical comparison with other editions of that difficult text, will be found in W. J. Verdenius's review article (*Mnemosyne,* 4th series, vol. 8, pp. 265–289).

As a translator, Hackforth is moved by sensitivities Plato did not share and consequently bowdlerizes the text: he veils sexual imagery and joking, tones down emotional terms of address, and mutes or simply omits cursing and blasphemy. Given the subject matter of these three dialogues, such "corrections" can be peculiarly misleading. But for the reader alerted to these changes, the translations will prove helpful, for they are superior to the others available and in some ways rival the out-of-print Carey translations (Bohn edition). Hackforth strives to render Plato in straightforward, clear, and unpretentious language. This attempt sometimes creates a rather colorless tone, and leads to a tendency to smooth over those passages in which Plato has characters speak in strange or unexpected phrases. But one is far too grateful for the overall literalness to wish to complain very much about these failings.

In fact, Hackforth's translations, while by no means completely reliable, represent a very hopeful change in the quality of modern translations of classical works. Most contemporary translators of Plato anachronistically continue a genre which makes considerable sacrifice of literalness for the sake of spirit, easy readability, and charm. Such translation had its place in an earlier world where the scholarly and reading public could be assumed to know enough Greek to be able easily to check the original for accuracy wherever a precise understanding was needed. This is no longer that world. But until classical scholars abandon nostalgia and face up to the need for a division of labor within the modern scholarly and intellectual universe, we shall, for the most part, continue to receive from their hands translations which are graceful, even mellifluous, but unliteral, inaccurate, and misleading. One can only hope that these translations help to start a movement in a different direction.

The introductions and commentary will prove of less benefit than the translations. It perhaps suffices to say that Prof. Hackforth presents a very conventional version of the approach to Plato that has dominated British scholarship for about a century. According to this view—an outgrowth of the late nineteenth-century preoccupation with problems of genesis, biography, and history—the dialogues do not constitute an ordered dramatic whole intended to convey a single if complex philosophic and political teaching. They are rather best understood as the record of Plato's "development." Prof. Hackforth's concern is with dating the dialogues and tracing the shifts undergone by Plato in what is seen as a rather volatile intellectual career: his break with the teachings of Socrates, his repeated changes in key doctrines of his own, his feuds and friendships with contemporaries, etc. Buried under this antiquarianism are all the dramatic, political, and philosophical questions, perplexing or wonderful, with which Plato confronts the reader. Since Hackforth moves in the deep rut of a long-established British tradition, he never once feels compelled to marvel at the fact that neither Plato nor Aristotle suggests that the dialogues are to be read and interpreted in this fashion.

Indeed, the very strange dialogue form in which Plato chose to present his political and philosophical reflections apparently holds no wonders at all for Hackforth. Without pausing to discuss the matter, he reveals his certainty that the dialogues constitute for the most part a charming if quaint backdrop for Plato's direct and candid presentation of either the historical Socrates' views or his own. He maintains this certainty while translating and commenting on the *Phaedrus,* the dialogue where Plato has Socrates speak most clearly about the playful character of all good writing ("a written discourse about anything must contain much playfulness and cannot be worth taking seriously": 277e). In his comment on this crucial passage Hackforth reminds the reader of Plato's own even more negative judgment:

> I certainly have composed no work disclosing the serious objects of my studies, nor shall I ever do so. . . . No man would ever think of writing about serious things for the general public. . . . (*Seventh Letter,* 341c–344c)

Yet he then goes on to affirm solemnly that "it is surely plain that Plato writes for the most part in a vein of deep seriousness, with a sincerity at times passionate" (p. 163).

Hackforth never reflects deeply on the problem of written philosophic communication—the dangers of atrophy of memory and independent thought, of "doctrinairism" and philosophic "schools." Consequently he is never led to raise and face the possibility that Plato's dialogue form may have been intended to capture the advantages of written philosophic discourse while minimizing its dangers. He never looks at the dialogues in the way Aristotle seems to recommend (*Poetics,* 1447b): as plays, dramas, in which none of the characters, not even Socrates (not to mention Plato), ever speaks directly, and which require as much interpretive thought and independent effort on the part of the reader as do the plays of Euripides or Aristophanes.

But rather than belabor in detail Hackforth's approach to Plato, it will be more helpful to indicate why these particular dialogues should be of interest to political scientists concerned with Plato's political thought. For the reader may well wonder about the political relevance of works dealing with the nature of death and the correct posture toward it (the *Phaedo*), the nature of erotic persuasion or rhetoric (the *Phaedrus*), and the nature of private pleasure (the *Philebus:* cf. Hackforth, p. 4). Doubtless it is true that the longer and more weighty political dialogues (the *Republic,* the *Laws,* the *Statesman*) should be the primary objects of our attention. Yet from these political dialogues it is evident that for Plato the core of political science is political psychology; for Plato, even more than for Aristotle or Montesquieu, it seems, the study of each political system should focus on the human type, the characteristic personality, fostered by the system (see esp. *Republic* 544 ff.). And this study of the variety of human types depends ultimately on an understanding of the human psyche in general, its structure and its needs. Of course Plato's political psychology conceives of man not as an "ego" or a "self" but as a "political being." But at the same time, Plato recognized elements and needs of the human psyche which are unalterably private and can never be politicized. These are above all the fear of death, erotic attachments, and the concern with bodily pleasure and pain. The political dialogues tend necessarily to underemphasize these apolitical themes; hence Plato's full understanding of man's political nature and behavior requires a separate inquiry into what is apolitical or even antipolitical in human nature. This Plato undertakes in the *Phaedo,* the *Phaedrus,* and the *Philebus.* The integration and comparison of these three dialogues with the more familiar political dialogues will therefore correct our onesided or distorted notion of Plato's teaching about the psyche, the teaching which is at the foundation of his political science.

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The Human Basis of the Polity: A Psychological Study of Political Men. By Jeanne E. Knutson (Chicago: Aldine-Atherton, Inc., 1972. Pp. 360. $12.95.)

Although political theorists dating back to the Greeks speculated on "man's nature," the modern study of personality and political behavior really began with Harold Lasswell's *Psychopathology and Politics* published in 1930. Since then, social scientists have generated a multitude of psychoanalytically inspired psychobiographies and empirical studies of political leaders, activists, and ordinary citizens.

Unfortunately, the political scientist confronting this body of literature can only come away puzzled. Radically different interpretive schemes based on the ideas of a wide range of psychological theorists—Freud, Adler, Erikson, Rogers, Maslow, Cattell, Skinner—seem promiscuously applied. The empirical literature is filled with a seemingly unlimited list of personality traits, each correlating modestly (but always significantly) with some political attitude. The interrelationships between many frequently employed concepts (authoritarianism, anomy, efficacy, self-esteem, rigidity, etc.) remain largely unexplored, and their conceptual meaning is further obscured (if not made totally unfathomable) by a proliferation of different scales commonly purporting to measure the identical trait.

Jeanne Knutson directly addresses these problems of theoretical and empirical proliferation. Holding Ph.D. degrees in both political science and psychology, Professor Knutson, looks at the interface between the two disciplines and observes that

> . . . with the theoretical frameworks and empirical evidence at hand, the time has come for a synthesis of past research and the integrated analysis of personality and political behavior in terms that are both *broader* (to include all personality types rather than isolated, superficial traits or single personality syndromes) and *deeper* (to include a study of the causal conditions and need requirements of predispositions to various types of politically relevant behavior so that *individual traits are seen as integrated manifestations of deep-seated motivational states*) (p. 11, emphasis in text).

Knutson attempts such a synthesis by organizing previous theories and research within the personality paradigm of Abraham Maslow. Maslow sees man striving to satisfy basic needs, with these needs hierarchically progressing from the elementary (physiological, safety) to social (affection, esteem), and finally, to self-actualization. If fundamental needs are thwarted, crippling personality dynamics result. Psychoanalytic theorists have done much to illuminate how such deprivational need-states arise and manifest themselves in pathological behavior. But, says Maslow, these theorists usually ignore man's positive side, for when basic needs are met, mentally healthy individuals function effectively. In contrast to persons motivated by psychic deprivations, the self-actualized are more creative, undogmatic, tolerant and secure, and socially and politically active.

After sketching out Maslow's basic ideas, Knutson draws together a great deal of past research from psychology, sociology, and political science touching on the relationship between personality and politics. She also orders a host of concepts, such as authoritarianism, alienation, prejudice, anxiety, and dogmatism, into the conceptual lattice work provided by the Maslow perspective.

Finally, Knutson demonstrates her thesis in an original study of nearly five hundred men and women from diverse socioeconomic backgrounds. Classifying subjects into discrete need-levels, she tests a number of specific hypotheses mental health" against variables such as status generated earlier by running the "continuum of concern, alienation, dogmatism, citizen duty, political efficacy, participation, and leadership.

Knutson's book ought to draw considerable attention to a perspective largely ignored up to now by political scientists. "Old hands" and students will find her synthesis helpful in organizing previously fragmented theorizing and research. Her attention to motivations growing out of both deprivational and self-actualization needs is in tune with current tendencies in cognitive, social learning, and humanistic psychology to view man as more than an actor reacting blindly to largely unconscious needs.

Still, several problems remain. Methodologically, a psychometricially validated instrument for assessing need-levels and especially self-actualization is required if meaningful empirical testing is to take place. (Knutson's own study is seriously flawed by this measurement problem. She employs many familiar scales, indices, and items whose validity is questionable in terms of freedom from response set and social desirability biases, then constructs simple additive scales without testing for unidimensionality and construct validity, and finally defines "self-actualization" as the absence of high scores on several deprivational need indices.)

Considerable theoretical clarification is also needed. As with many stage theories, the ordering of stages and the reasons some stages cannot occur simultaneously require further elaboration. Hypotheses linking some psychic need to distinct political behaviors appear sometimes to be derived ad hoc rather than deduced from fundamental principles. For example, the need to enhance low self-esteem seems capable of explaining both withdrawal from threatening social situations and the throwing of oneself into political activism. And, although Knutson acknowledges that "personality" variables alone do not account for all behavior, if she were to specify the circumstances under which particular needs are evoked or inhibited, some theoretical ambiguities might be eliminated. Academic phychologists today are increasingly critical of the notion of general personality traits, and argue that one should look to the social environment as the *primary* determinant of behavior.

Still, Knutson's book provides a valuable overview of a rich body of literature drawn from several disciplines, and offers a conceptual structure which seems potentially capable of synthesizing diverse data and theories currently bewildering students of personality and politics.

IRA ROHTER

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The Ruling Class: Elementi di scienza politica. By Gaetano Mosca. (New York: McGraw-Hill Book Company, 1939. Pp. 514. $3.95, paper.)

Gaetano Mosca's classic, *Elementi di scienza politica,* originally published in 1896, with successive revisions until 1923, is here reprinted in paperback from the hardback translation first published by McGraw Hill in 1939. While the decision to make available a less expensive paperback edition deserves to be welcomed, it is unfortunate that the publishers did not commission someone (James Meisel, whose magisterial study of Mosca, *The Myth of the Ruling Class,* Ann Arbor, 1958, provides a much needed corrective of earlier impressions of the Sicilian's political thought, immediately comes to mind in this connection) to add an introduction more relevant to the concerns of contemporary students of politics than that composed in the 1930s by Arthur Livingston. It is further to be lamented that the publisher's description on the cover of the paperback edition erroneously states that Mosca's "conception of social forces and of the rulers who acquire power to control them served as a theoretical foundation for Italian fascism." Even if one accepts the arguable proposition that Fascism had a "theoretical" (as opposed to an ideological) foundation, it has never been convincingly demonstrated that Mosca's work served as a major intellectual basis for Fascist thought. On the contrary, Mosca himself, as Meisel shows, opposed Italian fascism and retired from the Senate in 1925 after courageously denouncing Mussolini's regime for destroying parliamentary democracy. As early as 1904 Mosca had offered a reasoned defense of the *practice* (as opposed to the rhetoric) of parliamentary democracy (Mosca, *Partiti e sindacati nella crisi del regime parlamentare,* Bari, Laterza, 1949 ed. pp. 334–335. For a general discussion of Mosca and elite theory see Germino, *Beyond Ideology,* New York, 1967 Chapter 6). To "link" Mosca to fascism in this way is, therefore, fundamentally to distort his teaching; yet one all too frequently comes across this kind of attribution by careless interpreters.

The heart of Mosca's political theory, is the concept of the *classe dirigente,* usually translated as "ruling class," although the Italian means literally "leading class" and "leading" sounds less "authoritarian" than "ruling." The term "class"—a political sociologist's nightmare —also has a static, fixed ring about it which is quite foreign to Mosca's view of the permeability of elites to accession from below in any but the most rigid and repressive society. For Mosca, as for Machiavelli before him, context was all, and his achievement as a political theorist consists in his extraordinary capacity for evoking the flavor and vitality of the concrete political struggle, coupled with his unrelenting drive to free himself and his readers from the self-serving illusions of the conventional wisdom and its typically mushy analytical categories. Despite frequent claims to the contrary, political theory is an *empirical* enterprise, concerned with illumining through critically clarified symbols man's participation with his fellows in the drama of existence. The differences between theorists (e.g., Aristotle vs. Plato, Machiavelli vs. Hegel, Mosca vs. Voegelin) have to do, not with whether one is "empirical" and the other "normative," but with the accent of their analysis. To speak metaphorically, if we conceive of political reality as an immense canvas, theorists may be divided into those whose gaze is concentrated on the foreground, with the larger existential background at the periphery of their observation, and those whose intellectual priorities are the reverse. Mosca is clearly a "foreground" theorist: the immediate political and electoral struggle in all its aliveness and vitality is clearly at the center of his concern.

Mosca is at his best when "demythologizing" current political clichés, and he can be devastating when he comes upon reductionist theories which seek to explain all in terms of "race" or economics. Ever puncturing self-righteousness, ethnocentricism, and complacency, he acidly reminds allegedly "superior" societies of their weaknesses. An example of his salubrious acerbity in this regard is his remark that the belief that all non-Western societies "—the Egyptian, the Babylonian, the ancient and modern Chinese—have been, and still are, uniformly stationary seems . . . to be due to nothing less than an optical illusion arising from the fact that we view them from so far away" (p. 36).

Mosca's concept of the "ruling class," "social forces," "juridical defense," and the "political formula" have become part of our cultural inheritance. Nothing would be served here in attempting briefly to recapitulate them, for their original exposition in all its complexity and subtlety continues to be worth reading and pondering. Nor has "elite theory" stood still since the days of Mosca and of his more verbose, pretentious, and less interesting fellow countryman, Pareto. Guido Dorso, Norberto Bobbio, and Giovanni Sartori (to speak only of Italian scholars) have quietly and perceptively made refinements, elaborations, and revisions to Mosca's famous theory about the predominance of elites. Mosca deserves continued study by political scientists not only for what he wrote but also because of the creative reflection which he helped to inspire.

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Aging and Society. By Matilda White Riley et al. (New York: Russell Sage Foundation; Vol. I: **An Inventory of Research Findings.** By M. W. Riley and Anne Foner, 1968. Pp. 636. $25.00; Vol. II, **Aging and the Professions.** By M. W. Riley, John W. Riley, Jr., and Marilyn Johnson, 1969. Pp. 410, $9.50; Vol. III: **A Sociology of Age Stratification.** By M. W. Riley, Marilyn Johnson, and Anne Foner, 1972. Pp. 652, $20.00.)

Students of politics have only occasionally turned to the age factor or the aging process to explain political behavior. In recent years, however, talk of generational conflict and an increasing awareness of rapid social change has heightened the sensitivity of political analysts to the differential impact of events and life stages on the behavior of cohorts. In the past five years, for example, at least a half dozen political science dissertations have focused on age, and most survey research reports now routinely attempt some statement about the cohort vs. age vs. period confound. Yet, it has been only with great searching that those interested in the broader implications of age and aging could review the widely scattered literature on aging, or survey the practical implications of the substantive topic of the aged. More important, understanding how aging might relate to existing clusters of social theory required familiarity with a range of studies in different subfields of psychology and sociology. The appearance of these volumes marks an important beginning toward closing just such gaps in cross-disciplinary research.

Volume I, a survey of the better empirical research, is organized into four parts: the social context, the organism, the personality, and the social roles linking society and personality; this is the basic reference to which the subsequent volumes are keyed. As a research inventory, it is one of the better such exercises in the social sciences. The authors are justifiably cautious about the quality of research, signaling to the reader the unusual difficulties of design and sampling with older populations, and warning that the bulk of findings are confined for the most part to the American context and to a relatively narrow time span in recent history.

Political scientists will be interested in the chapter on political roles, which is organized into a series of propositions on political participation, commitment, ideology, party identification, and leadership, derived from some 45 studies. Many of the original data are displayed in tabular or graphic form, while cautionary notes warn the reader of limitations on generalizability. The specialist might note the omission of a few studies, but in general, coverage is quite adequate.

The second volume treats substantive problems of the aged from the perspective of practitioners in such fields as social work, medicine, law, etc. The rationale is that such professionals will have the most immediate use for the empirical knowledge-base referred to in Volume I. Although the policy-oriented analyst might find this occupational approach inappropriate, the substantive areas with which the various professions deal are, in fact, among the major policy issues involving the elderly. Thus, the chapters on medicine, nursing and public health could be helpful for the policy analyst interested in health care delivery.

One of the major obstacles social scientists have found in utilizing research on the aging process, or even on the substantive area of the elderly, has been the underdeveloped state of theory relating these phenomena to other areas of social inquiry. Volume III is an important effort in this direction. The authors' theory of age stratification is similar to social stratification, but is based essentially on demographic change theory, and largely on the contributions of Norman Ryder. In fact, Ryder's 1964 essay on the concept of a population, in which age is identified as a central measure of time and therefore change, is reprinted in full as a chapter. Essentially, the age stratification model specifies a set of structural factors (age strata, age capacities, age roles, and age expectations and sanctions), which are related to a set of processual factors (cohort flow, aging, and socialization and allocation).

This model is elaborated in great detail in four separate chapters, which outline some of the major methodological problems and spell out some of its potential implications. One contribution of this methodological emphasis is a needed simplification of a number of strategies for coping with confounds in longitudinal data, worked out originally by developmental psychologists—although rapid progress in this area already outdates some of the suggestions made. Applications of the model are found in the half dozen contributed chapters on education (by Parsons and Platt), science (by Zuckerman and Merton), and on community, the workforce, and friendship. Anne Foner's chapter on the polity will be of interest to political scientists.

Those who wish to learn something about theories of the aging process, should know that Riley and her associates define aging rather narrowly: as mobility through age strata, i.e., simply passage through the life course. Thus, they sidestep the troublesome issues as to what aging is, or what is its relationship to development. This may come as a disappointment to gerontologists, but it may be a sensible, if provisional, way around problems that have up to now blocked theoretical progress in this field. Looking at aging as movement through time could become an important link between macroanalytic social change models of the demographers and the microanalytic individual change models of the developmental psychologists. It is to be hoped that a theory of age stratification will facilitate a much needed shift in political behavior research away from its current preoccupation with childhood and youth toward the virtually unknown territory of individual change throughout the entire life span.

It should be pointed out that many of the elements in the age stratification model were anticipated in the preceding decade by Bernice Neugarten and her associates (whose work on age grading, age status system change and age norms is conspicuously absent from the bibliographies). Nevertheless, Riley and her coworkers have synthesized these ideas and rendered them capable of integration with a variety of social theories. For this they deserve credit, and for this their work will undoubtedly be widely cited in the future.

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The Political Writings of Leibniz. Translated and edited with an introduction and notes by Patrick Riley. (Cambridge: Cambridge University Press, 1972. Pp. 206. $13.00.)

Professor Riley makes an unduly modest claim for the interest of Leibniz's political writings. Leibniz is the first modern conservative of a certain type. He made use of the doctrines of Plato, Aristotle, Roman law, and the scholastics in order to oppose the modern egalitarian doctrines of natural law and natural right. Yet this conservative, and apparently derivative, political thought is placed on a foundation that is novel—a metaphysics whose modernity is assured by its close connection with Leibnizian dynamics and cosmology. Of this attempt at an extraordinary fusion of old politics and new natural science—an attempt whose uniqueness was guaranteed by Leibniz's breadth of genius—Riley is far from sufficiently aware.

Included here are finished essays, fragments, *pièces d'occasion* and letters, mostly hitherto unavailable in English. They treat thematically of justice, ancient and modern natural law and right (one a sustained critique of Pufendorf), "the perfect prince," international law and international relations, and what was for Leibniz the overriding practical issue of the time—the threatening hegemony of the France of Louis XIV. This last gave occasion for Leibniz's *Most Christian War-God (Mars Christianissimus),* perhaps the most mordant, and surely the most scintillating, assault ever made by a philosopher on a contemporary monarch. Of the extent and complexion of the body of political writings from which the selection is made, no indication, unfortunately, is offered, nor is there any indication of the criteria of choice. It is sometimes impossible to learn whether a given title is Leibniz's or the editor's, and whether a particular writing was published, transmitted privately, or remained in a drawer.

Riley's introduction is mainly a systematic account of the Leibnizian philosophy that descends from God to the substances of the world and thence to human things. His procedure appears modeled on Leibnizian writings that begin with God, the hierarchic principle of metaphysics, as distinct from those writings that begin with substance, the comprehensive principle. It is fair to say that despite certain appearances, this procedure was recognized as impossible by Leibniz: from the nature of God it is not possible to derive the nature of substance, its perfections and imperfections. Neglect of this issue—or at least acceptance of the "descent"—seems to have led Riley into the notable error of attributing to Leibniz the view that evil is an eternal, uncreated essence in the divine mind, the source of evil in the world (pp. 9, 10, 16; cf. *Dis. on Meta.,* XXX). Interpreted exclusively as a descent from deity, Leibnizian natural theology comes to resemble medieval revealed theology. "The central question in Leibniz is: why did God create a *necessarily* imperfect universe?" (p. 16).

That a different procedure is required is shown by Riley's failure to explain, specify, or derive the Leibnizian meaning of "perfection" —the "thread" that binds together the system (p. 7). Leibniz's second and more usual type of procedure is to begin with the comprehensive principle, substance or monad (as in *New System, Monadology*) and ascend to the existence and nature of God as required. Within the substance doctrine, the Leibnizian sense of "perfection" can be gathered from such assertions as "perfection is to be placed in form"; monads as "entelechies" "have within themselves a certain perfection" (*Monad.* #18); and "substantial forms" or "entelechies" are "primitive forces," a concept often referred to writings on dynamics (e.g., *New System,* #1–3). When Riley does turn to the substance doctrine, his reports are defective. He says that "substances themselves are not material" but also quotes the discrepant Leibnizian statement, on the same page and without comment, that "a complete corporeal substance" is a compound of "form, soul, entelechy, primitive force" and "primary matter, solidity, resistance" (p. 12).

The political writings have within themselves the same attempt to synthesize seemingly alien elements as in the system as a whole. In one context Riley recognizes that this harmonization is more apparent than real. Leibniz offered a "Christian doctrine of sufficient imprecision to be agreeable to everyone" (p. 30), i.e., Catholics as well as Protestants. Prudence subordinated theology to the requirements of the peace of Christian Europe. Yet Leibniz's political horizon included the non-Christian East. When he endows the Augustinian term "City of God" with the new meaning of "the assembly of all minds" or all rational souls (*Monad.* #82–86), the apposition of Christian and infidel is "harmonized." Again, Leibniz would seem to reconcile Christian charity with modern secularism by "broadening" the sense of "charity" to include the use of technology for men's material well-being and comfort. It therefore becomes peculiarly important to know which of these "harmonizations" are prudential and which are theoretical.

One wishes that Riley had addressed himself more fully to this problem, of which three additional examples must suffice. Leibniz gives a formal definition of justice as "charity in conformity with wisdom" that appears to combine Christian and Platonic elements. The wisdom required is knowledge of the relative merit of the receipient of justice. Charity, it seems, becomes unjust without a high degree of knowledge, the wise man unjust without a benevolence apparently universal in extent; only further study would disclose whether the Platonic or Christian element, or neither, predominates. Leibniz gives what we may call a material account of justice that comprises the three senses of *Nic. Ethics* V, in terms modified by a somewhat procedural notion of justice, taken from Roman law. A justice that presupposes wisdom, in Platonic fashion, is not easily reconciled with the Aristotelian moral virtues, which do not. Leibniz's ultimate harmonic effort is perhaps the attempt to reinterpret ancient virtue hedonistically, and to relate pleasure to the feeling of perfection, and thus to reconcile the ancient scale of natural excellences with modern doctrines of substance and cosmology.

While Riley's interpretation has the limitations indicated, his introduction has the great merit, unusual in Anglo-Saxon study, of placing the question of human excellence and happiness in that central position in his philosophy which it surely held for Leibniz.

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The Mythology of the Secret Societies. By J. M. Roberts. (New York: Charles Scribner's Sons, 1972. Pp. 370. $17.50.)

The problem Roberts addresses in this book is of great concern to political scientists; what he has to say about it is only intermittently so.

Roberts demonstrates how a widely-accepted mythology grew up around secret societies such as that of the Freemasons in the years between 1750 and 1830. The mythology attributed all sorts of subversive and revolutionary intentions to these societies. The problem is treated as one of political psychology. Roberts's concern is to explain why people should have accepted so readily "mythologies" about these societies, despite the tortuous counterfactual reasoning required to make such interpretations of the societies' activities plausible.

Roberts argues convincingly in his conclusion (p. 349) that such mythologies must be understood as "responses to a need to master reality," an attempt by individuals to find a satisfactory explanation for large bewildering processes of historical change in terms readily understood by ordinary people. He suggests that irrational conspiracy theories of political events are especially likely to be produced among people who have a bias toward expecting that acts will have been caused by persons, and links this tendency (p. 356) to "the Christian cultural tradition which located moral responsibility in the individual."

Roberts also proposes the possibility that many men somehow need to view society in a paranoiac fashion, which is "intensified at moments of great stress." If this is true, he finally concludes, "when a civilisation rejects one sinister mythology, it gains no inoculation against another."

These are suggestive ideas, and the evidence Roberts provides in the course of his book for the mythology that specially concerns him is occasionally fascinating. But there are several important weaknesses in his general approach. Roberts indicates in his conclusion that irrational conspiracy theories have had supporters for a long time, and that they have considerable strength today; he hypothesizes that such theories respond to a basic need; yet he fails to show why he is warranted in offering such broad generalizations on the basis of close study of one particular era. A study of secret societies in these turbulent decades, taken as a contained phenomenon, is interesting in itself: the Illuminati in Bavaria and the Italian Carbonari, important precursors of later revolutionary groups, fall within his period. But this is not the same as a comparative study that convincingly supports major generalizations about conspiracy theories and mythologies. Roberts looks to one period for his evidence, but offers generalizations that must transcend any such limitation. To make these generalizations substantial and plausible, we need to know more about comparable mythologies in other contexts.

Roberts might well contend that generalizations offered on the basis of close study of one particular period can be tested and developed by others studying different groups. This is the value of his work for students of political psychology and mythology; but the value is reduced because his generalizations, though very broad and comprehensive, are relatively sparse and only thinly linked to his evidence. A great deal of material has been collected and reported, but the author does not keep it firmly subordinated to his own purposes. Too often the wealth of eccentric data and narrative material overwhelms the structure of his argument.

Roberts writes elegantly about nonsense and offers a mine of data for researchers interested in conspiracy theories and mythology. His suggestive generalizations may also be useful; but one is occasionally tempted to complain with Prince Hal that we have here "a ha'penny worth of bread to an intolerable deal of sack."

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Natural Law in Political Thought. By Paul E. Sigmund. (Cambridge, Mass.: Winthrop Publishers, Inc., 1971. Pp. x, 214. $6.95, cloth; $3.95, paper.)

At first glance this book seems to be an anthology of excerpts linked together by commentary, intended for elementary courses in political theory. In fact it is *nothing* of the sort. The passages drawn from classical and recent authors serve merely to illustrate Professor Sigmund's own text. They are at most extended footnotes to a very complex and highly scholarly account and defense of natural law. This is, moreover, a most lucid and balanced examination of the place of natural law thinking in political theory, past and present. The emphasis should be on "political," since, unlike his most distinguished predecessor, Alexander d'Entrèves, Professor Sigmund is not at all concerned with legal theory. That also accounts for some, though by no means all, of the differences between these two authors, both such ardent defenders of this tradition of thought. For Professor Sigmund the break between medieval natural law and modern natural rights theory seems quite insignificant, while to d'Entrèves, as to most continental historians, the discontinuity between the two is complete. Given the decisively anti-authoritarian and nonreligious character of the once revolutionary notion of natural rights, this is not an unreasonable position. Again, the emergence of the sovereign nation-state, with its legal positivism, seemed to older scholars, especially in the era of the two World Wars, to be the greatest challenge to natural law theory. Not so to Professor Sigmund, whose age is that of the Vatican Councils and of the revival of Augustinianism, among Protestant, and now also Catholic, theologians. The last, and in many ways most interesting passage, quoted here is taken from Gregory Baum O.S.A. His argument is that although it is obvious that men have moral aspirations, the word "law" does not describe them at all well. Moreover, without grace there can be no genuine Christian life, so the term "natural" is also unacceptable when applied to Christian moral striving. One might add that it is also inappropriate to Plato's vision, which demands a transcending of normal experience to a blinding philosophical insight that has nothing to do with rules. The point is significant because the remarks of the latest Augustinians bring us right back to the point of origins and to Professor Sigmund's valiant effort to reconcile the diverse strands of thought that make up the history of political theory.

Natural law is based on "a belief in an ultimate moral order in the universe in which contradictions are capable of being resolved and an ultimate harmony can prevail" (p. 183). Professor Sigmund shares this faith, and his book is an effort to justify it historically by showing that most of the great political theorists of the past have adhered to some form or aspect of natural law theory, even when they seemed to oppose it. Only Machiavelli and the tyrants, old and new are icily excluded. Hobbes, Hume, and at least the young Marx are all in. Hobbes had some idea of right reason apart from society, after all, though no higher law doctrine. Hume's recognition of the necessity of social rules points to the natural character of prevailing norms and even to their rationality, or at least utility. Kant may not have left much ethical teleology in nature, but the categorical imperative has plenty of higher law possibilities. As for Karl Marx, was there ever a better Aristotelian? Human nature was for him a potentiality to be realized in the future, not the actuality of men perverted by class oppression and exploitation. One wonders what all that fuss has been about. For the fact is that these theorists had no use for natural law theory. The confusion of *is* and *ought* looked to some like a bit of propagandistic rhetoric. Others saw it, not at all unrealistically, as an ideological effort by the Church to constrain the emergent secular state. Finally, many saw it as a device to achieve a Ciceronian consensus (one law for all of imperialist Rome) upon pluralistic societies that ought, rather, to be hospitable to controversy and conflict.

Why harmony after all? The truth is that natural law is a doctrine for menders and reconcilers, not for innovators and challengers. Its temper can first be seen in Aristotle's trying to patch up the conflicts created by sophists, nihilists, traditionalists, and radicals of all kinds. It is clear in Thomas Aquinas' efforts to make peace between Christian piety and pagan civic ethics. There are the lawyers of the seventeenth century trying to provide a rational program for international peace in a Europe clearly doomed to religious division and animosity. Finally there was Leo XIII and his heirs trying to use Thomism to ease the conflicts created by industrial capitalism. It is an intellectual enterprise that will always have an appeal, even in the face of so little genuine success. Professor Sigmund makes no secret of his wish to show that natural law theory could still bring many opposed minds together and fulfill its traditional function in dealing with such issues as justice in war, the limits of political immorality, and the basic social needs of men as political animals. Even he is, however, less rationalistic and metaphysically less optimistic than a genuine Thomist. He is, of course, at his best in dealing with the Renaissance scholastics who were quite remote from Thomism in their voluntarism and, in Suarez's case, communalism. He is, moreover, very much aware of the nuances and divergencies even in the natural law tradition. All this gives this book a distinct tone of understatement and often of doubt.

This is, in short, a stimulating and intelligent addition to the history of political ideas. It has an excellent bibliography, mostly in the footnotes, for which Professor Sigmund's fellow scholars will be grateful.

JUDITH N. SHKLAR

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The Professor and the Public: The Role of the Scholar in the Modern World. Compiled by Goldwin Smith. Essays by Goldwin Smith, A. L. Rowse, J. H. Hexter. (Detroit: Wayne State University Press, 1972. Pp. 124. $4.95.)

Let me reverse the sequence of the essays' appearance in the book under review in order to leave the best until the end, and let me begin with Mr. Hexter's "sociological inquiry" into "The Historian and His Society." "The society of professional historians," says Mr. Hexter, commands its members to write history "in the right way," which means "the way our society wants history written" (p. 105). It enforces "guild standards" through judgment by peers (p. 106), establishes a pecking order, and metes out rewards and punishments. Those who pass with honor have "the prospect of a good share of the loot" (p. 103) and are offered "the cushier posts in history's corner of academia" (p. 91). Anyone unfamiliar with academic life might come to believe, if he listens to Mr. Hexter, that a historian's salary is set by his colleagues in the professional society! Mr. Hexter ascribes awesome authority to this society. Referring to Boyd Shafer, the one-time executive secretary of the American Historical Association, Mr. Hexter calls him the "shepherd of all historians in the United States," whose judgment that Edward H. Carr's book, *What is History?* was "the best recent book in English on the nature of historical study" was to be considered as a "pastoral injunction" by all members of the Association (p. 76). Mr. Hexter, however, does all he can to subvert Mr. Shafer's presumed authority. He quotes Carr as saying that when "we take up a work of history, our first concern should not be with the facts which it contains but with the historian who wrote it," (p. 76) and that you cannot "fully understand or appreciate the work of the historian, unless you have first grasped the standpoint from which he approached it" (p. 77). These two statements, says Hexter, make the same point (a point which Hexter does not like at all), and Carr has merely "formulated that point in another way." Now, I may have some reservations indeed about Carr's admonition to be concerned first with the historian and then with the facts he presents, but I find it hard to disagree with the assertion that a *full* understanding of a work includes an appreciation of the historian's standpoint. Hexter, however, rides high and roughshod, upholds guild standards, is little troubled over inconsistencies, and praises the society of the happy few who have learned how to please themselves and who now enjoy "the loot." I wonder just how many practicing historians recognize themselves and their situation in Professor Hexter's description?

While Professor Rowse does not celebrate guild history, he treats us to what I can only describe as the profession's lofty commonplaces. The historian's responsibility is to truth, which he must not distort by his desire to propose a thesis or by his inclination to fictionalize. Bias, says Mr. Rowse, is unavoidable to some extent, but "we can at least be self-aware, candid about our prejudices, declare ourselves at the outset, put our cards on the table" (p. 52). Mr. Rowse confesses his dislike of fanatics and their certainties, and opts for aesthetic values, patience, careful, responsible, constructive criticism, and justice of mind. Who could possibly disagree? I have found few historians who did not claim for themselves responsibility and constructiveness, and I must confess my inability to accept with admiration the pronouncements of an historian who, in one and the same breath, condemns the "extraordinary self-confidence, the power of . . . certainty and conviction" (p. 52) of his pre-Freudian nineteenth-century forebears, which arose from their being so unaware of their motives, self-interest and personal inclinations, and then blithely announces that his religion is "the religion of all cultivated people" (p. 58). I suspect that when pressed, Professor Rowse would find these cultivated people and their religion in the guild and its standards.

Professor Smith, whose address opens up the book, acknowledges that the great ethical commitments of scholars derive their tests and thus their meaning from their daily trials in the classrooms of our universities. Here the conflicts arise between the wisdom of scholarship and the moral indignation of youth. Here, too, the question about the scholar's role in the modern world is raised most insistently, and the historian's tendency to equate scholarship and history is exposed as the provincialism that it is. "The first duty of a university is not to develop skills but to develop a breadth of view, a sense of proportion and perspective, a sound judgment, and a depth of mind. This is the meaning and goal of a liberal education" (p. 23). Professor Smith insists that professors not sit in isolation. He recognizes that scholarship, to have meaning, does not exist primarily for members of the guild, but for the members of the body politic. Thus skills of writing and of communication are essential, for the scholar is accountable to all.

Professor Smith thus has taken the subject out of the provincial compass of Messrs. Hexter and Rowse. He has affirmed once more the great business of the education of the young in the liberal arts, and defined the scholar's responsibility in that framework. Messrs. Hexter and Rowse speak a different language and move in the world of specialized research scholarship. Certainly, one would not want to quarrel much with what they say in that context, other than to note that the context is limited and not designed to do justice to the subject of the scholar's role in the modern world.

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Institution Building: A Model for Applied Social Change: Edited by D. Woods Thomas, Harry R. Potter, William L. Miller, and Adrian F. Aveni. (Cambridge, Mass.: Schenkman Publishing Co., 1972. Pp. 291. $8.95, cloth; $5.50, paper.)

This book presents papers selected from among those given at a 1969 workshop sponsored by the Agency for International Development and the Committee on Institutional Cooperation. The first section consists of four papers devoted to the topic "A Rationale for Institution Building in Technical Assistance Programs." The second section is comprised of four papers around the topic "Introduction to the General Theory and Issues of the Institution Building Model." The third, "Application of the Institution Building Models to Technical Assistance Programs," contains seven papers. The last section has three papers under the topic "Research Applications of the Institution Building Model."

A major significance of this book lies in its attempts to formulate and discuss how institutions are created and developed, an area of problems (theoretical and descriptive) that has only recently emerged in the literature. It may be surprising to some readers to learn that the vast institutional or organizational literature has almost exclusively been concerned with chronologically mature organizations. Indeed, our theories of institutions or organizations orient us little if at all to how organizations are born, so that if one was going to create a new organization and wished to be guided by the knowledge and wisdom in our professional journals he would find much that would mislead him and very little that would be helpful. Institution building is not, as Jacobson (page 281) makes refreshingly clear,

> a simple linear progression toward a set of specific, firmly established, detailed goals or objectives. If that were the case, it would be possible to define the goals unambiguously and well in advance of the institution building effort, design a comprehensive set of indicators of progress toward the achievement of the objectives, and evaluate the total enterprise in terms of readily accepted criteria. The model suggested is that of constructing a house with well-tested blueprints, or taking a voyage to the planets, after exhaustive preliminary testing of operations, equipment, field conditions and contingency alternatives . . .
>
> But . . . the intent is not to build a structure or project whose appearance, functions and use can be anticipated and simulated in detail, well in advance of operation, but rather to initiate a change process whose outcomes can only be described in broad program objectives. If institution building, in this sense, is successful, one of the measures of its success will be the emergence of unanticipated combinations of events, circumstances and products. And, because the domain of institution building is the domain of human values, beliefs, aspirations and competence, and the tools of institution building include interpersonal influence, human communication, and management of the learning process, it is inevitable that the criteria of success will be complex, difficult to quantify, and measurable only within broad margins of error. Goals and objectives are likely to change, not only in detail, but even in major components, as the institution building process continues.

The point made by Jacobson is supported by much that is said by the other contributors, particularly Milton J. Esman who manages in relatively few pages to convey some of the major conceptual issues as well as the inadequacies and gaps in existing formulations. But at the end of his excellent paper Esman raises some questions which, had they been more directly addressed by other contributors, would have made this book more likely to receive the attention it deserves. My guess is that most readers will conclude that the book is only directly relevant to the circumscribed area of technical assistance, (primarily in regard to agriculture) to underdeveloped countries. But, Esman asks if it isn't, indeed, likely that institution building is a generic process that has significances far beyond technical assistance to foreign countries? Isn't it likely that although "it began with a strictly international, less-developed country focus, institution building may well be in demand for domestic development activities?" In a recent book (*The Creation of Settings and the Future Societies,* San Francisco: Jossey-Bass, 1972) I try to demonstrate that creating a new setting is a generic process applicable to human social endeavors ranging from marriage to revolution. In that book I do not talk about technical assistance programs to underdeveloped countries but about our own society, in which the rate of setting-creation (or institution building) has been fantastically high, and the rate of failure disturbingly high.

It is precisely because institution building is probably a generic process that the reader of "Institution Building" should not view it in narrow terms. Once he gets beyond the images conjured up by words like "institution" and "building" that make the task seem to fall in the realm of social engineering (we are not dealing with a "simple linear progression"), and if he can make the sustained effort to see the problem in the context of efforts in our own society, he will find this a truly stimulating volume. If the reader cannot do these things, it will not be all his fault, because so much of what is in this volume is made to seem "foreign," literally and figuratively. If he starts with Esman's paper, however, the reader increases his chances of seeing the general import of this volume.

SEYMOUR B. SARASON

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Political Violence and Civil Disobedience. By Ernest van den Haag. (New York: Harper Torchbooks, 1972. Pp. 123. $1.95, paper.)

This brief book is made up of two distinct but related essays—the first on the topic of civil disobedience and the second on the topic of political violence. The argumentative level in both is mediocre by current philosophical standards; complex positions are given sketchy summaries; and little that is new is contained for those familiar with the previous literature on either topic. The book might well have a certain provocative value if assigned for discussion purposes to a group of undergraduate students, but nothing in it justifies the intemperate praise quoted (on the jacket) from reviewers—e.g. "a model of analysis . . . never less than brilliant" (Sidney Hook) and "scholarship at its highest" (Robert Nisbet). It contains almost no careful philosophical analysis, is at all points considerably less than brilliant, and is too superficial to be regarded as a work of scholarship at all.

These are hard judgments, and I cannot hope fully to establish them in a brief review. I shall therefore simply select certain representative claims from the book that illustrate the failings I have condemned.

Scholarship: Failings of scholarship include incorrect or superficial interpretation of a text, attribution of views to a writer on the basis of inadequate evidence, and ignorance of or indifference to important contributions to the literature on the subject in question. With these criteria in mind, I should like to note the following:

(a) On page 15, Thomas Hobbes is presented as a philosopher who will accept no qualification on the obligation to obey the law. This is incorrect as an interpretation of Hobbes, since Hobbes explicitly states (*Leviathan,* Chapter XXI) that a man is not obligated to obey a law when that law threatens him with immediate death—the avoidance of violent death being the main reason why a rational man contracts for civil government in the first place. Thus even a reactionary like Hobbes helps pave the way for the "liberal" idea of resistance found in such writers as John Locke—i.e., the idea that our obligation to government is a result of our contracting to obtain certain ends (for Hobbes, peace and security; for Locke, protection of a variety of natural rights) and that, when government fails to secure these ends, it may be resisted.

(b) On page 11, Herman Melville is presented as a man who "affirmed the duty of authority to override individual rights for the sake of *raison d'état* in the novel *Billy Budd.*" It is by no means obvious that the central character in this work of *fiction,* Captain Vere, is properly interpreted as holding such a general view. Even if such an interpretation is accepted, however, this hardly provides sufficient evidence for concluding that Melville himself held such a view.

(c) Van den Haag, in his discussion (pp. 35 ff.) of Ronald Dworkin's "On Not Prosecuting Civil Disobedience" (*New York Review of Books,* June 6, 1968, and December 17, 1970), rejects and even ridicules the idea that there are "natural rights" or "moral rights" antecedent to any legally or politically given rights.

He does this without mention or consideration of what is surely the most important philosophical discussion and defense of natural rights in recent philosophical literature: H. L. A. Hart's "Are There Any Natural Rights?" (*The Philosophical Review,* 1955). This is particularly serious since Dworkin's ideas are best understood against the background of his general sympathy with Hart's legal philosophy.

Analysis: In analyzing the concept of *violence* (p. 54), van den Haag gives us the following: "Violence: physical force used by a person, directly or through a weapon, to hurt, destroy, or control another or to damage, destroy, or control an object."

This analysis is extremely problematical, in part because it contains locutions which are themselves greatly in need of analysis. What, for example, does "hurt" mean here? Is one hurting a person when he causes that person pain? A dentist might resist this suggestion, for he might well want to argue that when he causes a patient pain he is doing so, not in hurting him, but in *helping* him. And the dentist should no doubt be upset by any suggestion that he treats his patients violently. And what is it to "use force to hurt"? Does this mean to use physical force in such a way that hurting causally results? If so, then a battlefield surgeon who performs a painful amputation on a patient with consent will (if causing pain is hurting) be treating the patient with violence. But this is odd. To avoid this kind of counterexample, van den Haag might claim that he means not merely causally bringing about hurt, but acting in a hurtful way with causing the hurt as one's final *intention* or *purpose,* and not merely as a means to one's final intention or purpose. The problem with this definition is that it will entail that clear cases of violence are no longer counted as cases of violence—e.g., a policeman who is beating a demonstrator over the head with a club (a clear case of violence) may say that his final intention or purpose is not to cause pain or hurt but is rather to maintain order or control a crowd and that he must hurt simply (perhaps regretfully) as a means to these ends. If the difference that makes the battlefield surgeon innocent of violence and the policeman guilty of violence is the presence of consent in the patient and the absence of consent in the demonstrator, then the connection between violence and violation (of rights or dignity) may be closer than van den Haag (p. 55) is willing to admit. (For an exploration of the connection between violence and violation, see Newton Garver's "What Violence Is," *The Nation,* June 24, 1968.)

I make these observations not because I have my own analysis of violence to offer but rather to make a certain methodological point—namely, that with respect to emotively charged terms like "violence," bandied about so much in moral and political discussion, there is probably no fixed set of exact rules within the language to govern proper usage. If this is so, then any definition of such a term will be, to a certain extent, a *stipulation.* I have no objections to such stipulation if it serves a clear theoretical or even ideological purpose, but I do think that it is incumbent upon a writer that he (a) make it clear that he is stipulating and (b) make it clear by what criteria he regards the stipulation to be justified. This is what van den Haag does not do. When he offers a definition of a term like "violence," he speaks as though he is putting forth an empirical linguistic observation that would be obviously acceptable to any man who is rational and knows how to speak English. But the issues of analysis here are much trickier than he realizes; and, as a result, he puts far more substantive weight on his definitions than they can properly support—e.g., when he claims that poverty is not "violence against the poor" (p. 64), he seems to think that he is simply stating an obvious and ideologically neutral *fact.*

Argument: Central to van den Haag's conservative, though qualified, case against civil disobedience and political violence is his claim that "unless an infallible interpretative authority is assumed, there can be no ultimate operative standard to determine *when* to disobey legitimate authority" (pp. 26–27). He is claiming here that the only answers to be found to complex moral issues would be an appeal to some kind of *authority*—e.g., God, the Church, etc. Since there exists no such competent authorities, he concludes that there are no final answers to such questions.

But why suppose that the appeal must be of this sort? In a book on a paradigmatically *moral* topic, no mention is made of the role that *moral philosophies*—e.g., utilitarianism or Kantianism—might play in the assessment of the morality of civil disobedience or political violence. Such moral philosophies attempt to establish *rational criteria for the resolution of moral controversies* and are in no sense to be regarded as mere appeals to authority. (Of course these theories have to be interpreted and there are no "authorities" whose interpretations must be accepted as final. But it is true of *any* judgment of *anything*—including van den Haag's own book—that people, in interpreting it and applying it, have the responsibility of exercising their own informed reason to the best of their ability. And what is true of all acts of judgment cannot be used as a fatal objection against a particular act of judgment.) Now, van den Haag may believe that all moral theories ultimately fail, but he surely should at least consider these theories and *demonstrate* that they fail. Though he does flog the dead horse of natural law, he does not even mention utilitarianism or Kantianism—the two classical secular moral philosophies of greatest influence. And it is astounding that a book written on civil disobedience in 1972 contains no reference whatsoever to the important work relevant to the topic by John Rawls—a contemporary moral philosopher who has attempted to ground the obligation to obey the law, and to develop criteria for justified civil disobedience, in terms of his Kantian social contract theory of justice. [See John Rawls, "Legal Obligation and the Duty of Fair Play," in *Civil Disobedience and Violence,* Jeffrie G. Murphy (Belmont, Calif., Wadsworth: 1971); Rawls, "The Justification of Civil Disobedience," in *Civil Disobedience: Theory and Practice,* Hugo Adam Bedau (New York, Pegasus: 1969); and Rawls *A Theory of Justice* (Cambridge, Mass.: Harvard University Press, 1971.)] Similarly ignored is the prominent recent work by utilitarians on the topic—e.g., Richard Wasserstrom's "The Obligation to Obey the Law" (*U.C.L.A. Law Review,* 1963). By ignoring such work entirely, van den Haag prevents his book from being taken seriously as a contribution to contemporary philosophical thinking on this problem.

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Terror and Resistance: A Study of Political Violence, with Case Studies of Some Primitive African Communities. By Eugene Victor Walter. (London and New York: Oxford University Press, 1969. Pp. 385. $8.50, cloth; $2.95, paper.)

A growing body of literature is addressing itself to violence and to the political sociology of deviancy. Within that literature the publication of Professor Walter's book is an important event. This is a work in political anthropology, but it carries implications for the study of violence through the perspectives of other disciplines as well. The author looks at select African traditional communities and draws from their experience conclusions about the functions of violence and terror, the nature of resistance, and the interplay between power and fear in the structure of authority.

The author himself seems to shrink from addressing himself to the economics of violence and terror. But in this he is sometimes a little naive: "Since I am dealing with violence as an instrument of power . . . I shall exclude economic terrorism, ranging from armed robbery to systematic extortion, and the various methods by which violence may be used for economic gain" (p. 13). This is a singularly inept formulation. How is power to be so sharply differentiated from economic control? The threat of terror in Amin's Uganda in recent times facilitated the rapid exodus of thousands of Asians. Was this threat an exercise of power or a pursuit of economic gain? The distinction here is absurd.

But in fact this book does make a number of more important and valid distinctions and provides significant typologies of political and terroristic phenomena.

The author starts by distinguishing between a "siege of terror" and "regime of terror." Many of the movements underway for the liberation of Southern Africa, for example, are attempts to overthrow established systems of power—and are therefore instances of siege of terror. The utilization of terror for maintaining power which is already held constitutes, according to Walter, an instance of a regime of terror. What does not emerge about contemporary African conditions is whether we are to look at the regimes maintaining power in Southern Africa as being themselves regimes of terror, fortifying themselves against the rudimentary movements of national liberation, against the beginnings of a significant siege of terror.

Other important distinctions that Walter makes include those between terror and intimidation, between violence and terror, and between victims of terror and targets of terror. In clarifying areas which could so easily be obfuscated, Walter emerges as a rigorous theoretician. He also provides us with typologies. Thus, a typology of systems using violence as a source of power has at one pole systems that use violence as a last resort, and at the other systems that invoke it as a first solution (p. 26). Some of these typologies constitute stages in development. Thus, we are given a typology of African social entities, starting with the band of hunters, and going on to the tribe of communal but subpoliticized relationships, the chiefdom, and the state. On balance, Walter seems to accept Weber's definition of a state as a "human community that 'successfully' claims the monopoly of the legitimate use of physical force within a given territory" [Weber "Politics as a Vocation," in H. H. Gerth and C. Wright Mills, *From Max Weber: Essays in Sociology* (1957) p. 78].

One sometimes gets the impression that Walter regards the movement from band to state as a movement from primitiveness to civilization. Yet he is reluctant to call even a traditional African *state* anything else but "a primitive community." His study includes discussions of elaborate African political systems, ranging from the establishment of the Zulu state to which he devotes considerable attention, to briefer references to the states of the Baganda and Ashanti, and others. It is not clear what Walter would have lost by referring to all the different varieties of African political organizations as, quite simply, "African traditional systems" instead of "primitive African communities." It may be just a cheap way of emphasizing a distinction between the African examples that he is looking at in this book, on the one hand, and the ancient Greek and Roman examples scheduled for his next book (p. 10). Walter's typology of resistance has provocative elements. He sees resistance as including "criticism, challenge, disobedience, rebellion, and secession" (p. 336). What is not convincing is the preference for the word *resistance* for such forms of *opposition* as criticism, challenge, and even disobedience. Walter seems to prefer the word "resistance" even for political parties in opposition, whether or not they go beyond criticizing the government's policies. His distinction between reactive counter-resistance by those in power and preventive counter-resistance is more interesting and stimulating. But to include the normal behavior of the labor party in England in the same bag as the attempt of the Ibo to secede from Nigeria seems to me somewhat dubious (pp. 338–339).

Walter is fascinating in his interpretations of the use of terror by Shaka, the Zulu ruler of the early 19th century. He sees elaborate astuteness in Shaka's utilization of terror to consolidate the Zulu state and enhance its preeminence. If there was a brutal madness in Shaka, Walter sees a clever if diabolical *method* in it. Some have sought sexual and Freudian explanations for some of Shaka's wanton acts of cruelty, but Walter's position is reminiscent of that taken by James W. Fernandez in seeing Shaka's behavior "not in terms of the problems of sexuality, but in terms of the problems of power" (Fernandez, "The Shaka Complex," *Transition* 6, [February/March 1967] p. 13).

Walter addresses himself to a number of important political processes, including the process of transformation from kinship to statehood and the decisive process of political integration. There are times when I get impatient with some of Walter's interpretations. For example, he makes the statement that the only kind of state to develop in "primitive Africa was a monocratic form of rule in which either a king or a despot stood as the formal head of the governmental structure." Either this statement is circular and tautological, by which any political entity in Africa that is not monocratic is by definition regarded as not a state, or Walter is blinding himself to the diversity of state systems in traditional Africa.

But when all the criticisms of this book are completed, when all the irritation it sometimes provokes has subsided, there is in Walter's book an important intellectual and theoretical achievement.

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Deadlock in School Desegregation: A Case Study of Inglewood, California By Edna Bonacich and Robert F. Goodman. (New York: Praeger Publishers, 1972. Pp. 118. $12.50.)

Since the mid-sixties, the Los Angeles suburb of Inglewood, like many cities outside the South, has experienced conflict over the racial composition of its school system. This volume chronicles a portion of that dispute, from the origin of *de facto* segregation as an issue to a school board vote killing a proposed remedy. The events recounted are not dissimilar to those reported in other accounts of school desegregation, nor is there anything new either in method or in explanation, which is an interest-group analysis.

What the authors do provide, however, is an important suggestion for resolving this type of controversy. The major obstacle to successful integration, they contend, is not so much political as demographic. Black families from adjacent areas of Los Angeles were moving into Inglewood so rapidly that community leaders feared the city would soon follow nearby Compton, where whites had become a small minority of the city's population within a few years. In addition, the quality of the schools, already perceived to be declining at least in part because of the influx of blacks, had precipitated a withdrawal of white students from the system. According to the authors, a "preemptive" decision by the school board to pursue racial balance by redistricting, bussing, and the like would not halt and might even increase the rate of resegregation, an outcome desired by neither black nor white residents. Faced with moral and political demands for desegregation on the one hand, and demographic anxieties on the other, the school board temporized.

The traditional, i.e., southern, approach to school desegregation involves laying down the law and strictly enforcing its compliance, with racial accommodation expected to follow through increased group contacts. In a city like Inglewood, however, where the dictates of duty are less clear and where residential mobility within the metropolitan area is relatively easy, a strategy of compromising differences among local groups and offering improvements in the quality of schools as incentives for cooperation seems more advantageous. Opposition to school desegregation was not wholly a matter of antediluvian racial attitudes—the white backlash—nor confined only to whites; many of the city's blacks were apparently worried that the Los Angeles ghettoes, so recently left behind, might follow them to the suburbs. For the pro-integrationists, it would be a hollow victory indeed if ending *de facto* segregation of the schools diminished their attractiveness for whites and middle-class blacks. To have any chance of overcoming the "tide of demographic change," (p. 89) racial balance would have to be induced; it could not be compelled.

The significance of this lesson should not be diminished by the authors' recommendation that new "mechanisms of participation," more responsive to the "real needs" of students and parents, are called for (p. 89). Inglewood already possessed an elected school board, active educational interest groups, and a supposedly representative citizen advisory board; all were involved in the dispute, helping to produce a series of inconclusive actions. This may be testimony to their ineffectiveness, but might also reflect rather accurately the ambivalence of the community about desegregation. The authors' own preferences appear to lie not so much in new mechanisms as in a new style of leadership, one they view as more consultative, quicker to intervene, less polarizing, and more willing to negotiate privately. Greater public participation does not necessarily bring these qualities into government and may as readily bring just the opposite ones.

In any event, neither procedural reforms nor responsive officials assure the ability to deal politically with an issue like racial balance in the schools. Bargaining can also be ineffective, and it may be that no political actions short of metropolitan consolidation will prevent resegregation in cities like Inglewood. "The particular choices made by one local school board in one community appear trivial in light of the force of black expansion across the Los Angeles basin," (p. 94) the authors observe without presenting evidence of the relative importance of school board decisions on this trend, or even an account of what really occurred in Compton. While not all suburbs face the population pressures of Inglewood, nonetheless, even in these cities, conflict over school desegregation may ultimately have to do with more diffuse concerns for the quality of community life than with specific problems of the schools.

LESLIE LENKOWSKY

Harvard University

The Evolution of American Democracy. By William R. Brock. (New York: The Dial Press, 1970. Pp. 272. $7.95.)

William R. Brock, the distinguished Americanist who is Professor of Modern History at the University of Glasgow, has in this highly informed interpretive essay stated the most positive case I know of for the virtues of American democracy. As his title implies, he sees democracy evolving from the time of the Revolution to the present, confronting crisis after crisis, adapting to a modernizing society, and still exhibiting vigor and promise today.

Brock locates the origins of American democracy in the rational republicanism of the revolutionary era and emphasizes the truly innovative quality of the ideology that was to define the values, goals, and norms of the American regime. The federal Constitution, contrary to its critics, marked a decisive stage in the evolution of modern democracy by facilitating the adjustment from a simple to a complex form of republicanism. Subsequently, the invention of the two-party system greatly advanced the art of democratic politics by offering the electorate genuine but moderate alternatives and by bringing diverse groups together in support of the government.

The failure of democracy represented by the Civil War was due less to institutional than to intellectual rigidity and most particularly to the refusal of the South to tolerate open debate on the issue of slavery. The chief outcome of the Civil War was to ally democracy with a new feeling of nationalism. Brock acknowledges the shortcomings of Reconstruction but hails the Radicals as "representative men" who, while they did not immediately achieve their objectives, laid the constitutional and legal foundations for future democratic progress. Even the sordid interval of the Gilded Age, with its antidemocratic retreat to elitism and *laissez faire,* is depicted positively as providing an essential bridge between a rural, localistic past and the complexities of a modern, nationalizing society.

The "modernization" of American democracy in the years since 1890 is charted in terms of five landmarks—Populism, Progressivism, World War I, the Depression, and the New Deal—each of which made distinctive contributions to the evolution of American democracy.

In surveying the current condition of democracy, Brock firmly rejects both the "consensus" and the "power elite" interpretations, opting instead for a Madisonian pluralism that he describes as operating through the mechanism of "countervailing power." Although he is baffled by the insolubility of America's racial problems and distressed by the alienation of white, campus-based radicals, he suggests that the present discontents may be due largely to the increased availability of information and that they are a "sign of health, not disease." In any case, the remedy is to be found in more democracy, following the Progressive diagnosis.

Brock has written a powerful, forthright, and provocative book that seems deliberately intended to rebut the revisionist devaluations of American democracy that acquired such a vogue in the decade of the sixties. He exhibits no bias against capitalism; he has no commitment to mass democracy; he is not preoccupied with the plight of the inarticulate and the oppressed. American democracy approximates his ideal of the "free society," with the necessary mechanisms "to wrest power out of the wrong hands, and to divert it from wrong purposes . . ."

One might quarrel with every particular in Brock's narrative, but in this space I can deal only with the sum of his argument. My major reservation is that he does not confront the question of whether American democracy has produced a "good society," whether it has released the proper energies and encouraged the best type of human behavior. Democracy, as he presents it, has engendered support for the political system and fostered its maintenance, but is that enough?

Because of its general nature and its lack of novel hypotheses, this work would seem to have little interest for specialists in the field. But the careful reader will frequently be struck with the sharpness of Brock's insights into particular aspects of the American political system, insights that derive from the astute comparative perspective that informs his observations.

RICHARD P. MCCORMICK

Rutgers University

Securing the Revolution: Ideology in American Politics, 1789–1815. By Richard Buel, Jr. (Ithaca, New York: Cornell University Press, 1972. Pp. xii, 391. $14.50.)

From the moment the first American parties began to form in the 1790s, observers disagreed violently about why they coalesced, about their nature, about the motives of their leaders, and about their significance for the future of American politics. Until the last generation, historians have battled as ferociously as the party protagonists and in almost exactly the same partisan terms. Recent scholars, however, have tended to be less polemical and more interested in understanding the two parties and events in the period than in defending either side or assessing partisan charges. It is within this newer tradition that Richard Buel has attempted an assessment of political developments from Washington's to Madison's presidency.

Buel begins with a very promising thesis: Parties developed because of an ideological division between the leaders who created the Constitution of 1787—a division about how to "secure the Revolution" (that is, how to assure the future of stable republican government, although "Revolution" and "republican government" are never defined). Federalists drew their leadership basically from an insecure elite distinguished from the mass of the people only by the "claim of superior virtue and ability" and "ascetic discipline and the dedicated cultivation of talents" (p. 81). Republicans, on the other hand, were southern planters whose land and slaves provided more secure distinctions, and while mobility was frequent in both elites, Southerners could transfer their wealth westward and depend on the inflated life-style of the planter. Northerners were tied to commerce and could not migrate easily, nor was their tradition or style secure enough to defend their pretensions against some of the leveling tendencies of the Revolution. Federalists feared that republican government was unworkable and that unless the national government was safely removed from popular passion, the United States would dissolve into anarchistic mobocracy. The French Revolution confirmed and indeed sharpened these fears. Republicans had confidence in republican government but feared it might succumb to lingering monarchism in the United States and to the forces of monarchism abroad. Both parties became locked by the mid-1790s in a struggle for public opinion, Federalists trying to impose institutions and policies on the people while bidding for popular support, Republicans trying to block pro-British, monarchical Federalist policies by exposing them to the people. Caught in a dilemma, one party could not develop a popular ideology; the other did develop such an ideology in the process of mobilizing public opinion to save republican government. The Republicans won power and retained it; their opponents suffered defeat and oblivion.

As interesting as this theme is, the book unfortunately contains flaws in conception, assumption, and research that are so basic as to make the conclusions unconvincing. Buel sees only Federalists and Republicans, when in fact both parties contained several ideological, geographic, and socioeconomic factions, and the rigidity of viewing politics solely in two-party terms obscures the complexity of the period. For example, Buel is unable to deal effectively with the Federalist split in the late 1790s. Second, several of his assumptions are questionable: that parties did not grow out of the battle over the Constitution; that both were ideological parties rather than coalitions bent on securing power or rewarding the personal ambitions of their leaders; and that "public opinion was the single most important ingredient in the politics of the first party system" (p. x). Much surviving evidence and recent scholarship (some in dissertations unpublished but available) contradict these assumptions. Scholars agree that society in the 1790s was deferential and that politics was often personalized. The Federalists relied on Washington and on patronage—and built an effective party; Republicans created elaborate party machinery precisely to create a "public opinion," and it could be argued that both parties manipulated the public far more than they were influenced by "public opinion."

Third, Buel neglects manuscript sources entirely, yet he purports to explain the influence of ideology on politics and thus by necessity the inner thinking of each party's leaders. Much key material, however, even about the influence of public opinion, remains in unpublished manuscript collections. It is difficult if not impossible, for example, to explain John Adams's thinking in naming the Murray mission to France in 1799 without using the rich Adams Papers collection, and Buel (p. 202) misses much of Adams's calculations. Relying on the logic of his argument, Buel finds the Federalists paralyzed in 1797 by the crisis in French relations, by the retirement of Washington, and by the still potent pro-French bias of the public. My own research, however, based mostly on unpublished correspondence, indicates that Federalist leaders were playing a waiting game, and while some became intensely frustrated, they actually manipulated the President, French relations, and public opinion exactly as they planned and eventually pushed the country into the war crisis of 1798. Last of all, the book lacks a coherently articulated thesis. Midway through the book Buel loses the theme of securing the Revolution and narrows his analysis to the influence of public opinion on the party struggle, which given his assumptions borders on tautology and leads in several cases to a distorted picture of events.

However flawed, the book is still important. By focusing on public opinion Buel makes some new and interesting points about several well-known events. He has cut to the core of party rhetoric, given it meaning within the context of the new scholarship on political ideology in late eighteenth-century politics, and shown how the ideology affected the party struggle. Scholars interested in American politics should read and evaluate for themselves this important, but unpersuasive, work.

RICHARD H. KOHN

Rutgers University

Federal Judges: The Appointing Process. By Harold W. Chase. (Minneapolis: University of Minnesota Press, 1972. Pp. 240. $10.00.)

Professor Chase has given us a volume which rightly deserves to become a standard work on the appointment of judges to the lesser federal courts. Reported here for the Eisenhower, Kennedy, and Johnson administrations are useful data on appointees' ages, party affiliations, previous judicial experience, type of law schools attended, and ABA ratings. But it is not these facts—at least not these alone—which make this an outstanding work, rather it is Professor Chase's personal probing of the political process.

Relying on an old college tie with Nicholas Katzenbach, Professor Chase won unusual entree into the Justice Department. Ensconced in an office adjacent to those of Katzenbach and Joe Dolan, Katzenbach's chief aide on judicial matters, Chase was given access to Justice's files on judicial appointments, had opportunities for endless hours of informal "interviewing," and was soon invited by Dolan to sit in on conferences with state politicians about judicial appointments. For the Eisenhower and Johnson years, Chase extensively interviewed those involved. Summaries of and a few lengthy quotations from responses to Chase's draft chapters by Eisenhower, William Rogers, and other interviewees lend insight and balance.

Professor Chase surveys the role and the interplay of the President, Justice, the Senate, the ABA, the press, home state senators and politicians, judges, the FBI, and others in the appointing process. The highly personal yet cumulative nature of the appointment process is highlighted by Professor Chase's weaving into the fabric of his study the biographies of key actors—of Katzenbach and Dolan; of Bernard Segal of the ABA Committee on Federal Judiciary; of William Rogers and Lawrence Walsh in Eisenhower's Justice Department; of Ross Malone, the ABA activist who played a leading Justice role at the end of the Truman years; and of others.

Overall, Professor Chase portrays a more Executive-centered process than is usually found in political studies. The rigid rule of senatorial courtesy has eroded, he reports, and senators are less willing to invoke the rule because of political repercussions back home. Further, appointment delays bring greater pressure on senators than on the President, for it jeopardizes their states' justice systems. Recess appointments may be, and indeed are, used by presidents to get their way. In the Kennedy administration, several appointments were sometimes packaged together to achieve presidential objectives, including the appointment of Republican judges.

To be sure, administrations differ in the degree to which the President and his men exert themselves in judicial matters. Thus, Eisenhower, the military hero, accustomed to command, treated senators almost imperiously. On the other hand, Johnson, the consummate legislative leader, was more deferential to senatorial wishes in such matters. The Kennedy men, believing in the Lincoln-style presidency but conscious of their limited credit in Congress, cared deeply about judicial appointments but were sometimes prepared to compromise.

Surprisingly, Chase concludes that the White House staff, excepting only John Macy under LBJ, had little influence on appointments. And pleasingly, the men in Justice primarily responsible are deeply devoted to their work. On the other hand, Chase sketches a chilling picture of the ABA's role in judicial matters. Aggressive, as the self-annointed often are, the Committee on Federal Judiciary emerges as conservative, Republican, biased toward corporations and other big interests, unsystematic in its interviewing, inexplicit about most of its criteria (but not about age, trial experience, and Martindale-Hubbell ratings), and on a few occasions hostile to religious groups or government counsel. I found myself cheering the merciless root-and-branch critique of ABA evaluation by Ernest Friesen, Dolan's successor at Justice.

Professor Chase reports attempts to discover potential nominees' policy views—on defendants' rights by Eisenhower and on civil rights by Kennedy—but he does not do so systematically enough to permit a judgment of whether policy plays as large a role in the selection of lower court judges as it does in Supreme Court appointments. Also interesting is the Kennedy administration's stated desire to make minority-group judicial appointments and Eisenhower's lofty rejection of any criterion based on hyphenated Americanism. On the other hand, both those who believe Mr. Eisenhower cared less about civil rights than Mr. Kennedy and those who have accused Kennedy of pandering to Southern senators in district court appointments will have occasion to rethink their views in light of Chase's interesting, if not intensive, conclusion that both men's Southern judicial nominees were about equally disposed toward civil rights.

From interviews, "objective" data, and his own impressions, Professor Chase concludes that the federal judiciary is well thought of, at least compared to state judges, and probably deserves that reputation. If there is any culprit it is life tenure, for most federal judges who were bitterly complained against were clearly suffering from the ravages of age and had had good reputations in their earlier years.

Chase is not content with the appointment process, however, and he urges possible reforms. One is modest: eliminating the role of *individual* senators in picking and pushing candidates for the federal bench. One is more radical: vesting the appointment power in the Supreme Court on the theory that judges are "inferior officers" under Article II, section 2 and need not constitutionally be appointed by the President. The latter proposal overlooks entirely the argument of scholars and the experience of the nation that the presidential appointing power is the only way, and then not always successful, of imposing political responsibility upon the Judiciary. Robert H. Jackson warned after the court-packing fight of 1937 that the judges have usually been "the check of a preceding generation on the present; a check of conservative legal philosophy upon a dynamic people, and nearly always the check of a rejected regime on the one in being." How much longer this check would have persisted if lower court judgeships from 1933 to 1937 had been filled by the Old Court rather than by FDR!

This ill-considered proposal aside, however, Professor Chase has written an important book which will win wide readership and acclaim among political scientists, legal scholars, and others interested in the federal judiciary.

DAVID ADAMANY
University of Wisconsin, Madison

The Rise of Warren Gamaliel Harding, 1865–1920. By Randolph C. Downes. (Columbus: Ohio State University Press, 1970. Pp. 734. $17.50.)

Contemporary historians are actively re-examining the politics of Warren Harding. Randolph Downes's contribution is a lengthy coverage of the pre-presidential years which will complement Robert K. Murray's, *The Harding Era* (Minneapolis: University of Minnesota Press: 1969), a treatment of the presidential years. Together, they provide adequate data for reflection upon his politics, and should stimulate further examination. Downes's work is a narrative, painstakingly constructed from documentation. While decidedly a political study, it is not self-consciously an analysis of statesmanship, political thought, or particular hypotheses. Instead, it follows the current of "politicking" which the author believes was the essence of Harding's politics, and which was of a higher quality than his "statesmanship." The two most salient features of his "politicking," and the sources of his power, are "Americanistic" and "mirage-creating" rhetoric, and unity-oriented conduct within the Republican party.

As Professor Downes recounts his history, at age nineteen, Harding bought the floundering Marion *Star* which he built into a financial success. Throughout his life, it was the foundation for his civic and business activity. At the turn of the century he was elected to the Ohio state senate and rose to floor leader. Later, he was elected lieutenant-governor. He made his way through the jungle of Ohio political factionalism by consistently repairing to the standard of party unity and refusing to devote primary energies to factional warfare. Possessing unique oratorical skills and speaking to the common man, the popular Harding became one of the powers of Ohio politics. While he lost his bid for the governorship in 1910, he effectively defended Taft in 1912, and was asked to place the president's name in nomination. His rhetorical and organizational skills and style in reconciling Progressives and Republicans in Ohio after the Bull Moose split earned attention beyond Ohio and additional power within the state. He was elected to the U.S. Senate in 1914 and was Republican "keynoter" in 1916. One of the most articulate members of the national party, and dedicated to its unity, he became a darkhorse candidate for the 1920 nomination. Interested in remaining in the Senate, but encouraged by duty to the party and a threat to his power base in Ohio, he agreed to try for the presidential nomination. A well-developed strategy based on nationwide second choice commitments, and not the decision of bosses in a smoke filled room, paid off when the convention deadlocked.

For more than twenty years, Harding advocated and practiced party unity. He accepted factional defeats with grace, supported the winners, suffered pointed rebuffs without complaint, and successfully worked to mend the party's fences:

> While he himself was buffeted by bosses . . . he never for an instant—publicly at least—lost his poise and eloquence . . . Harding could gloss over the inner jangling discord with exhibitions of that personality and oratory which gave the appearance of harmony, efficiency, firmness, and dignity (p. 134).

Downes says that this behavior was motivated by opportunism, and that its efficacy rested on dependability and adjustability. The pattern, however, has a dignity and persistence which indicates at least a remarkable temperament, if not dedication.

> The mirage of "Harding unity" . . . was the image of something constant in the midst of bickering and discord . . . Harding was the one man who could at least talk and act unity, and appear unscathed as contests ended . . . In this role there was a certain strength, and no managers . . . created or maintained it . . . Harding created his own image (p. 158).

Its success also raises the unexplored possibility that Harding identified a rational common denominator among the factions.

According to Downes, Harding "could maneuver, manipulate, and spellbind any development into a Republican advantage" (p. 265).

> Harding . . . knew the feelings of the man in the street . . . It is easy to see why his practical-minded fellow Americans, as they listened to his eloquent, obvious, and adjustable phrases, would eventually trust him with the highest office in the land (p. 256).

Downes is interested primarily in the imagery and tone of Harding's rhetoric. He argues that Harding's understanding of issues was seriously deficient, but he is not always persuasive, for he does not develop fully Harding's own positions and tends to treat all issues as illustrations of Harding's manipulative rhetoric. Since Downes does not explore the soundness or unsoundness of Harding's thought and does not systematically link the substance of his rhetoric to intelligently held beliefs of the body politic, the Ohioan's rhetoric of persuasion emerges as mere exploitation of egocentric Americanism.

The pervasive argument of the work is that Harding was opportunistic, a mirage creator, adjustable to bosses, often evasive and uncomprehending in argument, but convincing and likable. The overarching argument of the book is that Harding's electoral and party successes resulted from eloquence, ambivalence, and adjustability; and that the successes occurred despite the superficiality of his understanding of Republicanism, Progressivism, major issues, and the principles of economics, world history, and international relations. This argument suggests that Harding represents a failure of American democratic politics—a serious gap between the requirements of party and electoral success on the one hand and the requirements of statesmanship on the other. The author does not completely make his point, however, for he does not address the possibility that Harding may have had a coherent understanding, especially of matters such as system maintenance, and that there may have been reasonable links between his understanding expressed in rhetoric and the perceptions of both party leaders and the public. Downes's work indicates that these possibilities could be fruitfully pursued.

Frank A. Burd

University of Maryland, Baltimore County

The American Mail: Enlarger of the Common Life. By Wayne E. Fuller. (Chicago: The University of Chicago Press, 1972. Pp. xi, 378. $8.95.)

This study was written for the Chicago History of American Civilization; as such it takes a long view and gives minimal attention to anything that has happened since World War I. It is a basic thesis of the book that all major elements of the American postal service had been put in place by that time.

The first two chapters provide a basic narrative history of the origins and evolution of the postal service in colonial times and during the nineteenth century. More specialized chapters then deal with the contributions of the postal service to the bonds of union, the diffusion of knowledge, the promotion of private enterprise, the expansion of the American empire, the safeguarding of the nation's morals, and the advancement of politics through the patronage of appointments and contracts. An "Epilogue" of twelve pages deals with recent history.

Political scientists who share the growing concern within the discipline for the study of communications may find this book useful as background. Even in these days of the mass media, interpersonal communication remains primary in its importance; and communication by mail is still important, though not as important as it once was. The book is well written and seems authoritative for what it covers. Specific passages, even quotations, are undocumented, but a full bibliographic essay is included in the back matter, and the book is well indexed.

Paul T. David

University of Virginia

The Changing Politics of the South. Edited by William C. Havard. (Baton Rouge: Louisiana State University Press, 1972. Pp. xxv, 755. $17.50.)

In 1946 the late Roscoe C. Martin, then of the University of Alabama, engaged V. O. Key, Jr., to study "the electoral process in the South." Mr. Martin hoped explicitly that the resulting book would stimulate future systematic research on the region's politics. Mr. Key, a man of controlled enthusiasms, was dubious. He would be grateful to finish his own attempt. Nonetheless, after *Southern Politics in State and Nation* appeared in 1949 it manifestly motivated and influenced much subsequent writing about political processes, generally as well as in the eleven former Confederate states of Key's South.

In following years, Key and I were often urged by scholars and publishers to revise, or update, or do the whole thing again. Once was enough, but when others expressed interest in undertaking some form of *Southern Politics* revisited, Key, so far as I know, always encouraged them to do so, and since his death in 1963 I have cheered on all who have raised the question.

The Changing Politics of the South is the only comprehensive treatment to appear since 1949 that has addressed southern political developments as Key did, state by state with additional regionwide commentary. Numerous important regional analyses have been published, many of high quality, but all have examined special phases of the region's politics. None has sought to examine the full context of evolving politics in each of the eleven states.

Professor Havard, editor and contributor of introductory and concluding chapters, disavows an attempt to provide a "successor" to *Southern Politics*. Rather, he and his fellow authors propose to use the earlier book as a "guidepost against which we could measure how far and how fast the politics of the South has gone over the past twenty or twenty-five years" (p. vi). The book has abundant merits. Professors Key and Martin would have been gratified—which, at least for those who worked with them, is all that need be said.

It is a considerable achievement that the book should appear at all. Aside from Professor Havard's own chapters, all the other twelve —one on each state and another on the South in Congress—are written by different authors. They are understandably uneven in concept and quality. "Although the time span and the broad theme of change during the period covered were agreed upon, each author was left largely to his own devices in the choice of methods to be used and the particular focus to be given to the treatment of his state" (p. vi). The authors met as a group two or three times, a few of the participants saw outlines or drafts of chapters other than their own, but otherwise the work was conducted independently except for coordination achieved by the editor through correspondence.

Key had thought at first that he would undertake only regionwide analyses of specified topics, not addressing individually the politics of each of the states. Early he concluded that this plan was intellectually unmanageable. Consequently, about 40 per cent of *Southern Politics* is devoted to the state chapters. The new book is confined almost entirely to the state stories. They run some 70 per cent longer than the comparable eleven chapters in Key's work. Each of the contributors to Professor Havard's book is well acquainted with the politics of his state. Many of the authors were in fact established political scientists at the time Key was studying the South. Not all the chapters reflect intensive field inquiries of the kind Key relied on, and the authors' tasks were also made more demanding, as well as in some ways easier, by the proliferation of intensive and sophisticated studies in recent years.

Some of the chapters are more than twice as long as others, and they vary greatly in analytical power. Inevitably, there are seeming differences from one to the other in interpretation of common elements. Some topics of importance in the 1940s are largely ignored. With only one serious exception, however, all the chapters, including that on the Congress, constitute highly useful additions to our information about state politics and to our understanding of more general processes of American society. The editor's own contributions, especially, reveal a spacious mind, deeply sensitive understanding of southern qualities, and wide knowledge of the evolution of the American political system and of the social analysis of the southern section.

The book is properly titled. The range and speed of change in southern politics have been startling. Political "reforms" whose pursuit 25 years ago seemed endless or fruitless have been widely attained. Electoral behavior has been modified in fundamental aspects. The central figure in the old southern politics seen by Key, the Negro, now has a radically changed role.

The political inquiry that later became Key's broad study of southern politics was originally conceived as a study of southern poll taxes. Those who aspire to make structural changes in the context of politics can take heart from southern experiences during these last two decades. Gone are poll taxes, gone is the Georgia county unit system, gone are all legal and most extralegal restraints on black voting, gone is legalized segregation, gone is the old legislative malapportionment, gone are other ancient shackles imposed on Republican minorities.

The volume demonstrates that modifications in the substance of southern politics have been enormous, a consequence of changes like these and of changes in social and economic conditions that bear directly on political life —e.g., migrations out and in, growth of cities and suburbs, enhanced economic income for the region, altered distribution of that income, the civil rights movement. The dominance by the Democratic party that constituted the most distinctive political feature of the Old South is so radically changed that in 1972 all eleven former Confederate states voted for the Republican presidential candidate. At all political levels Republican candidacies and officials are sufficiently numerous to constitute a revolution in the conditions of 25 years before. The significance of southern third-party presidential candidacies is potentially greater now than ever before—across the nation and across the region—a consequence of national social and political tendencies and reactions to them. Southern defections from the national Democratic party conspicuously increased in the Congress during the past generation. Rural dominance of state politics has declined or disappeared. Voting shifts within as well as between the parties signify a more independent and responsive politics than prevailed during the decades dominated by one-party factionalism.

Most important, southern blacks and their interests are no longer simply the objects of reaction, the ploys of demagogic debate. Blacks now constitute an affirmative force whose importance is reflected in the number of ballots they cast and in the deference shown that fact by black and white candidates. Their political participation is significantly a consequence of external intervention by federal courts, federal legislation, and federal executive initiative. The "race issue" is not dead. In fact, the *Brown v. Board of Education* decision and companion developments of the civil rights movement precipitated what Professor Havard calls the third major crisis in southern politics, the other two having been identified by Key as the Civil War with its aftermath and the Populist movement of the 1890s. In many places, racial harshness intensified in the 1950s and 1960s as force met resistance. But the system held, habits and attitudes thought unalterable gave way, and advances, often at fearful cost, were won. The focus of national racial concerns shifted beyond the South, perhaps the greatest break of all with the past.

What used to be solid about the South is largely gone. The diversities that Key emphasized are greater than ever. They are set forth in this admirable book from which all who seek to understand changing American politics will benefit.

ALEXANDER HEARD

Vanderbilt University

Stability and Change in Congress. By Barbara Hinckley. (New York: Harper & Row, 1971. Pp. 216. $3.50.)

In an "Author's Note" at the beginning of her book Professor Hinckley correctly observes that the recent "rush of research" on Congress "has not been brought together into any synthetic ordering." She offers her book as "a preliminary synthesis." As a preliminary synthesis it is a good book—it indeed synthesizes a variety of materials wisely chosen and gracefully explained. But it also contains a number of flaws inevitable in any effort labeled "preliminary."

Professor Hinckley knows both Congress and the literature on Congress well—and she is judicious about what literature she includes in her synthesis. For example, she properly includes some of the excellent empirical studies she herself has done, but she does not try to summarize most of the roll call studies because these reveal little about the congressional behavior in which she is interested. In general, her book summarizes in pointed fashion the most interesting empirical work on Congress by political scientists after World War II through about 1970 (the cutoff date is a function of the 1971 publication date).

The basic focus of her synthesis is on Congress as an institution that is important because it helps make policy. Given this focus she has selected well the topics she covers. Perhaps the two most notable topics missing are an extended treatment of relations with the bureaucracy and a discussion of the impact of congressional staff. These omissions can be explained largely by the lack of good (or even much mediocre) empirical literature to summarize.

Hinckley's major theoretical theme is that both change and stability in Congress need to be explained in dynamic terms. That is, stability must be explained by a balance of forces that must constantly be renewed. And change must be explained by a shifting of that balance. Given that the post-World War II Congress appeared in 1971 to be more stable than changing, Professor Hinckley concentrates on explaining "the dynamic of congressional stability" (p. 8). She enunciates this theme somewhat confusingly in chapter 1, but in the concluding chapter she states it clearly and also summarizes the argument of the book in two particularly intriguing pages (pp. 198–199) in which she identifies nine interrelationships among such phenomena as stability of membership, leadership, and a variety of norms (for example, seniority). These interrelationships explain the general stability of the institution and also lead to speculation about the key to change in the institution. She concludes that change is possible principally through changing membership, a conclusion I think may be too facile but one that certainly deserves careful empirical exploration.

The shortcomings of the book do not really damage its ultimate utility but do deserve noting. Two relatively minor items reduce the immediate utility of the book as a text. First, the opening chapter is not a clear guide to all of what is coming in the rest of the book. Second, the footnotes are helpful in identifying literature worth exploring further, but an annotated bibliography (and no bibliography at all is included) would have been even more useful.

I have more serious reservations about the treatment of two conceptual matters. First, in the discussion of norms, both in chapter 4 and again at the end of the book, I do not think that Hinckley ever really gives proper attention to how norms change. The process of change is certainly more complicated than simply saying new members bring new norms (because, in fact, as Hinckley herself points out, there is empirical evidence that new members of Congress are already pretty well socialized into congressional norms before they even arrive). Unlike some of her predecessors, she certainly realizes that norms change, but she does not grapple with the tough problem of explaining how.

Second, she identifies the critical shortcoming in the use of role analysis in studying Congress—role perception has never been successfully linked to behavior and so may make no difference—but she devotes a whole chapter to discussing "Representational Roles." This strikes me as an uncritical acceptance of a notion that abounds in the literature but that has little relevance to a book summarizing what we know about the behavior of an institution.

The above problems, however, do not detract from Hinckley's genuine accomplishment: she has applied good sense about Congress and about the political science literature on Congress in summarizing clearly the best of what the discipline has had to say about this ever-fascinating institution for roughly the quarter-century following World War II.

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Youth in Politics: Expectations and Realities. By Sidney Hyman. (New York: Basic Books, 1972. Pp. 436. $8.50.)

"Youth in politics" has received so much attention these past few years, and has been victimized by such an abundance of exaggerated claims and counterclaims, that first reactions to word of a new book in the area are apt to be tinged by a mixture of skepticism and boredom. Still, the subject is sufficiently large, multifaceted, and important that further systematic study is very much in order. It is true, of course, as Scammon and Wattenberg have noted [*The Real Majority* (New York: Coward-McCann, 1970), p. 46] that "American voters are mostly *unyoung*," but young men and women now comprise a higher proportion of the electorate than at any time since the early years of this century. The intellectual training and resources relevant to effective political participation which they possess can be said to exceed that of any previous generation. In 1972, to take one measure, about 40 per cent of persons in the 20–24 age cohort had acquired at least one year of formal higher education, in comparison to 25 per cent of those between 35 and 44 years, and just 17 per cent in the 55–64 cohort. The political mood and activities of young people, moreover, have had an exceptional impact on American life during the last decade. Whether, or to what extent, persons who first came to political consciousness during the "Vietnam era" have gained from the decisive events of this period a frame of reference which will shape their subsequent values and actions—the degree, in other words, that today's youth will act in future politics as a "political generation"—is of considerable importance and deserves examination.

The immediate focus of Professor Sidney Hyman's book is interesting. A wave of campus protests across the country in the spring of 1970, following the U.S. invasion of Cambodia and the killing of students at Kent State and Jackson State, held promise of generating massive student involvement in the November elections on behalf of peace candidates. Speculation was widespread that large numbers of universities would adopt "Princeton Plans," closing for two weeks before the election to permit full-time student electioneering. In this context, Professor Hyman and his associates undertook research to measure what levels of student electoral participation actually materialized; how orientations of the participants to the political system and its processes were affected; what concrete impact the youth "frontlash" had in various congressional contests; and the extent of "backlash" among segments of the public enraged by actions of student activists, a backlash directed in the first instance against candidates strongly supported by "the forces of youth." The research group conducted detailed studies of "a group of representative [congressional] contests, chosen by general type and geographical location after consultation with candidates, party leaders, experienced political reporters, and full-time academic students of American electoral politics" (p. 7). Participating students from "Princeton Plan" and "non-Princeton Plan" institutions were surveyed, and voters were queried to ascertain their reactions to student campaigners.

In view of the importance of the subject, and the considerable planning and energy which went into data collection, one can only be disappointed by the presentation of findings in *Youth in Politics*. The large outpouring of student participation which had been anticipated in the spring of 1970 never materialized in the fall, of course, but this does not account for the defects of Professor Hyman's volume. The eleven chapters on various congressional elections, from the Dellums-Healy contest in California to that between Philip Hoff and Winston Prouty in Vermont, are short, journalistic accounts. They provide casual reportage, not systematic information, on the impact of student participation. Data from the sample surveys are ineffectively utilized. The reader is given little information on how the surveys were conducted, and their findings are scarcely analyzed. A few bivariate tables drawn from them are introduced in the concluding chapters, almost as an afterthought.

Half of the book, including the most interesting portions, was written independently of the 1970 investigations. These long background chapters discuss the evolution in political activism of young whites and blacks, beginning with the "politics of hope" atmosphere of the early civil rights movement, on through the radical disillusionment of the late Vietnam years. Much of the ground is familiar: The movement from civil rights to black liberation; from northern-based forays against Jim Crow in the South, to the university as "staging area" and the "turf of conflict"; from Port Huron 1962, to Columbia 1968. Nonetheless, the commentary is informed, and often insightful. It is unfortunate that the latter half of *Youth in Politics*, presenting data on student political participation in 1970, never builds effectively upon a foundation so carefully laid, and appears as a casual journalistic appendage.

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Emblem of Liberty: The Image of Lafayette in the American Mind. By Anne C. Loveland. (Baton Rouge: Louisiana State University Press, 1971. Pp. ix, 196. $7.95.)

This engaging little book offers a report on "the collective American response to Lafayette . . . over three [sic] centuries" (p. 4)—the response, that is, of "articulate Americans" whose literary remains in the Cornell Lafayette Collection and other archival deposits have been painstakingly examined by Professor Loveland. The account focuses (pp. 26–132) on the 1820s and 1830s when Lafayette was "identified," according to the mayor of Nashville, "with our existence as a nation, with the glory of our republic" (p. 35). In showing how a French Catholic aristocrat came to be hailed by American Protestant democrats, Loveland deftly sketches the dominant features of Lafayette's image: "benefactor of America, disciple of Washington, lover of liberty, friend of mankind, model patriot, agent of the American mission" (p. 134)—in sum, the emblematic American "man of virtue."

This portrait projected not only the affirmations of its creators but also their anxieties. They were worried about the unity and identity of the nation, about the atomistic, acquisitive tendencies of its people, about the decay (as they thought) of public and private morality. They were perplexed by problems of foreign relations, centering in the disputed definition of America's "mission." In all these regards Lafayette's image served the purposes of politicians, editors, and clergymen who wanted to proclaim America's exemplary goodness at the same time that they attacked America's failings. Thus Lafayette's triumphal tour of 1824–1825 furnished both an occasion for rededication to the first principles of the Revolution and an opportunity to reaffirm commitment to the international cause of liberty (as in Greece, for example) without, however, pursuing policies of active intervention on behalf of that cause. Loveland concludes that "like the revival of republicanism, the renewal of the American sense of mission which Lafayette's visit fostered was primarily conservative . . ." (p. 118).

Although Loveland insists on "the enduring appeal of the Lafayette image" (p. 161), she devotes only a single brief chapter to its history from the man of virtue's death in 1834 to World War I when doughboys are said to have returned from the front singing, "We've paid our debt to Lafayette; who the hell do we owe now?" (p. 157). Lafayette's image, "once heralded as a model for Americans," became increasingly "anachronistic and irrelevant" (p. 143), leaving the historian, like Alice, to contemplate only its fading, ambiguous smile. This helps account for the brevity of a book whose strength consists mainly in its treatment of the national psychodrama of the Jacksonian period, in which respect *Emblem of Liberty* complements, though it does not deepen, the studies of Ward, Meyers, Somkin, Tuveson, Nye, Nagel, and others.

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Rites of Way: The Politics of Transportation in Boston and the U.S. City. By Alan Lupo, Frank Colcord, and Edmund P. Fowler. (Boston: Little, Brown, 1971. Pp. 294. $7.95.)

The theme of this volume is that transportation is much too important to be left to the highway engineer. It is a case study of how highway engineers, planners, politicians, and government officials blithely ignore the consequences of their activities. They not only fail to take into account the second and third-order consequences, but in some instances neglect the direct impact of expressway development. The first part narrates the reaction of citizen groups to transportation policy in Greater Boston. Next follows an analysis of how transportation policy is evolved and its impact upon the urban community. Finally, there is a comparative analysis of transportation policy in eight medium-sized American cities ranging from one to three million population.

While most studies of this type look at transportation from the top down, this is a bottoms-up approach—from the perspective of groups, neighborhoods, and communities affected by the bulldozer. It is a sifting of the policies of a half-dozen semi-autonomous state and local agencies "doing their thing" regardless of the consequences to the people in the affected areas. Mostly it is without knowledge of the results of these actions. Through a combination of federal law, ninety per cent federal funding, standard engineering practices, semi-autonomous state and local administration, and the pervasive influence of the highway lobby, we have created a system designed for rural areas that generates chaos when applied to the urban community. The system is designed to build roads and incapable of coordinating highway policy, much less the integration of highway policy with other modes of transportation. Even so-called comprehensive highway planning is so narrowly construed that it always comes up with a need for bigger and better highways without reference to street or parking capacities of the areas served. When we consider the relationships to social planning we are really out in left field.

More important, this is an example of how diverse city and suburban groups can organize to thwart road-building run wild and change entrenched state and local transportation policies:

> . . . before attempts to build expressways into the center of Boston, transportation decisionmaking [sic] in Massachusetts was concentrated in mutually autonomous state and city agencies unresponsive to the public. Their uncoordinated and arbitrary policies produced so much public outcry that politicians effected a change in policy. The reasons for the politicians' entrance were the intensity of feelings expressed in well planned demonstrations, the help provided citizens groups by [advocate] planners and by lawyers with political know-how, and the opposition's geographical and socio-economic heterogeneity (p. 163).

Prior to the Boston experience of the late 1960s, most protests of transportation policies were sporadic outcrys of the persons most deeply and directly affected and were dismissed as the ordinary protests you can expect from any public decision.

According to the authors, public policy in America is largely determined by governmental response to organized interest groups ranging from highway contractors to the gun lobby to automobile manufacturers and the oil interests. For the most part there is no way to sort out and effectuate social and economic policies except by an *ad hoc* measuring of one group against another, a most haphazard process. The wrong groups, those having a commonality of interest in overall transportation policy, often end up fighting one another rather than contesting the policies of the governments and programs involved. Characteristically, citizen groups lack the technical, political, and professional skills necessary for effective organization and for the purposes of challenging the system. By happenstance, these skills were present in Boston, but in most cities they are not and no one speaks for the metropolis as a whole.

Rites of Way is basically a descriptive case study of transportation policy and citizen protest in Greater Boston and this is the major contribution of the book. Very little attention is given to national transportation policy except as it impinges upon the Boston area. There is an attempt to look at transportation decision-making in seven other comparable cities with reference to the actors and governments involved in transportation decisions, the salience of transportation-related issues, and the subjects of transportation controversies. For the most part, however, the data for this comparison are culled from a miscellany of disparate sources which results in a less than adequate comparison of Boston with other metropolitan areas. Though beyond the scope of this research, there is a need for really comparable studies of citizen involvement and reaction to transportation policies in other cities. The authors have taken sides on the issue and make a number of assertions concerning the Boston situation that may or may not apply elsewhere: (1) the use of the planning process to justify preconceived notions, (2) faulty assumptions and misapplied methodologies by those involved in the administration of the program, (3) failure to look at the consequences of highway development, (4) administration by semi-autonomous and uncoordinated state and local agencies, (5) overriding and discounting of citizen protests, and (6) general bureaucratic incompetence. Are these criticisms peculiar to Boston or are they more general throughout the country? The reviewer is inclined to believe that Boston is not an isolated case, but without research in other areas we may never know the extent to which the situation in Boston is replicated.

If Boston is not unique, the real question is how we can condone a system that embarks upon programs costing monumental sums of money and stretching over two decades without consideration of its impact upon our cities and the environment? And, if we have failed using federal policies and guidelines, how can we expect to do any better under a system of general and special revenue sharing where the existing agencies of local government will be given even more discretion to develop individual policies and programs?

This is a case study that should be useful to many of us concerned with public policy and transportation, but the list does not stop there. *Rites of Way* raises many more questions than it answers in terms of citizen involvement, intergovernmental relations, governmental effectiveness, the role of interest groups, planning, and the urban environment. Regardless of the ambition expressed in the title, it is not a major survey of the politics of transportation in places other than Boston.

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From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776. By Pauline Maier. (New York: Alfred A. Knopf, 1972. Pp. 318. $10.00.)

Since 1965, when Bernard Bailyn published the first version of the work which became *The Ideological Origins of the American Revolution,* historians have rapidly accepted a new understanding of the way in which the colonists moved from resistance to revolution. Most now agree with Bailyn that a coherent structure of English opposition ideas gave pattern to American perceptions of British policy in the years after 1763, imparting to British measures a meaning which led irresistibly to independence and republicanism. Much of the current effort in revolutionary studies is directed toward an understanding of the social situation which led Americans to accept this conspiratorial structure of ideas or toward a refinement of Bailyn's story of the process by which it came to have its hold. Professor Maier makes an important contribution to the second of these problems.

Maier's "radicals" are the "informal intercolonial cadre" of local leaders who led the struggle against British measures from the Stamp Act on. These are the men who are sometimes seen as leaders in a struggle over who should rule at home: Sam Adams, Christopher Gadsden, Isaac Sears. In tracing the progress of their perceptions of the nature of their struggle, Maier makes what seems a legitimate claim to be following the development of the most advanced American thought. Her strongly supported conclusions suggest a revision of Bailyn's chronology and major problems for those who see the Revolution as a domestic contest as well as an imperial revolt.

The most important point about Maier's radicals is that they were not very radical at all. On the basis of careful research into the public and private writings of the "sons of liberty," she is led to emphasize the respectable social stations of even the most advanced of the revolutionary leaders, the degree to which their function was to organize and restrain popular enthusiasm rather than to stir it up, and the slowness and hesitancy with which they concluded that restrained resistance must give way to revolt. Maier presents strong evidence that the transformation which made independence and republicanism seem positive goods came with more reluctance and much later in the imperial struggle than Bailyn has led us to believe. *From Resistance to Revolution* re-emphasizes the conservative character of the imperial struggle, re-asserts the primacy of colonial perceptions of Britain in the movement towards independence, and insists on the close correlation of the rhetoric of resistance with the organizing efforts which would ease the transition to republican rule.

To my mind, the revisionary emphasis and the broad definition of "radical" rather weaken the story Maier tells—at least in the years after 1768. A sharp early focus on the Sons of Liberty becomes more diffuse as the revolutionary movement broadens and the number of radicals grows. The conservative emphasis remains too strong, the transforming aspects of the movement receive too little attention, and the thread of development becomes weak. Still, the book will be important to all who are interested in the refinement of Bailyn's thesis. And the strong early chapters, with their wealth of information on the sons of liberty, their inter-colonial network, and their contacts with English radicals, will justify any reader's time.

LANCE BANNING

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The Supreme Court and Religion. By Richard E. Morgan. (New York: The Free Press, 1972. Pp. 216. $7.95.)

In writing on the Supreme Court's handling of the First Amendment's religion clause, Professor Morgan does not tread on virgin ground. On the contrary, this topic has in recent years received wide attention from both scholars and polemicists; in addition to a variety of books best categorized as special pleading, these endeavors have produced distinguished works by such authorities as Mark deWolf Howe, Paul G. Kauper, Philip B. Kurland, and David Fellman, as well as a large number of splendid law review articles. Nevertheless, Morgan's book is a welcome addition to the field, for it is almost completely up to date, and moreover, seeks to pull together the sometimes disparate strands of history, law, politics, and public policy in the crucial area of church-state relations.

Mr. Morgan begins his book with a chapter on "Religion and the Law in Early America." In view of the author's (undoubtedly correct) position that the historical background is important to an understanding of the First Amendment's religion clause, it is disappointing that the drafting of the clause in the First Congress, with which the chapter concludes, is treated cavalierly. Since our knowledge of this event is incomplete, and no clear-cut conclusion concerning the intent of the framers seems possible, Morgan proposes to "lay out the record and suggest where the weight of evidence seems to lie" (p. 21). His "record," however, is too brief and even on its own terms does not warrant the conclusion that "the evidence establishes at least a presumption in favor of the hard-line [i.e. strict separationist], Madisonian position" (p. 24).

First, since the Bill of Rights was drafted in response to the demands of a number of states' ratifying conventions (Morgan ignores the specific proposals made by the conventions, it is only reasonable to ask what kind of religion provision these states sought. Second, was Madison, in Congress, espousing his own strict separationist views, or was he essentially attempting to meet the objections of the states? The answers to both questions seem clear enough. With the possible exception of New Hampshire, the states did not seek to prevent Congress from legislating in favor of religion generally, but only to prevent federal establishment of a single national religion and interference with the state religious establishments. In debate Madison assured the House that he sought no more than was "required by some of the State Conventions." (This too goes unmentioned.) Morgan's conclusion is colored because he "emphasizes Madison's [own] views . . . in giving meaning to whatever language Congress finally voted . . ." (p. 26, n. 35). But why can we not take Madison at his word?

Changing gears near the end of his book, Morgan concludes that the "hassle over historical materials" is "ultimately pointless" since it is the Court's task "to choose *between* conflicting traditions . . . then use the available historical materials to legitimize the choice" (p. 186). Of course, history seldom answers constitutional questions with finality; in the case of aid to parochial schools the problem of "intent" is compounded by the fact that such schools did not even exist in the eighteenth century and that while aid may benefit religion indirectly, it would be confined to the state-mandated secular portion of the curriculum. Yet I cannot agree that the Court should "legitimize" a policy choice through the use of "history" that is susceptible to a contrary interpretation. An intelligent policy choice made within the range of reasonable constitutional possibilities does not need the support of such a fragile reed.

The defects of Professor Morgan's book, I believe, are largely attributable to his covering too much ground in too little space. His "ruthlessness in simplifying and classifying" (p. 183) makes for engaging reading but results also in an occasionally distorted landscape and statements that are misleading at best. A few examples follow: Is it fair to refer to "the abberational Justice Story" (p. 36) merely because Story's accommodationist position has not been adopted by the Court, but "sits high and dry, out of the mainstream of American constitutional law" (p. 40)? Does it make sense to suggest that efforts in Congress to pass a prayer amendment affecting the public schools, may be a diversion from "the intractability of some other social problem (e.g. crime in the streets)" (p. 141) when the drive is essentially small-town and rurally based? Finally, one may, or may not, regard the half-century old decisions in *Pierce* v. *Society of Sisters* and *Meyer* v. *Nebraska* as lending support for aid to parochial schools, but one is no longer entitled to deny these decisions validity under the First Amendment, as Morgan would. The Court's dicta on several subsequent occasions (for instance, in Griswold v. Connecticut, 381 U.S. 479, at 482) have explicitly accorded these decisions a First Amendment status.

Furthermore, though Professor Morgan's writing is mercifully free of academic jargon, it is unfortunately marred by occasional colloquialisms and martial language that seem out of place in a scholarly undertaking: For example, federal prosecutors are referred to as "the Feds" (p. 41), judicial opinions are called "productions" (p. 92), and Justice Douglas who once wrote a dictum that "We are a religious people whose institutions presuppose a Supreme Being," is referred to as "he of the 'religious people'" (p. 138); the Justices drive their points home "brutally" (p. 148), land "stinging punches" in their opinions (p. 171), or else "savage" one another's contentions (p. 131).

Morgan's conclusion is provocative but well reasoned. The "conventional liberal positions" he considers "obsolescent," since "they no longer serve the liberal goal of maximum diversity and individualism consistent with a decently ordered government and society" (p. 210). Thus, in view of declining religious divisiveness, the need for educational diversity, the increasing secularization of church schools, and their popularity with minority group parents, the decision in *Lemon* v. *Kurtzman,* denying aid to parochial schools, was "a step backward"; but the potential for divisiveness in school religious exercises is still such as to justify their prohibition by the Court. "The Court should," in Morgan's colorful phrase, "proceed to turn the old Protestant-tinged separationism . . . (soft on prayers, hard on money) inside out" (p. 207).

Every Supreme Court decision relating to religion appears to have been covered by Morgan; he discusses the statutory as well as the constitutional cases, along with some important lower federal and state decisions. For this reason alone the book is a valuable addition to the literature.

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Community Action and the Poor: Influence vs. Social Control in a New York City Community. By Kenneth J. Pollinger and Annette C. Pollinger. (New York: Praeger Publishers, 1972. Pp. 282. $15.00.)

This book is an attempt to examine the impact of the New York City community action program at the neighborhood level. In their investigation, the authors initially argue that the New York City program under Lindsay attempted to bring the poor or their legitimate representatives into the political system and to increase their political activity. They then suggest two directions citizen participation may take—an influence output or a social control output. An influence output, given the general thrust of the community action program in New York City, will be manifested either through successful service programs controlled and administered by legitimate representatives of the poor or through effective large-scale organization and action by the ranks of the poor themselves. A social control output, in contrast, will be manifested either through unsuccessful service programs or through ineffectual group action by the poor. The general hypothesis of the study is that the "program's operation can best be described as a social control output, with the community action bureaucracy functioning as a social control agent and as another authority with which the poor must cope" (p. 18).

The protagonists of the book appear to be (1) the community action bureaucracy—the Council Against Poverty (CAP), the community action program's policy-making body, and the Community Development Agency (CDA), the administrative staff arm of the CAP; and (2) the Community Corporations and their Delegate Agencies, although the authors at times seem to consider the CC staff as social control agents.

To examine the hypothesis, the authors selected one of the twenty-six designated poverty areas in New York City, the Tremont poverty area, for in-depth analysis during program year 1969–1970. They collected substantial data through participant observation and through interviews with eighty-seven individuals involved in the Tremont Community Corporation (TCC).

In their analysis of the observational data, the Pollingers contend that inadequate fiscal and technical resources prevented the successful operation of the TCC and its Delegate Agencies. They note that only 2.5 per cent of the total budget of each delegate agency was devoted to actual program implementation and that CDA was unwilling to provide technical assistance, particularly with program evaluation.

The Pollingers conclude, on the basis of this evidence, that the community action bureaucracy acted primarily as an agent of social control rather than as an advocate of the poor.

Yet information is also presented which suggests that problems internal to the TCC contributed to the inefficiencies of the corporation and its delegate agencies. For example, the authors state that conflict between black and Puerto Rican members of the TCC Board was rampant.

When the authors examine their interview data, the evidence they muster in support of their general hypothesis becomes more tenuous. The Pollingers state the respondents felt that considerable ethnic antagonisms existed within the Tremont area and that a substantial amount of this antagonism had been a result of the community action program. Also, respondents believe that relationships among Delegate Agencies were more competitive than cooperative. The authors interpret this data to indicate that the community action bureaucracy acted as a social control agent. Such ethnic antagonisms and Delegate Agency rivalries, however, could readily be interpreted as resulting from competition over the allocation of funds to racially homogeneous Delegate Agencies.

In sum, the book suffers from a debilitating conceptual weakness—unsuccessful service programs under the control of the poor are automatically attributed to social control decisions by the community action bureaucracy. Although the authors gathered considerable data for their study, they consistently force that data to support their general hypothesis that the community action bureaucracy in New York City is a social control output rather than an influence output. Rivalries among individuals, ethnic groups, and Delegate Agencies are viewed as a result of social control efforts rather than simply as conflicts over the symbolic and material benefits of the program. It is unfortunate that a study which demonstrates considerable information and insight concerning the operation of the community action program at the neighborhood level does not focus on conflicts within the program, but utilizes a more intricate yet less applicable framework.

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Undeclared War and Civil Disobedience: The American System in Crisis. By Lawrence R. Velvel. (New York: The Dunellen Co., 1970. Pp. xvii, 405. $8.95.)

Undeclared War and Civil Disobedience predates even the Cambodian incursion of spring, 1970. Mr. Velvel's concerns, however, were old before the United States went to war in Indochina. He focuses on four dilemmas: the constitutional allocation of control between the president and Congress over the initiation of American involvement in hostilities; the proper role of the courts in determining that allocation; the validity of civil disobedience as a means of shaping government policy; and the possibility of institutional legislation to prevent American war-making unless authorized by Congress.

Aside from presidential response to sudden attack on American lives or property, Mr. Velvel reads the Constitution to require prior congressional approval for "armed force against another nation or . . . group within another nation," given a "reasonable possibility that any degree of combat" will result "within the near future" (p. 106). To avoid unconstitutional delegation of legislative authority to the executive, Congress must specify "clear upper limits" on the kinds and sizes of the forces that may be used, the kinds of weapons, geographic areas of operations, objectives, and lengths of time that military actions may continue (p. 97).

Mr. Velvel would swing wide the courthouse door to citizen attacks on the legality of government action, especially executive involvement of the country in conflict. The author stresses a "clear distinction between the judicial question of whether the President may constitutionally fight an undeclared war and the political question of whether or not we *should* fight the war" (p. 134).

Buttressed by intricate criteria for distinguishing good protestors from bad, and by belief that America historically depends on protest as catalyst for reform, Mr. Velvel argues that "there *are* times when civil disobedience should be permitted as a means for obtaining constructive change" (p. 215). To encourage desired disobedience, he would have juries acquit good protestors even though they violated constitutional laws. Failing jury nullification of the law, he calls for sentences responsive to whether the protestor harmed persons or property and to "the need to mediate between the goal of punishing people heavily enough to deter them from violating the law and the goal of not punishing them so heavily that they could never risk violating the law no matter how just their cause or how fine their records" (p. 248).

Finally, Mr. Velvel seeks "an institutional method of insuring that at the very moment when war may occur, Congress will be forced to make a clear decision on whether to fight" (p. 256). His solution is legislation blocking all funds for American involvement in combat until Congress authorizes it, except for executive response to sudden attack.

A theme recurrent through the four parts of *Undeclared War and Civil Disobedience* is the American system in crisis: the rule of law beset by executive usurpation, while Congress and the courts stand cravenly by, and citizens struggling for constitutionality are savaged. The rich development of this theme, in fact, is the book's most distinguished characteristic. Mr. Velvel captures the constitutional *angst* caused mainly by Vietnam and ensuing domestic ills.

Satisfying analysis of the dilemmas at hand is another matter. Although insight and sound argument are often present, the scope of the effort prevents thorough inquiry into the book's four concerns; vital problems go begging—for example, the possibility that American use of force may be in the national interest and only the president willing to act, as before World War II. Reliance on social and political salvation through judicial action is pushed beyond the bounds of the practical or desirable, a reflection perhaps of Mr. Velvel's battles in court against the war. His reasoning can be weak, for instance, misapprehension of the legal concept of neutral principles (pp. 186, 331). His use of history can be suspect; for example, he is "not at all sure that the Congressmen who didn't want to fight in World War I were wrong. One can make a pretty decent argument that without our entry into the war, it might have ended in a stalemate rather than in a situation which led to the rise of Hitler . . . and Communism in Russia" (p. 293).

Finally, Mr. Velvel takes on "the problem of how to prevent people in power from doing evil and how to force them to do good" (p. 253), under the imperative "of a certain distaste, not to say loathing, for what has occurred as a result of Vietnam" (pp. 9–10). Like St. George with the dragon, he wields a righteous blade. Cleft, for instance, are Lyndon Johnson, the "paradigm" of a man with "the most malign, even the most vicious, ideas" (p. 86); "the antiquarian and unthinking defenders of everything past who currently occupy too many of our judicial posts" (p. 113); the "classic imbecility and utter impracticality" of Frankfurter's view that malapportioned voters should seek legislative relief (p. 114); "red-neck Congressmen who hate the progressive ideas which demonstrators are fighting for" (p. 119); "the often stupid legal doctrine of mootness" (p. 143); the "Neanderthally stupid" use of the concept of standing to block cases involving injury to the general public (p. 160); the "highly debatable" if not "abominably stupid" objection to distinguishing between good and bad demonstrators on the basis of the values they espouse (p. 188); and "aged rural bunglers and bigots and pork barrelers" promoted by the congressional seniority system (p. 265). The sound of such falling dragons enlivens the text but steadily signals its skew as well.

Footnotes come in three kinds, all separately presented in the back of the book. There is neither table of cases nor index. Thus the way is not easy, nor the burden light for the reader once he leaves the text. Perhaps unified footnotes, a table of cases, and index fell victim to rapid publication, so that Americans "[a]rmed with the arsenal of arguments arrayed in *Undeclared War and Civil Disobedience*" could "force their government . . . to end its lawlessness, to cease the senseless slaughter," in the words of the book's sponsor, the Lawyers Committee on American Policy Toward Vietnam (p. vi).

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Community Economic Development. Edited by John C. Weistart. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1972. Pp. 308. $16.50.)

This book is a collection of 16 articles on governmental programs to stimulate economic development of the ghetto, with primary emphasis on the role of the community development corporation (CDC). *Community Economic Development* is the hardbound edition of an earlier *Law and Contemporary Problems* volume by the same title (Vol. 36, no. 1–2, 1971). Its organization accordingly follows no apparent logic other than that some of the essays were published in the first number of the volume, and the rest were included in the second.

A consequence of this format is that some questions are treated unevenly, with substantive articles followed by simpler discussions of the same issues. The CDC is thoroughly discussed in an initial set of essays, with the various contributors pointing up in detail the problems confronting ghetto corporations. Yet the book concludes with a piece by Stewart E. Perry which, in view of what has gone before, appears to be too optimistic and is too narrowly focused. This essay and the one just before it by Geoffrey Faux would have been deployed much more usefully as two of the three opening contributions to the book.

Random organization, however, does not significantly limit the book's overall usefulness. It is a valuable source of information about the CDC approach to combatting poverty, and is a worthy addition to any serious library concerned with black capitalism, ghetto enrichment programs, and the formulation of anti-poverty policy.

The CDC approach referred to in all of the articles involves the creation of business corporations in the ghetto for the purposes of establishing minority-owned and -operated enterprises, encouraging entrepreneurial initiative and facilitating the rise of a black middle class, employing local residents, enticing outside business interests to locate facilities and otherwise invest in the ghetto, and promoting community control of firms. Funding has come largely from Model Cities, OEO, SBA, and other federal sources.

A number of the essays dwell on the dilemmas inherent in an attempt by a CDC to accomplish all of the objectives just listed above. One of the most heavily attacked concepts is the concern to implement community control over the policies of the CDC. Wisconsin law professor, J. A. C. Hetherington, contends that the goal of community control is fundamentally opposed to the much more important goal of profit. In his view, "The effective destruction of any meaningful profit motive is probably the unavoidable consequence of any arrangement for widespread community control" (p. 28). Hetherington acknowledges that community control is written into the Community Self-Determination Act of 1969 and that it is espoused by "important" but "frustrated" black leaders (p. 16). Advocates of the concept, in his view, misunderstand the relationship between ownership and control in most corporations. He also points to the limited resources of indigenous investors and the necessary dilution of local control when stock is sold to outsiders. According to Hetherington, the proponents of community control have missed the fundamental difference between community ownership and control and black ownership and control.

Some of the same arguments are made in the article by Frederick D. Sturdivant. A useful discussion of present economic conditions in the ghetto is provided here, with the author pointing up the general lack of mass merchandising firms, the "mom-and-pop" scale of enterprises, consumer resentment and inefficiency, and numerous externalities that cut into profit margins. Sturdivant advocates economic efficiency over community control and insists upon primary concern with making the minority business both viable and attractive to outside loan officers, investors, and benefactors. Confusion of economic goals with political ones or with the provision of social services can be fatal to ghetto enterprises in Sturdivant's view: "The primary objective must be the establishment of self-sufficient, viable enterprises which are capable of attracting the necessary financial, technical, and managerial resources" (p. 49).

Two, more empirical, analyses lend support to arguments made by Hetherington and Sturdivant. William Strang found that ghetto businesses primarily foundered on the inabilities and insufficient motivation of their managers. Some of the problems specific to managerial weaknesses were inadequate record-keeping, inability to gauge the market with respect to production, accumulation of too many assets, and operating expenses too high relative to ability to pay. Lack of capital and problems deriving from the ghetto environment are also cited by Strang, who notes that black ownership of a business is no guarantee of its success in the ghetto, and may even require a major effort to overcome prejudices within the community. A problem with this analysis is that it relies upon mail questionnaires returned from 7 of 17 "organizations and individuals" who were involved with 792 minority businesses but returned data on only 48. John E. Oxendine and Alvin N. Puryear examined 12 minority-run businesses sponsored by a New York City CDC. Seven failed. Contrary to Strang's argument about inadequate capital, these researchers found that ample financing and removal of personal risk from business failure did not make for success. Instead, "the only consistent ingredient . . . was that the five successful entrepreneurs had a high degree of motivation of profitability, while it was absent in the seven unsuccessful owners" (p. 143).

Several articles detail the problems confronting CDC's in acquiring federal funding and the tax incentive possibilities open to investors. In a clever set of game analogies, Otto J. Hetzel points up substantial inconsistencies between federal rhetoric and actual practice. Too often what began with "come play with me" ends in "you fail." Robert S. Robin notes the present inadequacy of knowledge regarding poverty area tax incentives, and cogently argues for flexible "packages" of incentives that fit varying economic conditions.

The articles most explicitly concerned with the politics of community economic development are Faux's useful history of the Special Impact Program of OEO under Johnson and Nixon, and the opening essay by Milton Kotler. Not surprisingly, Kotler argues that the neighborhood is an important political and economic community. His analysis of the original intent of the famous "maximum feasible participation" requirement in the Community Action Program at best is too simple, and, at worst, it is contradicted by Blumenthal, Davidson, Moynihan, Rubin, Yarmolinsky, Donovan, and others who have contributed to the literature on this controversy. Kotler also argues that reductions in the number of people below the poverty line in the 1960s was largely explained by "staff jobs in neighborhood organizations geared to providing either political education or the social benefits necessary to tie people to political organization" (p. 7). It is not clear whether Kotler means that the poor were politicized by antipoverty agency staff, and were somehow able to translate this experience into economic gains, or that the poverty population was reduced largely because so much of it was employed in staff jobs. Evidence for the first interpretation is at best thin, but the numbers involved would make the second version absurd. Finally, Kotler's argument that neighborhood tax outflows should be retained and politically controlled by local organizations assumes much about the resultant economic allocations and how they would lead to a more vital ghetto economy. While the CDC hardly emerges from the other essays as an unqualified success, Kotler's alternative would have to resolve problems of mixed objectives, economic efficiency, and others analogous to those taken up by other contributors writing about the CDC approach.

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Who's Who in Government, 1st edition, 1972–1973. (Chicago: Marquis Who's Who Inc. 1973. Pp. 785. $49.50.)

This is a straightforward standard biographical reference directory. It makes no claim to inform us about who governs, or whether the ruling elites govern responsively and so forth.

Position and hierarchy are the two key variables here. Its title, however, is misleading, for the reader finds that the overwhelming bulk of this book consists of some 15,000 background briefs of people serving in the U.S. federal government. Selected others, probably around a thousand or two (I did not bother to count), refer to local, state and international officials.

Predictable biographical information is presented: position, education, career, military and professional affiliations, clubs, writings, home and office addresses, etc. The book's primary value, no doubt, is as a handy reference tool for practicing lobbyists and Washington journalists. The book is hardly adequate to justify the claim made in its preface that "The scholar will find essential data for numerous needed and fascinating studies of the person and personality who chooses a career in government service" (preface, no page number).

The book does serve a variety of secondary functions even if in this first edition there is much to complain about. For example, it goes much further than other existing sources in providing information about higher echelon personnel and does provide convenient addresses. It includes too an index of biographies according to both topics and departments. And in a rather forceful though indirect way it impresses on the reader the rich diversity of jobs, career patterns, and membership making up that sprawling conglomerate—the federal executive branch.

One difficulty confronting an exercise of this sort is the very real problem of circulation of elites at the top. White House staff and senior political appointees come and go with some frequency. One gathers the impression that these compilers gave up trying to keep up with this to-ing and fro-ing, for the quality of coverage on the Executive Office of the President is disappointingly inadequate.

But perhaps the most glaring deficiency in this whole enterprise is the virtual absence of any precise discussion of the volume's methodology. A scant paragraph or two is offered, but the information is cryptic and unhelpful. The compilers claim is that their only guide is "reference value." They then suggest that career position was the central rule that determined selection, but do not specify what this means, whom it includes, whom it excludes, etc. It is surely to be hoped that in future editions the Marquis' staff will put more effort into informing their readers about these important criteria. This would not only make the book more useful but it would also provide the needed assurance that the publisher is not listing persons merely because people desire to be listed, or worse, because they view those listed as especially likely purchasers.

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On The City's Rim: Politics and Policy in Suburbia. By Frederick M. Wirt, Benjamin Walter, Francine Rabinovitz, Deborah Hensler. (Lexington, Mass.: D. C. Heath and Company, 1972. Pp. xiv, 252. $12.50.)

The authors of this short collection of essays have constructed an excellent summary of the empirical literature on the American suburb. The result is a frontal assault on the prevailing mythology of the suburb—a mythology which has not only been perpetuated in the popular media but which has also been preached all too frequently by social scientists. Although some of the studies reported are new, most of the material upon which the authors base their essays has been readily available and cheerfully ignored for years. The book should be depressing to urban scholars, since the very fact that it was necessary to write it testifies to the pitiful smallness and shocking inattentiveness of their audience.

The unifying theme of the essays is the heterogeneity of American suburbs. Again and again the authors demonstrate that suburbs differ socially, economically, and politically. To those who have accepted this fundamental fact of urban life, the essays may appear redundant. They are not, however. Each brings to bear data which drive one more nail in the coffin of the assertion that suburbs are monotonously homogeneous. The authors clearly wish to leave no room for the superficial analyst of the suburb to escape. In Part Two, Wirt and Walter do a particularly effective job of presenting electoral data to demonstrate that the suburbs are not consistently Republican enclaves and that party support varies from election to election as well as from suburb to suburb. Hensler's discussion of the literature demonstrating attitudinal variation between suburbs on major social and political issues in Part Three further reinforces the claim of suburban heterogeneity.

The book is weakest in Part Five where Rabinovitz attempts to suggest some productive approaches toward a national suburban policy. The weakness here is not attributable to errors of omission or commission on the part of Professor Rabinovitz. In his introduction to the book, Robert Wood points out quite correctly that the empirical studies reported lead inevitably to a complex view of the suburb and that complexity "complicates prescription" (p. xiv). After convincingly demonstrating that the old assumptions and generalizations were inaccurate and useless as a basis for policy, the authors are unable to offer others to replace them. There is really no basis for a national suburban policy. It is not surprising, then, to find that Rabinovitz is forced to abandon the task which she set for herself. She concludes that the best policy would be to ignore governmental units and to focus national policy on providing funds to people in need. She prescribes a greater emphasis on social problems and direct aid to individuals (pp. 208–210).

Such an approach to national policy is, of course, no more suburban than it is urban or rural. What Rabinovitz appears to be suggesting in recommending that jurisdictions be ignored is that in a modern, complex, and highly urbanized nation the distinctions between rural, urban, and suburban have become so blurred as to be useless. The data presented in this book and elsewhere appear to support this position. The only real policy alternative, then, is to focus on people rather than governments.

While such an approach is attractive, particularly to neopopulists among us, it does have its drawbacks. Although Rabinovitz correctly asserts that the suburbs are virtually unorganized for the purpose of affecting national policy, this does not necessarily mean that an attempt to develop a national policy of direct aid to individuals would not be effectively resisted. The ill-fated attempt of the national government to wage war on poverty by dealing directly with community action agencies without benefit of jurisdictional participation by municipalities clearly indicates the nature of the reaction. If the imposition of metropolitan government is politically not feasible (p. 206) largely because of the threat which it poses to existing political structures, then it would seem probable that channeling federal aid directly to citizens would be less feasible politically by virtue of the greater threat posed to existing political structures.

Those who are policy-oriented will probably find this book depressing. Some will choose to ignore it as a result. It does not deserve such a fate. The fact of suburban heterogeneity and its unpleasant effects on national policy must be faced. If they make us more than a little unhappy, so be it. We cannot continue to design policies and programs for a world which does not now exist and which probably never existed.

Every serious student of urban politics needs to consider the material presented in this book and to come to grips with it. It is written clearly and concisely and will lend itself to easy use in the classroom. The authors are to be congratulated for bringing together current knowledge of the suburb in such a manner as to gain perhaps a larger and more attentive audience.

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Comparative Judicial Politics. By Theodore L. Becker. (Chicago: Rand McNally and Co., 1970. Pp. 407. $6.95, paper.)

Law in Culture and Society. Edited by Laura Nader. (Chicago: Aldine Publishing Co., 1969. Pp. 454. $10.75.)

Becker's book is an ambitious and interesting venture. He attempts to develop a framework in which meaningful comparative (cross-national) research can be undertaken on the judicial process. He adopts the language of structural-functional analysis, but the thrust of his book is to determine how one might discern (a) whether courts exist and (b) the degree to which structures resembling courts have certain characteristics that he believes are prerequisite for the existence of courts.

The two characteristics upon which he concentrates are impartiality and independence. Becker operationalizes impartiality as belief by court participants and members of the political community that judicial decisions are made on the basis of rules which we usually call law, and that leads him to examine the concept of judicial role. Independence he defines as the ability of judges to decide cases in opposition to others who hold political power, consistent with the judge's conception of his own role, even when that decision is adverse to other political influentials. One (but not the only) indicator of independence is judicial review, and Becker engages in a lengthy and interesting discussion of judicial review in the United States and elsewhere.

Becker's enterprise is unusual and stimulating, illustrated from a variety of sources. The questions he raises are important ones, rarely addressed by American political scientists. Yet on finishing the book, while considerably enlightened, I was left somewhat dissatisfied. Becker primarily addresses political scientists even though much of the most interesting work on courts and the legal process has been done by sociologists, anthropologists, and legal researchers. He stresses the sectarian divisions among political scientists who study legal processes that have little interest for outsiders. A more important limitation is that although he sometimes decries the limits of the public law perspective so dominant among political scientists, he himself is nevertheless a prisoner of it. Most of the book draws on examples of public law (e.g., the long discussion of judicial review) with little consideration of how disputes between private parties are disposed of by courts.

The questions Becker addresses are important ones, but do not go far enough to bring us swiftly toward an understanding of how court-like institutions participate in the resolution of civil conflict and affect the authoritative distribution of values. Becker asks what the consequences of having courts are; I would go one step further and ask what consequences flow from having a variety of institutions which resolve disputes. The degree to which members of a political community are willing (or forced) to take their disputes to a public agency, the kinds of disputes taken there, the kinds of remedies available and dispensed are some of the issues such research might address. I am not sure Becker would disagree, but he does not often venture that far from the courtroom in his analysis.

Although I quibble with Becker's emphasis, his book is a solid contribution to comparative analysis of judicial institutions. He provides a framework which enables one to break out of the mold of American institutions; he stimulates thinking about judicial processes in broader terms than the volume by Glendon Schubert and David Danelski [*Comparative Judicial Behavior* (New York: Oxford University Press, 1969)]. The reader must plow through quite a lot of *ad hominem* criticisms of others' work and reconstruct Becker's argument after each digression, but it is worth the effort.

The volume edited by Laura Nader is less concerned with building a framework for cross-national research but contains some extraordinary reports of the "legal" process in non-Western and Western societies. The book is the result of the 1966 Werner-Gren conference and contains chapters written mainly by anthropologists, with some chapters by sociologists and one by a legal researcher. The chapters emphasize the social and cultural context of legal processes that political scientists so often neglect; Nader's contributors usually concern themselves with more than judicial institutions, as for instance the chapter by Herma Kay on the development, application, and effects of an administrative regulation of the California State Department of Social Welfare pertaining to the offer of a home for children of welfare recipients. As an eye-opening introduction to the variety of legal processes and institutions existing in the contemporary world, this volume is the best of its kind. It also introduces the reader to a wide range of prior anthropological work.

Nader's collection can be fruitfully read with Becker's analysis in mind. It reflects (and perhaps settles) the dispute between Max Gluckman and Paul Bohannan about how to compare legal systems. Gluckman has argued for using legal concepts that are widely accepted in the Western world (much as Becker does), while Bohannan urges the researcher to understand a dispute settlement in terms of the analytic system of the natives. For instance, while Americans and Englishmen may think in terms of "contract," others may not. The suggested resolution of this conflict is to consider not only the "folk system" but also the "analytic system" of cultures in devising a more general comparative scheme. Thus one need not lose the cultural context in which a judiciary operates while formulating generalizations which transcend the idiographic observations on which they are based.

The volume does not devote itself to such theory building but many of its chapters provide the materials for it. P. H. Gulliver describes an African community which settles disputes without courts; this community, indeed, appears to have only very rudimentary political institutions and law itself is not explicitly developed. Laura Nader describes a Mexican court which is much more judicial than the one that Gulliver describes; she then compares it to two American courts which also differ widely, although operating in neighboring California cities. A number of other scholars describe roughly similar phenomena in a Navaho Pueblo (E. Adamson Hoebel), in Surinam (André J. F. Köbben) in Northern Togo (Raymond Verdier) and in the Philippines (Charles O. Franke). The volume includes few materials from more developed societies, although one especially interesting paper is by Vilhelm Aubert on the decline of adjudication in Norway between the early 19th century and the present time. The entire book contains a wealth of detailed descriptive material awaiting the kind of synthesis that Becker begins.

Becker and Nader demonstrate the considerable potential for truly comparative analysis of courts, dispute settlement procedures, and the roles and structures which accompany these phenomena (e.g. lawyers, judges, police, etc.). These volumes lead me to anticipate a breakthrough in judicial studies which will soon enable us to discuss non-American and non-Western processes in our classes and to develop a more general theory of adjudication.

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Egypt: Imperialism and Revolution. By Jacques Berque. (New York: Praeger Publishers, Inc., 1972. Pp. 736. $38.50.)

Jacques Berque's long acquaintance with the Arab world, its history, culture, and temperament, has never been better demonstrated than in this, his latest and most exhaustive work, which deals intimately and perceptively with the process of colonization and decolonization in Egypt from the time of Britain's entry in 1882 until the Nasser-led *coup d'état* of July 1952. Professor Berque, currently professor of Social History of Contemporary Islam at the College de France in Paris, deftly weaves for us, with intricate detail and a sensitive historical eye, the human and societal changes and continuities which Egypt experienced as a consequence of seventy years of British colonial rule. But Berque's investigation transcends a single polity and historical period to encompass a theme of far wider significance. Specifically, his organizing question is: "Under what circumstances, according to what correlations, does liberation result dialectically from dependence" (p. 25)?

He assigns himself the task of dissecting colonial Egypt into a historic variation characterized by five principal phases which he entitles: "The Interrupted Country," "Colonial Egypt," "The Hopes of 1919," "Aspirations Cheated," and "Phantoms in Power." Each phase is closely scrutinized to demonstrate the nature of the dialectic and its impact on native Egyptian society. Clearly we learn from Berque's analysis that Egypt, like most Third World nations which suffered grievously under Western imperial domination, managed to withstand the enormity of the colonial impact, remaining faithful to its historic past and traditions as exemplified particularly by the enduring character of the Egyptian *fellah* (peasant-farmer) whose individuality and identity with his land and religion survived amid universal upheavals. Berque rightly points out, in choosing Egypt to illustrate his theme, that "colonization and decolonization are not so much successive phases in time as rival phases within human beings, things and situations. The struggle takes different forms and must be differently evaluated from one period to the next. Demands become more radical and gains accumulate. But the struggle still goes on" (p. 25).

The analysis of Egyptian social history within this dialectic proceeds systematically and comprehensively. Part One of Berque's study details for us the dominant and enduring patterns of precolonial Egypt as manifested, among other things, in rural village life, religious belief-systems, and urban growth and conflict. How this pattern responds and then adapts to colonial intervention is the concern of the author's second and most penetrating part, on "Colonial Egypt," in which, *inter alia,* he attempts to demonstrate that Colonel Urabi's revolt against Britain in 1882 did not have the widespread native support so long assumed. In fact, it was fairly characteristic of the time that elites and masses alike were ambivalent toward their latest in a long line of foreign conquerors. Urabi's revolt was an act of desperation and individual conscience, not the work of a well-organized nationalist movement or military group. In the face of the alliance between imperialism and the Caliphate (the Sultan had repudiated Urabi on February 6, 1882) Egypt's would-be champion of modernity retreated and then reverted to popular piety by turning to the religious fraternities, with their prayers and litanies. Urabi's atavism following his defeat at the battle of Tell el-Kebir was understandable given the time and political conditions under which it occurred. As Berque indicates: "The fact that Urabi did not prove equal to the ideas which he represented, nor to the acts for which he provided the pretext, is characteristic of his time. All over the world, colonialism encountered or provoked similar shortcomings" (p. 122).

Urabi's internal conflict was also appearing at the rural-village level, where colonial rule led to widespread aggression and violence like that which Frantz Fanon witnessed and described in his vivid account of colonization and dehumanization in the Maghrib. Berque's chronicle of village violence in the hamlet of Tamalay best illustrates the process of social decay—from order to brigandage—that was taking place. As Berque informs us, the peasant's rejection of any innovation that did not proceed from himself found expression in two ways: "emigration—he became the rootless vagrant who formed part of the swelling urban population; and brigandage—he roamed the countryside capturing cattle, breaking into barns, and, if need be, killing" (p. 130).

Britain's relatively easy entrance into Egypt was followed by an equally unhindered institutionalization of the colonial system which left Britain in almost complete control of that country's government and administration. Toward both education and culture the colonialists were profoundly indifferent. Cromer himself felt only mistrust and hostility towards native scholars and intellectuals. Militarily the British were in total command, and in fact, once the pacification of the Upper Valley had been achieved by Kitchener and Wingate, "only some 3,800 British soldiers remained in Egypt and the Sudan" (p. 229). The presence of such a meager military force reflected the passivity of native opposition, a result not so much of indigenous and culturally-related indolence (as exemplified in the widely acknowledged *ma'lish* and *bukra* mentality), but of the seeming insurmountability of the imperialist infrastructure.

In the final analysis, however, Egyptian indifference and the *fellah*'s passive obedience to British authority, whatever its genesis, gradually evaporated as reflected in the Dinshawi incident of June 1906 when a number of British officers and Egyptian villagers died in a fracas of peculiar but not unpredictable origin. The swift arrest and sentencing of several *fellaheen* (some of them sentenced to death) aroused the population as never before. Thus for the first time, "the opposition of educated Egyptians made common cause with peasant violence" (p. 238). This common cause was to become a symbolic battle cry of later nationalist movements, particularly that of Mustafah Kamil, which was inevitably to lead to Egyptian independence.

Part Three describes and analyzes the hopes and aspirations felt by most Egyptians on the eve of the Paris Peace Conference of 1919, the frustrations when those hopes were crushed, and the outbreak and significance of the 1919 "revolution" which not only brought to the fore a competent and charismatic leadership institutionalized in the Wafd party but also became "the means through which political values were experienced for the first time in the innermost depths of the society" (p. 316). Parts Four ("Aspirations Cheated") and Five ("Phantoms in Power") cover both familiar and unfamiliar ground, as they describe, via illustrations from literature, culture, politics, religion, economics, and history, the process of nationalist atrophy and decay in the interwar period and the subsequent decline and collapse of British imperial rule following World War Two.

What is distinctly Berque is the careful description of the interplay of the multifaceted components of Egyptian life as they responded to internal and external changes. This is social and political history at its very best. Jacques Berque's vivid description and incisive analysis of a society whose unity and consciousness transcended but was not unaffected by the period of British domination will long remain as an outstanding achievement in artistic and scholarly writing. As in his definitive study on

French North Africa, Professor Berque has displayed the same "sympathy and penetration, the same outstanding breadth of learning" (jacket blurb) in his latest study on colonial Egypt. It is indeed unfortunate that a book of such significance, which should have the widest audience possible, has been so outrageously priced. One can only hope that the publisher will have the sense to put out a relatively inexpensive paperback edition, available to specialists and general readers alike.

JOHN P. ENTELIS

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The Foundation of the German Empire: Select Documents. Edited by Helmut Böhme. Translated by Agatha Ramm. (London: Oxford University Press, 1971. Pp. 271. $11.25.)

The period of unification is one of the best-documented in German history; yet most of the great documentary publications published in the Weimar period stress political and diplomatic history to the neglect of economic and social history. In his selection Böhme seeks to rectify this imbalance by including many documents dealing with the Prussian-Austrian struggle over the renewal of the Zollverein, some of which have not been previously published. This is in keeping with his thesis, first developed in his *Deutschlands Weg zur Grossmacht* (1966), that economic hegemony rather than military superiority was the key to Prussia's victory over Austria. "The actual ascendancy of Prussia was decided," he writes in the preface (p. xi) "when Berlin and Vienna settled scores with each other during the years from 1849 to 1864. In these years Prussia succeeded in manuevering Austria out of the German area in economic matters. This period provided the foundation for the Prussian-*kleindeutsch* (Germany-without-Austria) solution of 1870–1."

OTTO PFLANZE

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Englishmen and Irish Troubles: British Public Opinion and the Making of Irish Policy, 1918–1922. By D. G. Boyce. (Cambridge, Mass.: The MIT Press, 1972. Pp. 253. $10.00.)

In some respects, this is a better book than its title; in others, the content does not quite measure up to the label.

D. G. Boyce, a lecturer in Politics at University College, Swansea, has set himself twin objectives in this elegantly written exploration of the Anglo-Irish confrontation from Armistice Day, 1918, to the outbreak of civil war in 1922. He attempts an analysis of the attitudes of the British public toward the Irish relationship, and he also tries to determine how much public opinion affected governmental policy in those years of Celtic crisis.

The problem, as any undergraduate political science major would have discovered in his first weeks of research, is that in 1920 there were no valid tests of public opinion *per se*. No George Gallup or Elmo Roper sent his middle-aged inquisitors scurrying through the Midlands or out upon the West Country to gauge the response of John Bull to Lloyd George, the Black and Tans, Terrence McSweeney, or the Balbriggan Reprisal. Instead, having admitted early in the work that no one, not even the British Prime Minister, really knew what the stolid Englishman in the streets was thinking about Ireland, Boyce proceeded to fine-comb every major newspaper in the country for clues. Editorials, headlines, the scope and slant of news reports, even the preferences of British journalists covering the scene in bleeding Ireland were logged to establish some basis for measuring the trends of public opinion. Beyond that, Boyce noted the relevant debates and votes in Commons, the by-elections, the speeches of highly visible public men, and perhaps most useful of all, the behavior of major interest groups—the Established Church, the Nonconformist churches, the Catholic Hierarchy, the Trades Unions, and others.

The result, then, if not what might be legitimately regarded as an analysis of public opinion today, is nonetheless an immensely informative—and unusually readable—treatise on the forces that shaped opinion and clearly influenced the course of Britain's decision making on Ireland. Much of it will come through as more than vaguely recognizable to Americans of the Viet Nam Age: vigorous and angry protests at government policy by High Church leaders; casual indifference by Nonconformist ministers; a very pro-imperial stance by the Catholic Archbishop; bitter denunciations by rank-and-file Catholic priests; trades-union leaders more interested in unemployment at home than solidarity with Irish fellow-workers; newspaper reporters, threatened bodily by British forces in Ireland, sending home a daily diet of chilling columns that could not help but shame the British conscience.

What is more, Boyce has made himself the master of the documents relating to that particular stage of the Anglo-Irish conflict, and he is able to carry off his second objective, plotting the impact of external opinion on the Government's policy, with genuine success. In fact, so fine a narrative does he spin of those years of Irish struggle for independence and nationhood that the book might be strongly recommended for that virtue alone. But it is not just another short history; it is a first-class case study of a democratic government trying to subdue a segment of its own body politic and failing, not only because of the tenacity of the rebels, but because the government could not carry on without the full support of its own people for iron methods of suppression. Or as the author concludes:

> The British concession of dominion status in 1921 was made, not because Great Britain had been militarily defeated, but because the government had lost the support of public opinion and could no longer wage war in the face of public criticism of its policy. For it was the revolt of the British conscience, not the defeat of the British army, that obliged Lloyd George to seek terms of peace and settlement with Sinn Fein (p. 180).

If, however, Lloyd George was ultimately obliged to yield to Sinn Fein a pearl of greater value than the "Home Rule" for which John Redmond had so valiantly struggled before the Great War, he was by no means empowered, even had he so wished, to grant Ireland complete independence. His treaty with the Irish could not abjure their obligation of deference to King and Crown and Imperial commitment. That stance the molders of British opinion would never have tolerated. Having outmaneuvered the Irish plenipotentiaries on Ulster, he could deliver his ultimatum: sign the Treaty or face "immediate and terrible war," knowing he would be backed by the country, at least for a time, because the symbols of Empire were mightier than the stings of conscience, and if the matter were put to the people of giving Ireland a Republic or preserving the Empire, he had no doubt which they would choose.

Boyce is eminently clearheaded and informative in his analysis of this British agonizing toward a "peace with honor," though, parenthetically, he seems not to grasp why De Valera could not accept the Treaty with grace, to acknowledge that he too had a constituency and a "public opinion" to which he was ultimately answerable.

As a final point in its favor, *Englishmen and Irish Troubles* will be of considerable value to the general reader who is seeking a broader understanding of the current malaise in Northern Ireland, for it treats the Ulster aspect of the issue with intelligence and perception.

PAUL A. PFRETZSCHNER

Lafayette College

The Origin of Forced Labor in the Soviet State, 1917–1921: Documents and Materials. By James Bunyan. (Baltimore: The Johns Hopkins University Press, 1967. Pp. 276. $10.00.)

Mr. Bunyan has already put all students of the early years of the Soviet regime in his debt by the volume of documents on *Intervention, Civil War and Communism in Russia, April to December 1918,* which he edited, and by his collaboration with H. H. Fisher on the equally valuable earlier volume of documents on *The Bolshevik Revolution 1917–1918.* Both volumes have proved of outstanding use to scholars as a guide to the vast material available, and especially helpful to students who cannot read Russian in order to add depth to their reading by introducing them to primary sources. The present volume, although confined in scope to the more limited subject of forced labor, is a worthy successor to the earlier two. The documents, mostly not hitherto available in English, are well selected, edited and translated, and the introductory sections are clear and concise.

LEONARD B. SCHAPIRO

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Emerging Nationalism in Portuguese Africa: Documents. By Ronald H. Chilcote. (Stanford, Calif.: Hoover Institution Press, 1972. Pp. 646. $25.00.)

Ronald Chilcote's compilation, organization, and publication in English of important primary sources concerning nationalism in Portuguese Africa represent the kind of labor of love which all scholars deeply appreciate but very few are ever prepared to undertake. This volume, the culmination of an effort which Chilcote began in 1959, represents a small portion of his voluminous collection of documentary ephemera presently on microfilm at the Hoover Institution (a total of 2,500 documents).

Following a lengthy introduction in which Chilcote attempts to clarify (not altogether successfully) types of nationalism and to delineate the nature of his documentary collection, the first two chapters contain materials representing both the official Portuguese position and that of the Portuguese opposition. The major black African movements in Angola, Mozambique, Guinea-Bissau, and the islands of Cape Verde, São Tomé, and Principe are the themes of Chapters 3–6. The efforts by the various nationalist movements to achieve unity are documented through 1965 in Chapter 7, and the final chapter includes some of the major resolutions on Portuguese Africa passed by the United Nations and other international organizations. The extremely valuable appendix lists the name, origin, and political position of each of the hundreds of opposition, prenationalist, and nationalist organizations relevant to the Portuguese-controlled territories in Africa. Finally, we should be very grateful to the dedicated soul who had the patience to compile a most complete index to this volume.

Professor Chilcote's yeoman effort fills a great lacuna for those who wish to make their own interpretations of this important aspect of the history of the Portuguese held territories in Africa. It is an indispensable volume for all serious students of this part of Africa as well as for all libraries which have even modest African collections.

Gerald J. Bender
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Korean Development: The Interplay of Politics and Economics. By David C. Cole and Princeton N. Lyman. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 320. $12.00.)

This volume, sponsored by the Project for Quantitative Research in Economic Development in Harvard University's Center for International Affairs, is written by two American bureaucrats—David C. Cole, now an economist with the Center's Development Advisory Service, and Princeton N. Lyman, a political scientist who was then Chief of the Civic Participation Division, Bureau for Program and Policy Coordination, Agency for International Development. In it Cole and Lyman describe South Korea's transformation during the 1960s—especially from 1961–1967—into a country of rapid economic growth and what they believed to be reasonable political stability. The political trends and considerable political history of the period, the dimensions and causes of economic growth, and the patterns and significance of economic planning are described well and in detail by two professionals who served the American aid program in Seoul during the central part of the period described. This is a book greatly to be welcomed.

Although, in an introduction written after the text, the authors note the growth of political repression in Korea, they essentially maintain that economic growth in a developing state can be brought about not through the Apterian "mobilization system" with its "mindless authoritarianism" but can be realized through a complementary development of economic and political forces in which "a transformation of economic structure [has] taken place at the same time as pressures were brought to bear to reduce, rather than strengthen, the degree of authoritarianism in the political system" (p. 2). The authors thus hoped that a fair degree of democracy—albeit of an authoritarian tinge—could accompany rapid economic development and believed they had found this phenomenon in Korea.

Two years have passed since the publication of this book and some four since its final preparation. Unfairly to the authors, we can look back. Of the essential fabric of this argument the past four years have left virtually no shred. The brief hopes they spotted of "mobilization . . . by consensus rather than by coercion"; of "compromise . . . between the new government and its opponents . . ." (p. 74); of the government's "[beginning] to give ground to respect more the demands of public opinion . . ." (p. 76) now appear as flickering lights in a darkly authoritarian landscape. Compromise in any meaningful sense between the Korean government and any other political elements in the country is dead and unlikely soon to be resurrected. The constitution has been overthrown in a government-conducted martial law coup, and the government, through an imposed constitution in several respects more overtly authoritarian than those of Fascist or Communist nations, has created a state subject to one of the most thorough and repressive systems of surveillance known to the contemporary world. The history which Messrs. Cole and Lyman believed—or hoped—would amount to a semi-liberal qualification to the normal authoritarian "mobilization system" path to economic development is now revealed as a scattering of temporary expedients on the road to one of the most authoritarian systems of them all.

Even though the political-economic system did not ultimately accord with Cole's and Lyman's hopes, the story of these years is important and is well, if partially, told in this volume. Moreover there is certainly truth which the years since have not obscured in the subtheme that the energetic concentration of the Korean government on economic growth contributed to the confidence of most Korean citizens in their own capacities and accomplishments, hence to some feeling of political acceptance if not consensus. Although the Park government has, in effect, ceased even such attempts as it once made to be genuinely popular and although repression renders the degree of clarity by two men who observed its details, is support or opposition alike immeasurable, this economic concentration and its fruits surely continue to give the government whatever degree of genuine stability it has; and, for the moment, this degree appears still to suffice. The story of how this took place, told with some of great value. The economic sections have great authority, and the history of Korean politics as it appeared to two American bureaucrats is of considerable interest.

To a degree, the interest is unintentional. The book, in its essential sympathy for the Park regime, omits precisely those parts of the story of economic-political interplay which American bureaucrats making their case are prone to omit. Their story is very much the Seoul-concentrated story which their Korean opposite numbers would have us believe: a story of government planning, of bureaucratic initiatives, of banking, of exports. Yet, however swollen their capital city, most Koreans do not live in Seoul and a picture of how participation in economic growth at the local level was broadened (or narrowed) is a part of economic-political interplay too vital to be omitted as it is here. Where politics and economics meet we usually find corruption; it is massive in Korea touching on significant percentages of the GNP. After all, most political funds for the government come from its close control of the economy. Cole and Lyman, not unsurprisingly, play all this *pianissimo* when not in utter silence. They consistently underplay the role—both political and economic—of the ubiquitous Korean CIA. Nor do the authors consider the political and economic effects of the great new firms which have risen to prominence in the wake of the Park government, some of which are political fiefs of the new leadership. State politics now milks all these cows, but can we expect to see some *zaibatsu* stage of Korean politics like that in Japan toward the end of the 1920s? The authors provide no assessment. Korean labor conditions and organization are also passed over in silence; we are told the government controls labor but not how nor is the political-economic future of labor adequately appraised. Such omissions reveal something about Korea and about Korean economic bureaucrats; but they also reveal much about the view American bureaucrats acquire in the Seoul milieu and their failure to assess correctly the fascist direction of the state they were aiding.

Cole and Lyman recount how both carrot and stick brought Korean economic prosperity. Now that coercion has overtaken consensus, will that prosperity produce the political stability the authors once thought they found? The answer must be sought in a future study with quite altered premises.

GREGORY HENDERSON

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The Battle Stalin Lost: Memoirs of Yugoslavia, 1948–1953. By Vladimir Dedijer. (New York: The Viking Press, 1970. Pp. x, 341. $8.50.)

The differentiation of Communist systems has been a challenge to political scientists, and at last, the response is bearing results. Yugoslavia's "exceptionalism" as early as 1948 cast doubt on the utility of the monolithic perspective dominating Communist studies of that time. We know now that the "exceptionalism" was not unique, for the intervening variables of economic development, political cultures, and the genesis of particular systems profoundly modified the seeming commonalities of the Communist systems. The years 1948–53, the primary focus of Dedijer's work, were decisive in Yugoslav development.

An important partisan leader, Dedijer gives us an idiosyncratic case study of that segment of Yugoslav history. The 1968 events in Czechoslovakia impelled him to reexamine Yugoslavia's independent socialist course; to reassert its continued legitimacy now under pressure; to lecture the young much too willing to forget those early years; and in drawing parallels between the '40's and the '60s, to suggest that the posture of the Soviet Union had not much changed. Eager to reaffirm the correctness of the utopian vision he held in the mountains during the war, Dedijer offers an interesting if not altogether satisfying view that reveals much by the way of its emphases and omissions.

Outlining the Soviet campaign against Yugoslavia and the Yugoslav response to it, Dedijer highlights anew the decisiveness of Tito's leadership. Tito's self-esteem and intransigence time and again served to shore up wavering support within the party heirarchy. Dedijer vividly conveys the participants' confusion and painful disillusionment. The obvious and enormous legitimacy enjoyed by the Soviet Union and Stalin himself among Communists everywhere virtually precluded the possibility of evil intent. Dedijer reminds his young readers that even the most popular individuals of the 1960s were not exempt, recalling Djilas's "very favorable" articles about Stalin (p. 108), as well as his impractical proclivities. Dedijer for example characterizes Djilas's UN position paper as being "a Marxist disputation in philosophy, with theological overtones" (p. 285).

Dedijer's problems in readjusting his views of the Soviet Union lead him to proffer inadequate explanations: Soviet behavior is due "to distortions in Soviet society" (p. 47). His analytical categories at times harken to the past, as when he speaks of "Albanian feudalists," "obscurantists," and "certain circles" (pp. 194 and 197). Across the years he projects the shock and the slow acceptance of the reality of the wartime agreements affecting the Balkans. These dissonant notes penetrated the utopian vision only with the greatest difficulty. Stalin's signature on secret treaties is seen as the *prima facie* evidence that he no longer was a revolutionary. Dedijer harbors some ambivalence about the Soviet purges and believes that "Stalin substituted loyalty to the Party for the idea of fidelity to communism" (p. 232). Shocked that Stalin would look at his revolutionary admirers as "honest fools," Dedijer acknowledges that it was only in 1952 that he sought to reappraise the meaning of the Yugoslav-Soviet relations. And, in seeking to legitimatize the Yugoslav path, he resorts to the exegetic reference to Leninist authority (p. 208).

Why was Yugoslavia not more decisive in its response? Dedijer's answer is the extant Western animosity, as well as the underlying ties to Moscow, the source of all authority at the time. The United States is seen largely in terms of animosity, though Dedijer's dealing with some Americans proved to be rewarding. An interesting tangent is his reference to Louis Adamic, who labored on behalf of his native land, only to meet a mysterious death that to Dedijer has never been satisfactorily explained. The author however maintains that the core of the answer must be sought in Yugoslavia itself.

Some points remain unclear: Dedijer glides over the Yugoslav role in the Greek civil war. Unelaborated are the tantalizing allusions to the alleged Stalin plan for the piecemeal federalization of Eastern Europe and the amalgamation under the aegis of the Soviet Union. The author's presentation of the inquisition in Eastern Europe would have been enhanced through the inclusion of many additional revelations, particularly in 1968 out of Czechoslovakia.

Dedijer acknowledges the dogmatism and unnecesary toughness toward those unresponsive to Tito's direction in the struggle with the Soviet Union. He discerns two processes in the 1948–53 period: "the strengthening of power as such, and at the same time the ebbing away of absolute power" (p. 303); indeed, the Yugoslav base came to affect the superstructure. When going beyond the period in question, Dedijer falters. He comes to recognize that "socialism's goal cannot be mere industrialization but the raising of human values to a higher level" (p. 325). It is precisely in this area where seminal debates have been conducted in Yugoslavia that one wishes for the author's own contribution. He correctly observes that the Yugoslav revolution was born in special circumstances, giving rise to a participatory thrust "not only within the framework of the various nationalities of Yugoslavia, but also in terms of local self-government. Later developments not in keping with this initial spirit weakened and violated it" (p. 326).

An interesting book, *The Battle Stalin Lost* vacillates between a personal and sometimes frustrating memoir, and an analysis of the Soviet-Yugoslav schism. Nonetheless, every student of Yugoslavia and for that matter of Communist development generally, should ponder Dedijer's version of those momentous events.

Milan J. Reban

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The American Bibliography of Russian and East European Studies for 1966. Edited by Fritz T. Epstein; Associate Editors Penelope H. Carson and Michael E. Shaw. (Bloomington, Ind.: Indiana University Press for International Affairs Center, 1972. Pp. 148. $3.50, paper.)

This volume is the latest in an annual series begun in 1959; future volumes (one may hope that they will not be as greatly delayed) will appear under the auspices of the Ohio State University. Under the distinguished editorship of the historian Fritz Epstein, the book is as comprehensive a guide to books and articles on all aspects of the U.S.S.R. and the Soviet bloc by British and American authors—including their publications in other languages—as could be imagined. Some five-hundred periodicals were examined. Arrangement is by subject, with meticulous cross-listings and a complete author index enhancing the reference value.

John A. Armstrong

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The Night of Long Knives. By Max Gallo. Trans. by Lily Emmet. (New York: Harper & Row, 1972. Pp. 310. $8.95.)

Hitler and the Beer Hall Putsch. By Harold J. Gordon, Jr. (Princeton, N.J.: Princeton University Press, 1972. Pp. 666. $19.50.)

The time and place: Munich, the *Bürgerbräukeller*, evening of November 8, 1923. In the main beer hall, a right-wing mass rally is off to a rousing start: foaming mugs and patriotic speeches. The programmed main event: a major policy address by the recently appointed State Commissioner (read: dictator with emergency powers) Gustav von Kahr, a conservative with monarchist leanings, somewhat uneasy now in his role of conspirator for the establishment among dozens of lesser conspirators without the law.

Shortly after 8 P.M.: commotion at the door. Adolf Hitler, waving a pistol, surrounded by a phalanx of heavily armed *SA* troopers, forces his way into the assembly, pushes his way through a basically antagonistic crowd, reaches the speakers' platform. A volley of shots (probably fired by Hitler) rips the ceiling. Now, sudden silence, bred by fear. Kahr, General von Lossow (commander of the Bavarian division of the *Reichswehr*), and Bavarian State Police chief Seisser are herded at gunpoint into a back room and harangued by Hitler to join actively in a national revolution just begun and headed by himself. Fifteen minutes later, Hitler reappears, and moves the grumbling crowd to cheering frenzy. The captive triumvirate, seemingly cowed, pledge their support. The meeting breaks up in euphoria, all depart, presumably to bed to gain strength for the morning's task. Instead: a night of confusion; the triumvirate's visions and revisions, followed by the abortive armed putsch; arrest and trial of Hitler and associates; conviction of the ringleaders who are sentenced to a period of comfortable detention at Landsberg fortress. . . The scenario, part *opera buffa*, part nightmare, is familiar.

Professor Gordon, in a massive exercise of scholarship, has now assembled what must surely represent *all* pertinent and available sources not only on the beer-hall putsch, but also on its genesis and aftermath. The result is both admirable and frustrating.

On the credit side, the almost monumental depth of the author's research and the resultant scholarly apparatus command awed respect. The basic structure of his account (which comes in three major parts: I. The Contenders in the Struggle for Power; II. The Conflict; III. The New Political Milieu) is commendably sound, even though it skips rather lightly over the struggle in the Ruhr, the resultant currency collapse, and the desperate efforts of the Stresemann Government to regain control in the face of what has almost become national disintegration. Also much to Professor Gordon's credit is his striving for fairness in assessing the personalities, motives and actions of the participants in the drama (his General von Lossow, for example, emerges as a much more believable, even engaging, certainly tragic human being than in previous accounts).

Bright light can produce strong shadows. The author's quest for total reconstruction has resulted in a sometimes overwhelming array of *minutiae* which tend to obscure his main lines of argument and exposition. Nor can his style be said to do justice to the drama of the occasion. All too frequently an involuted syntax forces the reader to ponder the author's meaning and intent. Nevertheless, this work may be classed as definitive in the sense that it is unlikely that fresh material will come to light which may tempt another writer to reopen this subject in our time.

Still another recent attempt at total recall involves a sequel to the events discussed above: *The Night of Long Knives* by Max Gallo chronicles the liquidation of Ernst Röhm and associates, the destruction of the *SA* as an element of revolutionary political power in the Nazi state, and the gangland slaying of uncounted victims of political or personal vendettas carried out with great verve on June 29–30, 1934 by men then grasping for ascendancy in Hitler's regime. Here, however, all parallels with Professor Gordon's work end, for Mr. Gallo, a French journalist, writes fluently, often grippingly about what Otto Strasser has called "the German St. Bartholomew's," with minimal recourse to research in depth and no scholarly apparatus at all.

As popular history, Mr. Gallo offers a rousing suspense story, featuring as chief plotters Himmler, Heydrich, and Goering, busily manufacturing damning evidence against former comrades-in-arms, and joining an uneasy alliance with military, industrial and conservative political leaders fearful of the *SA*'s radical threat to the *Reichswehr* and to the financial and industrial establishment. Pushed by these factions, himself in an agony of indecision, Hitler forces himself to make a move which, for a variety of reasons, he dreads. The fact remains: the old Field Marshal lies dying. General von Blomberg's offer—for once the "Rubber Lion" meant business—to trade the *SA* for the Reich's presidency, prompts the Führer. The unwary Röhm (though hardly an object for righteous compassion), his followers and protegés, and scores of others quite uninvolved are doomed.

Mr. Gallo's somewhat irritating device of telling his story in a series of hectic flashbacks and shifts of scenery, with impressionistic-pointillist effects, does not really obscure the events, which are described with reasonable accuracy, even though the author relies almost exclusively on memoirs, secondary works, and newspaper accounts. Unfortunately, he gives little weight to the bias of his sources: to accept, for example, Franz von Papen's accounts of his motivations and actions at face value seems downright touching. Odd gaffes in the translation are due to the author's frequent use of German sources in a French edition.

Still, *The Night of Long Knives* is a fine tale of horror for the general reader to curl up with on a lazy afternoon or a long evening.

C. F. Latour

Beaver College

British and Soviet Politics: Legitimacy and Convergence. By Jerome M. Gilison. (Baltimore, Md.: The Johns Hopkins Press, 1972. Pp. 186. $8.50.)

There are two basic problems involved in a book-length comparison of British and Soviet politics. The most obvious is that of acquiring a sufficient knowledge of the academic literature on both countries since, with perhaps the sole exception of the United States, no other political systems have been more written about than these two. It would be easy to write a book of this nature with a reasonably thorough knowledge of one system and a superficial knowledge of the other. This is not a charge which can be laid against Professor Gilison who shows himself to be at home in the literature of both fields. The second problem is that of finding a suitable basis for comparison and this Gilison tackles eclectically by comparing both institutions and functions. Such a solution turns out to be not so simple as it might appear. Formal institutional similarities between the British Parliament and the Supreme Soviet may disguise the fact that some of the functions of the House of Commons are performed elsewhere in the Soviet system—for example, in the Central Committee of the Party. If the House of Commons is the forum in which it is of most critical importance for the Government to persuade its party supporters of the wisdom of its policies, it is the Central Committee (containing within its ranks the most important party officials from the all-Union, Union Republican, and regional levels of the Soviet system) which the leadership of the C.P.S.U. must take the greatest pains to persuade. The format Gilison adopts, with long chapters devoted to "Representative Assemblies" and "Parallel Functions of the Assemblies," tends to obfuscate this point.

Just as comparison at the level of "representative assemblies" may obscure important distinctions, so may comparison of "the parties" within the two political systems. Though the organizational problems of writing a book such as this are immense, it is surely only too great a respect for the similarity of *names* which leads Gilison to discuss (and much too briefly at that) departments of the Central Committee of the C.P.S.U. in his chapter on *parties* rather than in the chapter which he devotes largely to the function of policy making. For the equivalents of the minister, junior ministers, and civil servants within a department of British government are to be found not within the Soviet ministry alone but in the ministry plus the relevant department of the Central Committee. Indeed, it should be evident that the typical Soviet minister comes much closer to being the equivalent of the Permanent Secretary of a British department than of a British minister.

Other points of disagreement concerning interpretation and, occasionally, facts can be mentioned only briefly. Gilison exaggerates the extent to which British politicians who have at some time quarreled publicly with the policy of their party find the "way to the top" barred to them (p. 15). Four of the last six British Prime Ministers were at one time very prominent rebels within their parties. On the Soviet Communist Party, Gilison's statement (p. 21) that "systematic, large-scale purges are no longer fashionable as a means of weeding out insufficiently conscious and devoted members" was badly timed, for it has been promptly followed by just such a systematic, large-scale check-up on all the holders of party cards with the purpose of expelling insufficiently devoted members. On the British side of the party comparison, Gilison seems to be unaware of changes in the leadership of several large trade unions which have had the effect of altering the balance of forces at Labor Party conferences, so that left-wing motions are no longer "invariably defeated by large margins" (p. 28). In his desire to stress the similarities between British and Soviet politics more than the differences, Gilison exaggerates when he states that so far as the "democratic" part of democratic centralism is concerned, "the leadership of the British parties and the Soviet party strive for the same sort of democracy within the party" (p. 30). Certainly the *practice* is extremely different, as Enoch Powell would doubtless discover if he were to be transferred for a day from the British Conservative Party to the C.P.S.U. Gilison is wrong when he states (p. 68) that "as of the beginning of 1968, working-class members comprised 52.2 per cent of the C.P.S.U." The proportion of working-class members at that time, according to the official figures, was 38.8 per cent, and though the proportion has subsequently risen slightly, it remains well short of half the total membership.

One of the most controversial sections of Gilison's book must be that in which he discusses the roles of Prime Minister and General Secretary and Cabinet and Politburo. So far as policy making is concerned, the "ultimate decision" is, he suggests, taken by the General Secretary and Prime Minister rather than by Politburo and Cabinet (p. 125). I should be inclined to reverse the order in both cases. Gilison states (same page) that "the growing importance of the choice between Prime Ministerial candidates" is "a prime factor in the outcome of elections." This common journalistic assumption has little evidence to support it. Harold Wilson enjoyed a long public opinion-poll lead over Edward Heath in 1970 at a time when the opinion-poll popularity gap between the two parties was much narrower and when in fact the Conservatives won the general election. Gilison further states that "the fact that the Prime Minister enjoys more power over his colleagues than the General Secretary does over his seems indisputable" (p. 129). But this "fact" is highly disputable. "It is important to recall" (we are told, p. 149) "that the most sweeping leadership purge of the last decade was carried out not by Khrushchev or Brezhnev, but by Harold Macmillan in 1962." The restriction of our recollections to the "last decade" is an artificial one. In 1962 Macmillan dismissed a third of his Cabinet and was careful to sack his friends rather than his enemies, the people who had gone as far as they were likely to go in the leadership stakes rather than his potential rivals. Five years earlier Khrushchev used the power base he had built up in the Secretariat and Central Committee to remove from membership and candidate membership of the Politburo a similar proportion of colleagues, but with the important difference that these were people who were opposed to him and who included all his most senior and dangerous rivals. It is true, of course, that in 1964 Khrushchev was to discover a new rule of Soviet politics—one long familiar to British Prime Ministers—that to ignore or bypass the views of leading colleagues is ultimately to court disaster. It is fair to say that the relationship between the General Secretary and the Politburo has become much more like that between the Prime Minister and Cabinet than ever it was in Stalin's day, but the political resources at the disposal of the General Secretary are greater than those which the Prime Minister may draw upon, and in recent years Brezhnev, like Khrushchev before him, has shown signs of becoming *much* more than a first among equals within the collective leadership.

A general problem of Gilison's approach is that he tends to adopt a very short-term perspective of Soviet politics and virtually excludes the Stalin years from his discussion. Thus, he is able to conclude his comparison of British and Soviet politics by asserting (p. 174) that "both systems are stable, within our previous definition of gradual, orderly change, and are supported by a very wide social consensus." For a number of reasons, among which may be numbered memories of the thirties and forties and the continuing significant role of the political police, it is reasonable to suppose that a large majority of Soviet citizens accept, without too much questioning, their political system. Given the alternatives, this is not surprising. One is reminded of Margaret Fuller's announcement: "I accept the universe." And of Carlyle's reply: "Gad! she'd better!" It seems a little odd, though, to equate the stability of a political system which since the late seventeenth century has evolved by a process of peaceful change with one which by 1940 had physically destroyed 70 per cent of the Central Committee elected in 1934, not to mention several million less noteworthy citizens.

These criticisms notwithstanding, much of Gilison's study displays a shrewd and realistic appreciation of the ways in which both the Soviet and British political systems have been working in recent years. It is the first serious comparison of the Soviet polity with a Western one since Brzezinski and Huntington's pioneering work, *Political Power: USA/USSR* (1964), and constitutes a creditable attempt at fulfilling a formidably difficult task.

A. H. Brown
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The Culture of Childhood: Child's-Eye Views of Society and Culture. By Mary Ellen Goodman. (New York: Teachers' College Press, 1970. Pp. xiv, 167. $6.25.)

Many readers will remember Goodman's fine book *Race Awareness in Young Children,* a model of patient and sensitive research. This book, her last, is sketchier and less focused, but has the same virtue of empathy with its subjects. Goodman argues forcefully that it is important to understand the child's perspective on its world; and that young children have greater powers of perception, reasoning, and emotional control than they are commonly credited with by adults—at least in the U.S.A. She argues equally strongly that many received psychological theories of child development, cognitive no less than dynamic, are ethnocentric, and can be refuted quite simply by anthropological evidence.

The Culture of Childhood is really about these themes, not about what the title suggests. Goodman makes no reference to the Opies' superb collections (in *The Lore and Language of School Children*) of customs, chants, rhymes and songs. The book is a collection of snippets of psychological and anthropological observations of children, arranged in roughly chronological order of growing up. Some, such as the bits on Goodman's own studies of the social perceptions of Japanese children in the 1950s, are gems. Others are not. The book could be useful as a starting point in the anthropology of childhood, but does not develop many ideas. It is descriptive and polemic rather than interpretative.

In the final analysis, it is the polemic that is the most interesting. Anthropology used to be, in broad terms, a science that gave comfort to the Left, pressing a case for the brotherhood of man and presenting models of alternative forms of social organization. Goodman, however, writes from a distinctly conservative point of view. The two central themes are developed as a sustained polemic against permissive childrearing "experts" (the term itself, complete with inverted commas, being characteristic of the contemporary Right's demonology). It winds up with a splendid summary of "lessons from the cross-cultural record"—that believing in peer group influence leads to accepting peer group tyranny, that the generation gap can be bridged, that if children are undisciplined they grow up to be criminals, that an orderly society needs duly constituted authorities and respect for law which must be inculcated in children, that clamor for rights will produce indifference to responsibilities. No wonder that wise parents and teachers ought to find the cross-cultural record "not only instructive but also emancipating" (p. 158). One doubts whether the children will.

R. W. CONNELL
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Uganda: A Case Study in African Political Development. By Peter M. Gukiina. (South Bend, Ind.: University of Notre Dame Press, 1972. Pp. vii–xii, 190. $7.95.)

A book on political development in Uganda is a welcome addition to the literature on a vastly underwritten country. Political scientists came to Uganda after colleagues working in other disciplines, and the scholarly literature remains scanty and episodic. Gukiina's study is the first attempt to present a comprehensive analysis of national politics in Uganda covering the Obote years (1962–1971).

The merit of this volume is that it represents a Ugandan's efforts to correct the deficiencies in the literature about his own country. Readers will find that the approach is historical and traditional, the style lucid and unpretentious, and the anecdotes engaging and amusing. The author utilizes his experience as a participant-observer to give the reader a sense of local political culture. For example, Gukiina relates that shortly after the British returned the *Kabaka* (king of Buganda) from exile in 1955, the colonial Governor—who undoubtedly chose to act according to proper European etiquette—was observed pouring tea for the traditional monarch. This incident was widely publicized in local Ugandan newspapers. In Ganda culture, it is the duty of children or servants to pour drinks. As far as the local populace was concerned, the Governor's behavior amounted to a tacit acknowledgement of a servant-master relationship with the *Kabaka.*

Uganda's historically paramount heartland group—the Baganda—dominates the previous literature, and all too often Uganda's other ethnic groups are treated only indirectly or incidentally. By explicitly recognizing the diversity of Ugandan tradition, this study provides a useful corrective to the imbalance. In the process, Gukiina dispels many of the persistent myths about "tribalism" in Africa. Finally, the author's account of resistance to colonial intrusion (which at times included collaboration as well) is informative and should be read by scholars interested in imperialism and the concepts of domination-subordination.

On the other hand, despite the paucity of literature, the major limitation is that the author's contribution is largely restricted to a chronology of events. The problem is not that Gukiina's analysis is flawed by major errors. Rather, the reader will be disappointed to find insufficient conceptualization and interpretation. Fleeting allusions to Truman's research on interest groups and an abbreviated summary of Eastonian systems analysis (which is never applied to Uganda) hardly fill the void. Gukiina's major theme appears to be that because cross-cutting loyalties do not exist among members of major ethnic units, Uganda lacks ". . . the right kind of consciousness that would have made democratic government possible" (p. 12). Unfortunately, the author fails to recognize that grafting Western theories onto the grid of African politics is a dubious venture. In highly fragmented countries characterized by severe structural inflexibilities, the dilemma of control versus participation is much more subtle than Gukiina's notions about dictatorship versus democracy would imply. On balance, Gukiina displays little concern for ordering concepts or methodology, and his book will not satisfy theoretically minded political scientists.

Uganda: A Case Study in African Political Development is also flawed by sins of omission. There is no mention of the 1964 struggle for leadership between the Kakonge and Ibingira factions of the Uganda Peoples Congress. The author does not deal with the role of noncitizen Asians in the Ugandan economy. In describing a polity which has been so open to exogenous influence, it is difficult to understand why the author neglects external-internal linkages. Surely a balanced analysis of political development in Uganda must note the nature of dependency relations, particularly the role of multinational corporations. Finally, the discussion of unsuccessful socialist construction and military intervention is insufficient and altogether too brief.

The author appears to be sanguine about the future of military rule in Uganda. General Amin is desdribed as a "national symbol of unity, peace and social justice" (p. 175). Gukiina concludes that ". . . Uganda's political life and environment after the coup are better than ever before for a true democratic and representative political system" (p. 176). The concluding remarks exemplify the danger of writing at a point in time too close to the political phenomena. Events in Uganda since the coup of January, 1971, include widespread murder within the army directed at members of two northern ethnic groups, diplomatic and military clashes with Tanzania, incarceration and arrests of foreign journalists, the expulsion of noncitizen Asians, and anti-Semitic comments emanating from the President's office. One wonders whether the author would like to revise his conclusions in light of overriding evidence suggesting the military's inability to govern effectively.

While Gukiina's study represents an attempt to fill a formidable gap in the scholarly literature, the effort is marred by a lack of analytical rigor, inappropriate adaptation of theories derived primarily from the Western developmental experience, major omissions and inadequate treatment of fundamental aspects of social change in Uganda.

JAMES H. MITTELMAN
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Politologen im Beruf. Zur Aufnahme und Durchsetzung neuer Qualifikationen im Beschäftigungssystem. By Dirk Hartung, Reinhard Nuthmann, and Dietrich Winterhager. (Stuttgart: Ernst Klett Verlag, 1970. Pp. 250. DM 16,40.)

In the Federal Republic of Germany, as in many other advanced industrial states, there has been a rapid expansion in university enrollment in the last decade. This is especially the case in social science. From 1962 to 1966 students in political science, for instance, increased by 132 per cent. This has led some employers to express fears about the creation of an academic proletariat.

The authors, members of the Institute for Educational Research at the Max Planck Society, have selected political science as a case study, because it developed after 1945 as an academic discipline in the absence of any demand for political scientists in terms of their specific qualifications. Not once did any of the representatives of the discipline stipulate clearly how and where the graduates could apply their knowledge. The authors have, therefore, hypothesized that one would expect the graduates of such a new discipline to have difficulties getting jobs.

To verify this hypothesis, they sent a detailed questionnaire to all the graduates (1951–1968) of the Otto Suhr Institute at the Free University of Berlin, by far the largest single source of political science graduates in the Federal Republic. Although they surveyed only one institution, their findings have a high degree of generalizability for the political science graduates of the other universities in the Federal Republic.

In the beginning the Institute operated under the double goals of developing political science as an academic discipline and also considering practical societal needs. Soon the emphasis shifted almost exclusively toward the "academicization" of the discipline, and the course of studies became more and more oriented toward the "reproduction of the discipline." Now once again the question of the relationship between theory and practice is raised.

Considering that political science in the Federal Republic developed without a clear understanding of what political scientists could or should do, the graduates have had remarkable success in securing positions, with only 4 per cent being unemployed. Unlike the situation in the United States, where those in university teaching constitute between 75 and 90 per cent of all political scientists, slightly less than 16 per cent of those in the Federal Republic are in university teaching. Their proportion is, however, increasing. Nearly 17 per cent are in the public service, 13 per cent in primary, secondary, and adult education, and nearly 10 per cent in the mass media. A significant percentage is also employed in industry, business, political parties, and labor unions. In Berlin there are more political scientists in the public service than in the Federal Republic as a whole, but there they also face stiff opposition from the public servants, especially in the higher public service which is dominated by law graduates. Those employed by political parties and labor unions have insecure status and little chance for advancement.

Recruitment methods vary, but 56 per cent used informal professional and personal contacts, and only 22 per cent employed formal methods in securing their positions. These percentages are about the same as in the United States. There has been only a very slight increase in the use of formal methods. Those who did best in their studies placed the greatest reliance upon informal methods. Nondisciplinary criteria frequently became quite functional; thus, nearly one-fourth indicated that membership in an organization such as a political party or a labor union was helpful in securing their present positions. In no case was membership in the political science association useful in job attainment.

More than half of those surveyed expressed a desire to see changes in the possibility of access to positions. As for professional education, two-fifths indicated that they received too little education in empirical social research methods and slightly more than one-third indicated that they got too much what may be broadly termed intellectual history.

The book has a selected bibliography, containing the most significant works, but it lacks an index. While it might have been beneficial if the authors had speculated upon the future supply and demand of political scientists, nevertheless the book is a pioneering work, deserving considerable attention.

John Dreijmanis
St. Thomas University, Canada

A Bibliography of German Studies, 1945–1971. By Gisela Hersch. (Bloomington: Indiana University Press, 1972. Pp. xvi, 603. $12.50.)

Professor Hersch's *Bibliography*, despite its selected character, is more than a "useful teaching and research tool" (p. xiv) for introductory purposes at American universities. The author has compiled literature in the German, English, Slavic, French, and other languages, relating to the two postwar German republics. This guide to developments in the two Germanies covers interdisciplinary territory. Following two sections on "Germany under Allied Control" and "The German Question," the bibliography lists, separately arranged but under identical headings, literature concerning the history, geography, population, international and domestic politics, military and police establishment, legal system, religion, economy, sociology, culture, mass media, and education of the two societies. Only the Federal Republic gets additional space for the New Left.

It is in the nature of such ambitious enterprises that they contain minor and major omissions or false identifications of titles. The selective principles for some fields—e.g., "Germany under Allied Control" and "Cultural Development and Intellectual Life"—appear to be rather narrow and arbitrary, whereas in other areas—e.g., "Economic Conditions . . ."—even ephemeral pamphlets have been included. Important books on political, historical, and educational developments and problems by Habermas, Hamm-Brücher, Golo Mann, Picht, et al. are missing. Habermas's *Strukturwandel der Öffentlichkeit* (p. 279) is a theoretical analysis of an historical category and not a study on the Federal Republic. Agnoli/Brückner's *Transformation der Demokratie* (p. 54) is a Marxist diatribe against and not a history of the Federal Republic. A *bibliographie raisonnée* would certainly have helped to avoid some of these errors. But as excusable as these and other accidents may be in a selected bibliography, the absence of citations of the major liberal newspaper in the Federal Republic (the *Süddeutsche Zeitung*) and of two important intellectual journals (the *Merkur* and the *Kursbuch*) among the periodicals reflects a major knowledge gap at Indiana's *Institute of German Studies* where this *Bibliography* has been compiled.

It may sound rather eccentric to mention the political foundations of a bibliography. But it is highly questionable whether the year 1945 should be taken as an historical point of departure. To cut off the Nazi era from its successors not only suggests that 1945 was an end and a new beginning; it also confirms the sacred liberal prejudice that Austria was a victim and not part of the Nazi syndrome.

Be that as it may, the director of Indiana's *Institute of German Studies*, Louis F. Helbig, sees the *Bibliography* as a political contribution. He writes in his introduction:

> Readers of the *Bibliography* . . . may wonder if there is a connection between the Institute's early beginnings and the policies of the newly elected West German government in the fall of 1969. [The Institute got a generous grant from the Volkswagen-Foundation in 1968, M.H.]. Although this is clearly not a cause and effect relationship, it is certainly not without significance. West Germany's new *Realpolitik* and a more realistic attitude here in America toward the two German states produced similar results: At last, the socialist German state is being recognized as a political force and as an academic subject matter. In this respect the *Institute of German Studies*, like the West German government, is staking new ground (p. xii).

This product of Indiana's new *Realpolitik* deserves praise, despite minor and major shortcomings.

MANFRED HENNINGSEN
University of Hawaii

Republic to Reich: The Making of the Nazi Revolution. By Hajo Holborn. (New York: Pantheon Books, 1972. Pp. 491. $12.95.)

Hitler's Weltanschauung: A Blueprint for Power. By Eberhard Jäckel. (Middletown, Conn.: Wesleyan University Press, 1972. Pp. 140. $8.00.)

Hitler: The Man and the Military Leader. By Percy Ernst Schramm. (Chicago: Quadrangle Books, 1971. Pp. 214. $2.95.)

The study of German in American schools dramatically decreased during World War I, then recovered slightly, and is now slipping along with all other languages in the wake of a new internationalism that assumes all foreigners will learn English. This trend makes the appearance in English translation of significant works of scholarship originally published in German of special importance and utility. The three books reviewed here all fit into this category, although the approaches are somewhat different.

Before his death, Hajo Holborn selected from the major German journal of scholarship on contemporary history—the *Vierteljahrshefte für Zeitgeschichte* of the Institute for Contemporary History in Munich—a number of articles that had appeared between 1953 and 1964. Though written by different authors, the articles chosen all deal with a common theme: the rise and nature of National Socialism. They deal with the end of the Weimar Republic and its parties, the early stages of National Socialism, and aspects of the regime during the years of war. The translations are reliable, and the notes have been retained, with introductory material and a few additional notes supplied. An excellent index is provided—something the Institute itself might well do for the first 20 volumes of its journal.

Readers of a professional journal are not likely to be helped by comments on selections from another journal; it makes more sense to list the pieces included. They are: (1) "The Role of the Personality in the Crisis of the Weimar Republic: Hindenburg, Brüning, Groener, Schleicher," by Theodor Eschenburg, (2) "The Downfall of the Old Social Democratic Party in 1933," by Erich Matthias; (3) "Stages of Totalitarian 'Integration' (Gleichschaltung): The Consolidation of National Socialist Rule in 1933 and 1934," by Karl Dietrich Bracher; (4) "The Reichstag Fire and Its Political Consequences," by Hans Mommsen; (5) "The 'Second Revolution'—June 30, 1934," by Hermann Mau; (6). "The Position of the SS in the Third Reich," by Hans Buchheim; (7) "Goebbels' Speech on Total War, February 18, 1943," by Günter Moltmann; (8) "National Socialist Europe Ideology," by Paul Kluke; (9) "Art in the Political Power Struggle of 1933 and 1934," by Hildegard Brenner, and (10) "SocialDarwinism in Germany, Seen as a Historical Problem," by Hans-Günter Zmarzlik. With the exception of the first article which would now have to be modified somewhat in view of the appearance of Brüning's memoirs, these pieces all accord with the most recent scholarship. Considering the flood of publications in the 20 years since the earliest of these articles, that of Hermann Mau, that judgment is a tribute to the *Vierteljahrshefte für Zeitgeschichte* and this selection.

The German historian Eberhard Jäckel published a study of Hitler's ideology in 1969 that has now been published in a good translation with some added commentary to deal with the problem of citing from translations of Hitler's works. Jäckel's approach summarizes prior scholarship on the issue, but moves forward to a far clearer analysis than hitherto available of the development and inner coherence of Hitler's views of the state, and of the policies Germany must follow. Although the chapter on foreign policy has also appeared in translation in Henry A. Turner, Jr.'s *Nazism and the Third Reich* (New York: Quadrangle, 1972), it is good to have the whole work available to the general public. While most of the material on which this book is based has been published, the general refusal of scholars and the public to take Hitler's thinking seriously has hitherto left us without the kind of careful analysis presented here. At a time when the market is being flooded with psychohistorical and journalistic nonsense about one of the most significant figures of this century, we should welcome a presentation based on scrutiny of the evidence rather than extrapolation from dubious even if juicy tidbits.

A combination of evidence and analysis is offered by the two essays of the late noted German medievalist, Percy Ernst Schramm. Assigned to Hitler's headquarters in the middle of the war to keep the war diary of the High Command of the Armed Forces, Schramm kept and preserved an important record. After the war he both talked to numerous figures from Hitler's inner circle and participated in the editing of some key documents. The two essays that appear here in translation were printed in these editions; one in an edition of Hitler's table talk from the early war years and one in the volumes for 1944–45 of the war diary of the High Command of the Armed Forces. The first attempts a characterization of Hitler as a person and as a political leader, while the second deals with his role as military commander of Germany's forces in World War II. The translator and editor, Donald S. Detwiler, himself the author of an important monograph on German-Spanish relations during the war, has provided both an introduction and additional notes. The fact that Detwiler was a student of Schramm's has an advantage and a disadvantage: The advantage is the close cooperation between the two in preparing this edition, a cooperation explained in the editor's acknowledgement. The disadvantage is a complete absence of critical distance; the proud and self-satisfied Schramm was not infallible. As a contribution to an understanding of Hitler, Schramm's pieces share with Jäckel's book a basic willingness to take the dictator seriously, to probe and not simply reject his actions and his words.

In their various ways, all three books contribute to an understanding of Hitler that places him in the context of his time but takes his own views and perceptions as clues to his behavior, policy, and mode of action. Given the role the German dictator played and the significance of his own words for all such attempts, it is all the more unfortunate that his second book is not available in a reliable English translation and his table talk is not available in a dependable scholarly edition in any language. Such gaps in the literature make the translations discussed here all the more welcome.

GERHARD L. WEINBERG
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Revolutionary Leaders of Modern China. Edited by Chün-tu Hsüeh. (New York: Oxford University Press, 1971. Pp. 572. $4.95.)

For centuries, biographies of famous men have been an important genre of Chinese historiography. Usually chronicling the lives of prominent individuals, biography has continued to be a major vehicle for understanding intellectual and political currents of the modern period.

Professor Hsüeh has used this genre in an attempt to bridge the wide chasm in Chinese studies that still remains between history and the study of political science. He has organized nineteen biographies into three revolutionary periods: the T'aip'ing, Republican, and Communist. The introductions and several articles are contributed by Professor Hsüeh, while the remainder consists of reprints of biographies or specially written essays for the volume by other scholars.

For the most part, high standards of the biographical art have been achieved. In several cases, the reader can recognize credible human beings, with feelings and failings, through the maze of names and other information that the nonspecialist may find distracting. Ernest P. Young's essay on the late Ch'ing revolutionary, Ch'en T'ien-hua, portrays a prophetic figure motivated by despair and eclipsed by more familiar revolutionary heroes. Another interesting figure rescued from relative obscurity is the pamphleteer Tsou Jung, whose eloquent denunciations of the Manchus give the reader a sense of the humiliation which motivated anti-Ch'ing revolutionaries. The proto-modern revolutionary leaders of the T'aip'ing include Hung Hsiu-ch'üan and two of his lieutenants, Hung Jen-kan and Li Hsiu-ch'eng. P. M. Yap's earlier article on the mental illness of Hung Hsiu-ch'üan is also reprinted.

The Republican revolution is represented by such figures as Sun Yat-sen and Sung Chiao-jen. Melville T. Kennedy's biography of Hu Han-min covers the KMT leader until his death in 1936, but says little about his important role in the Nanking government. Howard Boorman's profile of Wang Ching-wei examines not only this "romantic revolutionary's" role in the Republican period, but also the question of motives in his collaboration with the Japanese during the war—attributed to Wang's apparent desire to spare his country from even greater destruction in the face of superior Japanese military might.

Six of the eight biographies from the Communist movement deal entirely with the pre-1949 period. Edgar Snow's biography of Mao Tse-tung is reprinted here, along with essays on other co-founders of the Chinese Communist Party—including Ch'en Tu-hsiu, Chang Kuo-t'ao, and Ch'en Kung-po. Meisner's study of Li Ta-chao is perhaps the most intellectually stimulating piece in the volume, dealing as it does with the sinification of Marxism. T. A. Hsia's essay on the life of Ch'ü Ch'iu-pai is also a penetrating examination of "The Making and Destruction of a 'Tender-hearted' Communist." The volume ends with a revealing portrait of Chou En-lai by Kai-yu Hsu, and some glimpses of the shadowy "iceberg," Liu Shao-ch'i, by Howard Boorman.

As an introduction to the personalities who were so influential in directing the course of modern China, *Revolutionary Leaders of Modern China* is a most competent work and highly readable. Nevertheless, the editor has not gone far enough in certain respects. For one thing, the Nationalist revolution should have been given at least as much space as the T'aip'ing rebellion—but only passing references are made to the Kuomintang movement. For another, the social scientist will be disappointed at the reluctance of the editor to relate the biographical information to various ideas about revolution and elites in general, although Howard Boorman's introduction takes some preliminary steps in the direction of generalization and also notes the obstacles to writing adequate biographies about modern Chinese figures. Finally, the criterion for selecting individuals to be included is not clear. Personalities tend to be drawn from an intellectual elite and are exclusively male. A more representative selection should have included at least one militarist and a woman.

In summary, the volume is to be recommended to anybody interested in understanding a few of the leaders of China's long revolutionary history. But at the same time, the reader must be cautioned against drawing any conclusions about the nature of that revolution or its leaders from the material presented.

ROBERT E. BEDESKI

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Australia and Papua New Guinea. Edited by W. J. Hudson. (Sydney, Australia: Sydney University Press, 1971. Pp. viii, 198. $8.45.)

Australia, like the United States, has never regarded itself as a colonial power, even while administering overseas possessions. Such metropolitan attitudes preclude much discussion of colonial policy and practice. When Hudson, therefore, claims that his book provides an unprecedented overview of Australian rule in Papua-New Guinea he is certainly right. He and his six contributors attempt to cover Australian involvement with the island to the near north from the early interest of missionaries, gold miners, labor recruiters and traders before Papua became a British colony and New Guinea a German colony, through formal control in Papua from 1906 and military rule and mandate administration in New Guinea from 1914, to the joint administration of Papua and the Trust Territory of New Guinea after the Second World War. It is a useful introduction to the more detailed works on particular aspects of Australian colonial rule.

Four of the contributors, including the editor himself, largely summarize their previously published work which makes for unexciting reading, especially when it is done, for example, in Roger B. Joyce's leaden prose on New Guinea before 1906. New ground is broken by Margriet Roe on the territories during the Second World War, a period hitherto neglected except for military operations, but the inaccessibility of the records of the Administrative Unit set up to carry on the routine work of government and a narrow focus of interest reduce the value of the chapter. Heather Radi, in the book's longest essay on the mandated territory, might also have broken new ground, but here the difficulties of evidence are even greater. The local records perished during the war; the metropolitan ones were not available. What is left are the published annual reports, parliamentary debates, and newspapers, although there is still a body of oral testimony of participants which Radi gives no indication she has tapped. As a result Radi's chapter is no advance on the good account provided by S. R. Reed in his *Making of Modern New Guinea* some thirty years ago.

Lacking originality, the book stands or falls by the synthesis it offers. Like any colonial policy, that of Australia's projects metropolitan attitudes into a situation in which they are modified by local, alien, circumstances. Hudson is the only contributor, through his work on Australian policies at the United Nations, to have an adequate grasp of part of this metropolitan background; the others make some loose assumptions, and substitute for knowledge speculation about what may have been the case. For example, one theme to be found in all of the chapters is that of Australian indifference to Papua-New Guinea until very recently. It is true that before 1946, there was not much informed political debate about either of the territories, but it is not true that either the responsible government departments or particular groups and individuals were indifferent. The public record is deceptive, but even if it gave an accurate picture, it would be inaccurate to deduce indifference from it. The limited resources and attention given to Papua-New Guinea may rather be a matter of priorities in a country like Australia which was itself underdeveloped and seeking the aid the contributors imply it should have been giving to its territories.

Much more research remains to be done, both in Australia and in Papua-New Guinea, before more than a superficial overview can be given. This book uses what has been done and digests it competently. But it does not even point to the questions still to be asked, perhaps because the contributors seem to feel that they have a duty to sit in judgment upon colonialism rather than to explain what actually happened.

FRANCIS WEST
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Revolt in Athens: The Greek Communist "Second Round" 1944–1945. By John O. Iatrides. (Princeton, New Jersey: Princeton University Press, 1972. Pp. xiv, 340. $11.50.)

The study of Greek history and politics has undergone a definite revival over the last five years. This renewed scholarly interest has produced a number of works of considerable insight into the political forces that have shaped modern Greece.

Most of the earlier books on the "Second Round" of the Greek Civil War have either been accounts of eyewitnesses, such as that of William H. McNeill, or subjective accounts by various participants in these events. Professor Iatrides in his latest work has given a judicious interpretation to this critical period of Greek politics and has definitely contributed to a better understanding of the events that transpired in Greece from liberation to the spring of 1945. Utilizing existing but also some new evidence (i.e., the papers of U. S. Ambassador MacVeagh), the author concludes that the December 1944 revolt in Athens was not the result of a deliberate Communist attempt to forcibly seize control of the government. It was rather the outcome of a combination of factors ranging from the complete exhaustion of the country after years of dictatorial rule and foreign occupation to the external management of the Greek political problem. Moreover the "Second Round" was due to misunderstandings and suspicions between Britain and the British-backed Greek government on the one hand and the Greek leftist resistance forces on the other.

The outbreak of World War II had left unresolved the question of "who rules" in Greece. While during the war the King's government remained in exile, in Greece the dominant resistance movements were republican and leftist in orientation. The postliberation Greek political crisis therefore reflected the fact that the experience of the war years assured that there could not be a return to the political, economic, and social conditions existing before the war. Thus the fear of radical changes in Greek politics and society propelled Churchill's decisive military intervention in December, 1944, in Athens. For the first and only time in postliberation Europe, a wartime resistance movement fought against Western armed forces in Athens. With Russia scrupulously observing the "Percentages Agreement," Churchill proceeded to consolidate Greece in the British sphere of influence. But what began as a domestic power struggle in Greece soon assumed international dimensions characteristic of the cold war. And two years after the end of the "Second Round" Greece became the birthplace of containment.

Professor Iatrides' account not only shows how strongly Churchill influenced British policy toward the Greek political problem, but also how well U. S. Ambassador MacVeagh understood the weakening of Britain's position in Greece. Not only did the U.S., despite MacVeagh's warnings, refuse to take an active part in Greek affairs until 1947, but when it did, it continued Churchill's policies within the framework and the pressures of containment.

Certainly an interpretation that does not place exclusive blame for the "Second Round" on the Greek Left will not satisfy the cold war advocates. Professor Iatrides' account, if it fails in any way, does so because it is perhaps too judicious. Although his substantive conclusions about the causes and the impact of the "Second Round" are correct, they are not as fully developed as one may have expected. This is particularly evident in Chapter V which analyzes the aftermath of the "Second Round," including the consolidation of conservatism in

Greece and the Anglo-American management of the subsequent Greek political crisis.

Two other points must also be made. One is the absence of an explanation of why the British, at the most critical points of the Greek problem, used Greek Liberal Prime Ministers to promote their policies in Greece, while at the same time leaving untouched an ultraconservative power-structure in the bureaucracy, the army, the police, and the security services. The other point has to do with the author's evaluation of the Greek National Liberation Front's failures. Certainly, as the author shows, EAM was not blessed with the kind of leadership its Yugoslav counterpart had. What is more intriguing is the fact that EAM, which had control of Greece at the time of liberation, preferred to pursue its objectives by political rather than by military means. Nor did EAM ever develop an adequate ideological program of its own applicable to Greek needs. I feel that the study of EAM's failure to become a truly revolutionary movement deserves more attention than it receives in this book and for that matter in all other accounts of the "Second Round."

Despite such weaknesses, this book must be read by anyone interested not only in Greek politics, but also in the origins of the cold war.

Van Coufoudakis
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Les Facteurs locaux de la vie politique nationale. By Institut d'Études Politiques de Bordeaux, under the direction of Albert Mabileau. (Paris: Pédone, 1972. Pp. 411. 45.00 francs.)

In 1967 a French center for the study of local politics (*Centre d'Etude et de Recherche sur la Vie locale*) was established under the aegis of Bordeaux' Institute of Political Studies. Its first major act was to organize a symposium to discuss the local components of national political life. As a consequence of the political turmoil and activity which characterized France in 1968 and 1969—the May crisis, the ensuing discussions and ramifications within the University, a national referendum, and elections—the conference was postponed until November 1969. *Les Facteurs locaux de la vie politique nationale* presents the proceedings of this symposium assembled and organized under the direction of Albert Mabileau.

The work is divided into four major parts, each containing research papers especially prepared for the conference, synthetic introductory and concluding remarks by the chairmen of each session, and discussion and debate on the issues raised in the papers by the participants in the symposium. The work is introduced and concluded by particularly insightful presentations by Marcel Merle, general chairman of the conference. The first section of the book is devoted to an examination of some subnational political and administrative institutions. Special attention is directed toward the role of the departmental organs of political parties (especially with regard to the selection of candidates), the significance of the departmental councils (*conseils généraux*), and the effect of the 1964 regional reforms (which created the C.O.D.E.R.) on the local political-administrative system. The second section is concerned with the local ties of national legislators. In particular, there are analyses of electoral campaigns, the platform statements of candidates for parliament, and the tendency in France for national political leaders simultaneously to hold local elected office. The third section of the book directs attention toward the voters. There is some analysis of the extent of participation in different types of elections (e.g., national and municipal) by different kinds of voters (e.g., rural and urban), as well as some re-examination of existing surveys to serve as a basis upon which to speculate about the importance of local concerns and issues in determining candidate preference. The final section retrospectively examines the local components of national political life by looking at a set of discrete historical cases.

Proceedings of symposia rarely constitute insightful and definitive books. The study under review is no exception to this general rule. Most of the papers seem to have been inadequately prepared. Furthermore, most are empirically based but without appropriate or sufficient data to make the findings reliable. Finally, there are no stimulating, imaginative thinkpieces. There are, however, some interesting discussions—for example, on the significance of national political leaders simultaneously holding local elected office. Some suggestive data are also presented: thus, findings by François Goguel and Odile Rudelle provide additional support for hypotheses which have been advanced by Mark Kesselman on the differential rate of participation in local elections by inhabitants of rural and urban communes. And, there are also some vague (however, not very startling or new) general conclusions upon which the conference participants seem to have reached some agreement—for instance, that under the Fifth Republic, local factors have been of less importance in French national politics than previously (in other words, to use Merle's term, there has been a certain "nationalization" of political life). But, neither the few stimulating discussions, nor the limited findings, nor the points of relative consensus are of sufficient importance to make the book worthwhile. Nor, for that matter, do the insightful, synthetic reports by the various conference chairmen (Merle, Lalumière, Lavau, Dupeux, and Goguel) justify the book. Much of the value of these summary reports lies in their criticism of the papers and their calls and suggestions for future research.

The prime object of the symposium, however, was not to provide definitive answers but rather to stimulate research in the area of French local politics—an extremely laudable goal. With the exception of Kesselman's *The Ambiguous Consensus,* some monographs and articles by Pierre Grémion and Jean-Pierre Worms, and research conducted in Grenoble by Suzanne Berger, Peter Gourevitch, Patrice Higonnet, and Karl Kaiser, there has been a notable and regrettable lack of interest in French subnational politics. The proceedings of the symposium clearly delimit a wide variety of research projects which could and should be undertaken. The participants in the conference quite freely and openly noted the absence of information at their disposal and pointed out a great deal of virgin terrain requiring exploration. It is hoped that students of French politics will respond to these calls to action. To the extent that *Les Facteurs locaux de la vie politique nationale* stimulates research in local-level politics, it may be considered a successful work.

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Pakistan: Failure in National Integration. By Rounaq Jahan. (New York: Columbia University Press, 1972. Pp. 248. $12.50.)

Rounaq Jahan, a member of the Political Science Department at Dacca University in Bangladesh, is the author of this concise, readable study. The book emerged from Miss Jahan's Harvard University political science doctoral dissertation. In this volume the author traces the course of Pakistan's political-economic history in an effort to show the inability and/or reluctance of the country's leaders to cope with the essential problem of nation building; i.e., the molding of ethnic diversity into a unified polity. The focus for Miss Jahan's study is East Pakistan, the more populated but territorially smaller and even more impoverished segment of this once two-winged state. Professor Jahan, who is herself a Bengali, draws the reader's attention to the plight of her fellow countrymen who were demanding justice and equity as well as political understanding. Pakistan's leaders, according to Miss Jahan, were heavily weighted in favor of the west wing and they came to see in East Pakistan a land fit more for exploitation than development. As the author emphasizes, Pakistan's leaders were more inclined toward "state-building" than in the creation of genuine community. The political design which envisaged Pakistan as an integrated state was never seriously pursued either by the political leadership in the pre-1958 period or later under the tutelege of the civil-military bureaucracy. In the epilogue to her book Professor Jahan notes that the dismemberment of the country and the establishment of an independent Bangladesh may be considered a tragedy, but it came as no surprise.

This is a small book, and if one does not include the time examining the numerous tables and charts which the author has carefully selected in support of her thesis, it should take the average reader less than two hours to digest. In part, the volume reads a little like a handbook, the central characters, organizations, and issues being identified and briefly explained. In another way it is somewhat polemical in the sense that little effort is made to examine what was transpiring in East Pakistan between the numerous political aspirants and their seldom harmonious organizations. The author's insistence that the West Pakistan leadership's distorted sense of superiority and latent malevolence is the crux of the dilemma apparently nullified any analysis of internal Bengali politics. True, Miss Jahan does refer to those Bengalis who became the "instruments" of the Karachi and later Islamabad power structure, but unfortunately virtually nothing is said about the incessant struggle for power in East Pakistan, how this manifested itself, and what its consequences have been and still are likely to be in the new state of Bangladesh. While I sympathize with Miss Jahan's overall thesis, it seems only fair to point out that at least up to the black days of March 1971 the East-West Pakistan problem was hardly a conflict between the forces of light on the one side and the forces of darkness on the other. Unfortunately this is an implicit suggestion in the book, and it indicates that the matter will demand continuing scholarly investigation.

The author is correct in stating that the "growing inability and unwillingness to form a national party or a national coalition was an indicator of the process of national disintegration during the Ayub regime," (p. 142) but it is also true that Ayub declined to use ruthless methods to forge such a party or coalition. Pakistan was never intended to be a one-party state. Ayub did aim at a future merger of his Basic Democracies and the Pakistan Muslim League, but little serious effort was put into this activity after 1966. Ayub's dependence on the higher bureaucracy, while unfortunate, was still a more civilized approach to the problem of governing the country in the absence of coherent political organization. There is nothing in the historic record, as in so many countries in Afro-Asia, to suggest that Ayub was determined to impose a single-party apparatus on the country and liquidate all those who disagreed with his peculiar perception of a modern Pakistan nation. Ayub made many mistakes, and he certainly was insensitive to popular complaints, particularly those emanating from East Pakistan. He was no doubt ill-equipped for the responsibility which he assumed in 1958, but it is still difficult to classify him as an unrepentent dictator.

Students of Pakistan studies should welcome this volume, particularly as it represents one of the few books to concentrate its attention on the subject of political decay. The book is perhaps another first step in our attempt to understand the political collapse of a country whose economic indicators as late as 1968 pointed to significant material progress. It is not too late to recall that hardly three years before the establishment of the state of Bangladesh the Harvard economists insisted that Pakistan might well stand as a model for other developing societies. Apparently the foreign experts were no more aware of this impending disaster than were their Pakistani counterparts. Gustav F. Papanek, an advisor to Pakistan's Planning Commission and a former Director of Harvard University's Development Advisory Service is representative of this professional group. In 1967 Papanek wrote: "The economic performance of Pakistan in the last few years has been primarily the result of good economic management—sensible policies and plans. Most important has been Pakistan's pragmatic approach to the role of government and private enterprise." *Pakistan's Development: Social Goals and Private Incentives* (Cambridge, Massachusetts:. Harvard University Press, 1967, p. 270). One can only wonder at this time whether a more equitable policy of distributive justice could have saved the old Pakistan design, but it is fairly clear that many well-meaning people were looking and working in the wrong direction.

LAWRENCE ZIRING
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Conspiracy at Matsukawa. By Chalmers E. Johnson. (Berkeley: University of California Press, 1972. Pp. x, 460. $10.95.)

Conspiracy at Matsukawa is a detailed study of how the Japanese judicial-legal system applied the U.S.-inspired revision of the Code of Criminal Procedure, in force since 1949. The case involved charges of conspiracy to sabotage a government railway train in August 1949, required five court actions, and ended when a final Supreme Court decision dismissed the charges fourteen years later. A revisionist, the author argues that the revised Code was not wisely conceived, was not necessary, was imperfectly applied, and did not improve the quality of Japanese criminal justice. He also offers a secondary case study of social change seen in three aspects: the role of U.S. occupation policy, the influence of labor in Japan's economic miracle, and American attempts to inject democracy into the Japanese judicial processes.

The research on the Matsukawa case is meticulous and exhaustive, the organization and mastery of documentary and other materials is outstanding. The study is skillfully presented with the drama and suspense of a detective story. The inefficiency of initial police and prosecution work, the improper use of confessions, the absence of true adversary proceedings in the initial trial; the continued evidence of a protective bureaucracy operating in the procuracy and the courts are easily demonstrated. So too is the fact that the final correction of judicial injustice came as a result of older traditional practices and not those of the new Code. Included is a full account of the major political-public mass movement also important in remedying judicial injustice. The comparisons of the pre-1949 traditional continental system and that of the revised Code are useful and valuable. In sum, the Matsukawa case study is a fine descriptive analysis of key facts as they developed in this case. Since the author uses this case to peg his revisionist argument, the real problem is to demonstrate that the Matsukawa case was typical of the system.

The heavy use of color, details, journalistic captions for chapter titles; the chronological approach; and the absence of an organization by problems, functions, or a systems analysis, impede such a demonstration. Were the reforms of the Supreme Command for the Allied Powers (SCAP), necessary to achieve the ends sought? Were they efficiently instituted or carefully sandbagged by bureaucratic opposition? Were the errors of justice due to the new procedures or to inheritances of traditional practice? Were the errors of police and procuracy in the initial stages unique to the case and the time, were they due to the incompetence of a rural staff, or were they the result of an inadequate preparation and training in the new system that time and experience would correct?

The errors in discovering, evaluating, and inventorying evidence and the improper use of confessions, were those of men trained under the old system. The crucial errors in the case occurred in the initial stages; the case was dismissed when the falsity of an early key confession was demonstrated, years later. Defense lawyers did not adequately use, and the court did not direct, the adversary proceedings envisioned by the revised Code in the District Court. But whether improvement here and in police and procuracy practice came in later years is not examined. The concluding chapter surveys the nationwide application of the revised Code in terms of the use of confessions, length of trials, procuratorial prejudgments, and the bureaucratization of the system. No fundamental change is found save in the shortening of trials. But an increasing approach in practice to the standards of the revised Code is noted in younger members of the system, though the cause, substance, or potential of the change is not examined. A greater use of aggregate data analysis here and in the first chapter would have aided the study. Much credit is properly given to the public mass movement's role in the study, but whether this became a typical feature of the new system is not answered, nor does Johnson pursue the question of how the system responded to the potential danger that judicial decisions might be tried in the media and the streets as much as in the courts.

The secondary case study of SCAP's role in social reform indicts SCAP for many sins, but most particularly for a lack of an underlying philosophy and for not recognizing earlier than 1949 that democratic institutions require a viable economy to survive. The dilemma over reform-with-confusion, or stability at the cost of reform, and the question of timing, seem to be dismissed too readily. The study ignores the consistency with which major SCAP reforms sought to pluralize the bases of power and participation in education, labor, business, politics, etc. Japan's contemporary mass society suggests SCAP's contributions to that result. The importance of economic health to support reform was recognized before 1949. The influence of international and U.S. and Japanese domestic developments in pacing change is noted, but this reviewer would stress the weakness of liberal-moderate reform forces after January 1949 and the unchallengeable conservative control of the Japanese government as also factors that forced the pace and scope of change. The final revisionist position is that SCAP was not as omniscient and all powerful as it and its protagonists claimed—not a novel discovery. Labor's role in Japan's economic miracle is offered more in conclusions than by demonstrations. Japanese government and industry did exploit SCAP's shift in labor policy but whether SCAP policy and exploitation produced the economic miracle needs further examination. Nor is the related argument proven, that labor was too weak to secure its proper share in Japan's burgeoning economy after 1949 because of a public rejection of the movement in fear of labor's physical violence and political radicalism. American attempts to democratize the Japanese judicial-legal system is the heart of the entire study. Though the inaccessibility of U.S. archival records on the subject is noted, no mention is made of SCAP attempts to influence the Matsukawa trials. The chapter on the possibility of U.S. intelligence agents sabotaging the train to discredit radical labor political movements seems more journalistic than relevant to the question of court and legal practices.

The study successfully captures the mood, and dynamism of the period, but political scientists who are not area specialists will find the absence of a comparative or systems approach a limitation on its value.

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The Politics of Land Reform in Chile. By Robert R. Kaufman. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 321. $12.00.)

"It will thus take a far longer perspective than we now have to determine whether we have witnessed the birthpangs of a new socio-political adjustment, or the beginning of the ultimate deterioration of the system as a whole" (p. 285). "Is, therefore, the alternative to the admittedly frustrating and slow process of incremental change actually a more violent, but ultimately more progressive revolutionary process? Or is the alternative anarchy, reaction, and stalemate?" (p. 286).

On such a note of appropriate uncertainty, Robert Kaufman concludes his thoroughly interesting and in some ways pathbreaking inquiry into the politics of Chilean agriculture. Although the title of the book indicates a consideration of twenty years of agricultural politics, the principal emphasis is on the period from approximately 1962–63 through 1967–68. More particularly, Kaufman devotes primary attention to the forces operating to produce the

Christian Democratic agrarian reform statute of 1967. The inquiry took Kaufman mainly into the corridors of the Chilean congress and the offices of party politicians and interest group leaders. Making excellent use of approximately 60 original elite interviews, he has produced a reasoned study of the intraparty debates surrounding agrarian reform, a study replete with plausible explanations of the behavior of many of the principals in the controversy.

Kaufman's assessment of intraparty developments is particularly good with respect to the Christian Democratic party (PDC). Chapter 3, "The Christian Democratic Factional Struggle and Agrarian Reform, 1964–1967," is typical in that it not only says a great deal about the controversy surrounding agrarian reform but also contains a substantial number of insights concerning the dynamics of Chilean politics generally (see, for example, the discussion of "intraparty bargaining and external competition").

Kaufman neatly details the nature of the factional struggle within the PDC and attempts to explain why the agrarian reform became the most "leftist" of PDC reformist initiatives. He notes that "by 1964 it was clear that the age, the outlook, and the sources of support for the men who were to administer the program differed considerably from those who were entrusted with the other concerns of the Frei regime" (p. 87). Within the heterogeneous coalition of forces that comprised Chilean Christian Democracy, it was perhaps necessary for the "officialist" wing of the party to grant the more "leftist" faction operative control of some policy domains in exchange for leftist support on other officialist initiatives—such as the "Chileanization" (instead of outright nationalization) of the copper industry.

Then, too, there was the fact that a rather wide consensus existed on the necessity for a thoroughgoing program of agrarian reform. Such could not be said with regard to other policy arenas such as, most notably, the industrial sector. A concerted program of expropriations might also have been expected to have favorable electoral consequences for the PDC, although Kaufman's data show that these hopes were at best dubiously realized. It is apparent that the Christian Democratic agrarian reform had different electoral consequences in different regions of the country, in some areas consolidating support for the PDC and in others contributing to a polarization of political forces that operated against the centrist Christian Democrats.

In addition to his discussions of intraparty developments (of which those within the forces of the Chilean Right are particularly illuminating of larger patterns of Chilean politics), Kaufman offers several other pertinent observations concerning Chilean agriculture. To a considerable extent, research on the politics of Chilean agriculture has been dominated largely by a concern with the so-called "structuralist" theory, according to which the concentrated structure of land tenure is the chief factor in explaining the retardation of agricultural production and the perennial economic inflation. Since we have always been vaguely uneasy with structuralist emphases, we note with considerable interest Kaufman's critique of structuralism. He disputes, for example, the argument "that agricultural price increases stimulated price increases in the larger society" (p. 33). Examining price data between 1949 and 1960, he discovers that "in six of the twelve years . . . agricultural prices were equal to or below the price levels for industrial products" (p. 33). The conclusion is that "the causal connection between the two phenomena must be regarded as far from proven" (p. 33). Kaufman also disputes the notion that "a consumption-oriented aristocracy was the principal cause of low agricultural production" (p. 33). He suggests as alternative explanations the following: the granting of subsidies to competing foreign imports, the use of price controls on domestic products, the inadequacy of distribution networks, and the role of monopoly distributors. "Finally, even granting that aristocrats had retarded agricultural production and that this in turn had retarded the overall growth of the economy, it was by no means clear that large-scale land redistribution was the most efficient way of correcting this problem" (p. 33).

Another set of well-taken observations concerns the economic and social impact of the *asentamientos,* the Frei administration's quasi-cooperative agrarian reform settlements. Concerning economic effects, Kaufman makes a case for the essentially neutral impact of the reform on agricultural production levels, rejecting the extravagant claims of failure put forth by the Chilean rightists. Concerning social effects, he shows how the formation of *asentamientos* created new antagonisms and contradictions among different strata of the peasantry. These included conflicts both among members of the *asentamientos* themselves and between *asentamiento* members and those rural workers excluded from them. These destabilizing effects of the Christian Democratic reform continue to be felt under the Allende regime.

At various points in the manuscript, Kaufman uses the agrarian controversy as a vehicle for offering commentaries on some features of the larger Chilean political system. Such commentaries are too numerous to summarize here, but it should be noted that Kaufman has much to say concerning the social and programmatic bases of the major political parties; the operational style of parliamentarians; the political deficiencies of the Chilean Left; and the characteristic modes of transacting political business in Chile. These observations do not add up to a full-blown theory of Chilean politics but they provide many useful fragments from which such a theory might eventually be constructed.

If there are any disappointments in the manuscript, they concern the somewhat vague nature of two general orienting questions posed in the Introduction and the somewhat tortuous efforts to resolve them in the concluding chapter ("The Vertical Brokerage System and the Process of Reform"). Kaufman argues that

> the Chilean case provides an opportunity to ask two larger questions. What is the impact of prior social change—urbanization, limited industrialization, and the emergence of new, nonagricultural social forces—on the prospects for rural structural reform? To what extent do well-developed representative institutions—parties and interest groups—facilitate the adjustment of the political system to new social demands? (p. 2).

The principal conclusion is that "the study of Chile would suggest that a combination of urban indifference, peasant protest and organization, and the conflict-averting potential of the vertical brokerage mechanisms should permit a gradual and uneven, but significant, absorption of at least some peasant groups into the political system" (p. 275). This is an excessively vague, overly hedged, and basically unsatisfactory comparative generalization. But if Kaufman's study does not really cast much light on the larger comparative questions he poses, it nevertheless remains an extremely valuable case study of an important issue in the turbulent politics of contemporary Chile.

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The Making of the Australian Constitution. By J. A. La Nauze. (Melbourne, Australia: Melbourne University Press, 1972. Pp. 369. $20.00.)

Australians have not been given to revering their founding fathers. The men who made the Australian Constitution of 1900 have never enjoyed the same prestige in the eyes of posterity and historians as Hamilton, Jefferson, and Madison in the United States, or as Sir John A. Macdonald and his colleagues in Canada. Of course the Australians lacked drama. The makers of the American Constitution met in the aftermath of a victorious colonial rebellion; the Canadians were spurred by fear of American intentions and the ever-present need to conciliate the special interests of Quebec. No such pressures moved the Australians, who were homogeneous in their origins and felt themselves at that time to be shielded from external threat by the power of the Royal Navy. It has even been suggested recently that the Australians went into federation much as the British went into the European Common Market, less in response to a wider sense of nationalism than as a baffled gesture toward trying something new after a period of protracted economic crisis. A Commonwealth founded under such auspices does not lend itself to myth-making.

It has been left to Professor J. A. La Nauze to direct new attention to federation as a critical issue in Australian history. Strategically placed at the Australian National University in the federal capital, Canberra, Professor La Nauze has done much to stimulate the work of other scholars in this field, partly by launching a series, "Studies in Australian Federation," to which his own book now forms a valuable addition. It is not a history of the Australian federal movement, but something more limited and precise. Professor La Nauze concentrates on the process of framing the Australian Constitution. He is concerned with the way in which eighty-four politicians and three or four civil servants at successive conferences and conventions beween 1890 and 1900 worked the nebulous idea of federation into a shape finally acceptable to all six Australian colonies as well as the somewhat exacting, if at times uncomprehending, views of the British Colonial Office.

This needed to be done. Since Quick and Garran's *Annotated Constitution* of 1901, now a collector's item, no such unraveling of the narrative has been attempted. La Nauze has combed the archives for hitherto unexploited sources. He has written an admirable book: judicious, balanced, and full of good insights. One may nevertheless regret his self-imposed limitations. He does not seek to chart the mobilization of public opinion for and against federation, nor to analyze the interest groups outside the conference-room which may have influenced the ultimate shape of the constitution. There is no mention, for instance, of the Corowa Conference of 1893, and hardly any of the Australian Natives' Association or of the Federal Leagues which subsequently stimulated public debate. Nor is there any comment on the voting figures at the two federal referenda of 1898 and 1899, though their regional variations have been the subject of controversy among historians since the Parker-Blainey exchange of 1951.

Nevertheless, La Nauze makes several interesting contributions to Australian historiography. Australia's founding fathers were influenced not at all by contemporary notions of Imperial Federation, and hardly more by the federal enthusiasm shown by earlier British colonial secretaries such as Grey and Carnarvon. North American examples appeared more relevant, particularly as interpreted by Bryce's *American Commonwealth*, which made its timely appearance in 1888. From this arose the concept of the Senate as states' house—a role which in practice it almost immediately abandoned in consequence of Australia's tight party discipline—and also the emphasis placed on the function of the High Court as an instrument of constitutional review. There was, however, no Bill of Rights. The Australians lacked the eighteenth-century Americans' sense of having preserved hard-won liberties against the inroads of tyranny, and professed an optimistic faith in liberal democracy that made such legal guarantees unnecessary: besides, La Nauze points out quietly, it was thought that a Bill of Rights might be used to sabotage the popular desire to exclude colored aliens from white Australia.

As for the High Court, it becomes clear that many of the issues which most exercised the delegates to the Federal Convention have in fact provided it with very little business, whereas clauses passed without much discussion have become the lawyers' bread and butter. Isaac Isaacs is shown to have been thoroughly justified in his Cassandra-like warnings against the sloppy phrasing of Section 92, with its provision that interstate commerce should be "absolutely free"; but his ill-timed persistence in debate lost him the chance of making his point. In the light of recent controversies, it is interesting to confirm that founding fathers were largely anti-centralist; they did not anticipate the erosion of State powers which has gone on since 1901. A question yet to be answered is whether this process has been fostered by the overwhelming preponderance of the Melbourne and Sydney bars in appointments to the High Court since its foundation in 1903.

La Nauze does not much favor the suggestion put forward some years ago by L. F. Crisp that federation was promoted by property-owning interests in reaction to the growing strength of labor after 1890. Admitting that some radicals distrusted federation as a device to unify the power of the possessing classes, La Nauze nevertheless argues that delegates to the conventions showed themselves capable of setting aside regional or sectional viewpoints in the interests of attaining a national concensus. Reinforced by a widely shared sense of maturing nationalism, they sought practical and convenient arrangements in such matters as immigration, defense, and postal services.

In consequence no two or three men could be said to have dominated the making of the Australian Constitution. Certainly Edmund Barton, whom some historians have thought a little lucky to achieve the distinction of being Australia's first prime minister, is shown to have deserved it for his masterly handling of the 1897–98 convention. He was at the top of his form in that year, and was the necessary man to reconcile the divergent and often assertive politicians who took part in framing the constitution. But many of its provisions—the right to legislate for old age and invalid pensions, for instance, or the power to invest State courts with federal jurisdiction—were the work of individual "back-benchers," representing no discernible interest group.

Toward the end there is an unostentatious hint that Professor La Nauze is writing a tract for the times. In recent years, the course of federal-state relations has been increasingly troubled, both under the Liberal Gorton and Labor's Whitlam. "States' righters" and centralizers are at loggerheads, and no cool and rational analysis of the constitution—still largely the 1900 model—has been carried out on an Australia-wide scale for many years. Just as Professor Creighton's account of the Confederation movement of 1867 preached an implicit lesson for modern Canadian nationalism, so Professor La Nauze seems to be commending the careful, well-informed, and surprisingly high-principled transactions of Australia's founding fathers as a model for legislators in the 1970s. It is too early to say whether his work will have an impact comparable to Creighton's. But simply as a piece of humane and meticulous scholarship, it was well worth doing.

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The Mind of Adolf Hitler. By Walter C. Langer. (New York: Basic Books, 1972. Pp. 269. $10.00.)

The Life and Death of Adolf Hitler. By Robert Payne. (New York: Praeger, 1973. Pp. 623. $12.95.)

These two books clearly do not pretend to deliver an intellectually satisfying account of the complex phenomena we identify with Adolf Hitler. Both books deal almost exclusively with Adolf Hitler the man, to the all but total exclusion of the social, economic, political, and cultural influences that intersected to produce the events associated with his name. As a consequence, serious explanation eludes us. But more than that, because of their narrow focus, it is doubtful if either could pass as a competent biography. Payne's volume is considerably better than Langer's—at least in part because Payne has access to the wealth of reasonably reliable information that has become available after the end of the Second World War. Langer's book, written during the war years, contains little credible information. But the unavailability of substantive information is only one, and perhaps a minor, cause of the disabilities that characterize Langer's "wartime report" on the *Fuehrer.*

Langer, a professional psychoanalyst, is so wedded to Freudian simplicities that his book can only, and most charitably, be characterized as bizarre. In its pages we find the entire catalogue of Freudian psychosexual impairments imputed to Hitler. Hitler (predictably) suffered an unresolved Oedipus complex, had castration fears, projected, identified with the aggressor, suffered toilet training trauma, enjoyed aggressive fantasies, was sexually impotent and sadomasochistic. In effect, Hitler was the model Freudian man. One might have predicted—not knowing anything about Hitler—what Langer would say about him, solely from having read Freud's introductory lectures on psychoanalysis. This is not to say that Langer's account is completely devoid of novelty. We learn, for instance that Hitler was a vegetarian and a teetotaler because he suffered from "coprophagic tendencies" (p. 172)—unconsciously identifying meat and beer with feces and urine (p. 170)—and, as these things work in psychoanalytic tracts, he resisted them. But more entertaining than that, we discover that Hitler's psychosexual development produced in him an "extreme form of masochism in which [he derived] sexual gratification from the act of having a woman urinate or defecate on him" (p. 134).

All of Hitler's perversions were (predictably) rooted in the orthodox "family drama"—but were aggravated by the fact that Hitler had a drunken, unjust, inconsiderate, brutal father who "had no respect for anybody or anything. [Hitler's father felt that] the world was all wrong and an unfit place in which to live" (p. 145; cf. pp. 150, 159, 161, 172). But this was only the beginning of Adolf's problems. To add to his psychosexual distress, Langer speculates that little Adolf must have discovered his parents indulging in sexual intercourse (p. 151)—a surmise that Robert Waite elevates in his "Afterword" to a conjecture that little "Adi" witnessed his "drunken father" raping "his dear mother" (p. 227).

All of which makes delectable reading about someone as universally despised as Hitler. Unhappily for serious scholarship, almost all of it is woven out of fantasy. The evidence for the Freudian "family drama" is as conjectural as the rape scene. Robert Payne tells us that Alois Hitler, Adolf's father, was "strict with his family" but that his "bark was worse than his bite. He never struck his children . . ." (p. 22). Bradley Smith, in his *Adolf Hitler: His Family, Childhood and Youth* (Stanford: Stanford University Press, 1967) tells us that Alois Hitler was a frugal, reasonably pleasant, somewhat pedantic bureaucrat who was a mild drinker and a positive Germanophile (pp. 43–49, 59–61). Payne characterizes Hitler's father as a "solid, respectable citizen who never doubted that he was living in the best of all possible worlds" (p. 30). Payne simply states, "Alois Hitler has been represented as a drunken, tyrannical husband, and his young wife as a supine woman embittered by a life of suffering, but this is to misinterpret the weight of evidence. . . . We hear of no feuds, no violent outbursts of temper, no sudden upheavals. The very ordinariness of the family commands a kind of respect" (p. 14).

Langer had used Hitler's vegetarianism and his putative avoidance of alcohol as evidence of his coprophagic tendencies. We learn from Payne that Hitler drank beer and ate (of all the incarnate Freudian symbols!) sausage. Facts, unfortunately, never infirm the kind of analysis indulged in by Langer. Like many psychoanalytic interpretations, it could go both ways: if Hitler were "coprophagic," why not eat symbolic feces and drink symbolic urine?

In any event, Langer's book is incredibly silly. His sources of information are less than credible. He has collected hearsay from the most peccable sources—third-party recitations, political opponents, refugees from Hitler's Germany, disgruntled relatives, venal informers, and self-seeking opportunists among others. He nowhere attempted to invoke minimal controls over his sources, nor did he seek independent corroboration of any claim—no matter how outrageous. His entire "analysis" is held together by unsupported speculation. Langer candidly admits as much. What he offers his readers is, by his own admission, "speculative and inconclusive" (p. 142). He admits that "our factual information on [the early] phase of [Hitler's] life is practically nil" (p. 142).

Bradley Smith had already warned his readers that "the kind of information that psychologists and analysts value highly . . . details on toilet training, food preferences, ideas on compromise, etc.—are, in the main, not available for Hitler. Therefore, one must either apply the conclusions of a particular theory . . . or try to explain the case on the available evidence. The historian must take the second alternative and let individual psychologists fill in the gaps as their separate schools dictate" (p. 51, n. 5).

For all of Robert Waite's afterthoughts about the merits of Langer's "imaginative guesses" (Langer, p. 224), there is little in *The Mind of Adolf Hitler* that warrants writing, publishing or reading it.

Robert Payne's *The Life and Death of Adolf Hitler,* written by a professional biographer, is a far better book. It obviously profits from the specialized work of any number of competent scholars. It is an interesting story about one man's life. Since there is no mention of the social, economic, and political forces that shaped the events of that life, we receive the impression that an "improbable adventurer . . . conquered the German people by the sheer force of his will" (p. ix). Payne feels that Hitler's rise to power is one of those events in world history "which are almost totally inexplicable in any rational terms" (p. ix). We are left with the uneasy and improbable suggestion that a "single man," and that man alone, caused all the havoc (p. ix). We are provided an account of a solitary "demonic madman," who with the assistance of "criminal clowns" like "the froglike Goering, the lame dwarf Goebbels, and the chinless Himmler," killed forty million people in the "most crucial and mystifying event of this century" (pp. xiif).

By his own admission, there is little in Payne's book that might coherently explain the entire complex of events. We are left with a "mystification." Not only is the entire phenomenon a "mystery," but Hitler himself never clearly emerges. We are told on the one hand that Hitler "dressed well" as a young man (p. 44), and then we are told that it was Dietrich Eckart, years later, who taught Hitler, "who was indifferent to clothes, to dress well . . ." (p. 139). On page 95 we are told that Hitler "expected everyone to wait on him, and was unappreciative and difficult"—and then we are told three pages later that Hitler was a "silent lodger, whose manners were impeccable . . ." (pp. 98f.). From the prison governor of Landsberg we also learn that Hitler was "amenable, unassuming, and modest. He . . . never made exceptional demands, [conducted] himself in a uniformly quiet and reasonable manner, and . . . put up with the deprivations and restrictions of imprisonment very well" (p. 202). On page xi we learn that Hitler killed the leadership of the SA because he did not wish to be reminded "he had depended on them"—on page 227 we are told that he killed them "to placate the army." On page 345 we learn that Hitler "had a mind like a cutting edge," and on page 353 we hear that the design of the Berghof was a confusion, a design that captured "the confusion that reigned in [Hitler's] mind." On page 433 we further learn that Hitler had a "disorganized mind. . . ." On page 109 we discover that Hitler was a "good soldier," who was "quite fearless," and on page 461 that Hitler was a "coward in the dentist's chair. . . ."

After all this we are left with the insight that Hitler was "the strange magician who could reduce hundreds of thousands of people to mindless robots" (p. 292). The best Payne can offer by way of serious explanation is that Hitler employed the vast resources of "powerful industrialists" and "great bankers" (pp. 237, 243) to come to power—even though that story has long since been laid to rest.

Both Langer and Payne admit they have little to say that might assist us in coming to understand Adolf Hitler or the movement that shook Europe for more than a decade. Fortunately, a great many better books are available to aid us in that task.

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The South African Voter. By H. Lever. (Cape Town, South Africa: Juta and Company, 1972. Pp. 221. R7.50.)

Though much shorter and of narrower scope, this book is the most substantial work on South African white politics to have been published since Gwendolen Carter's 1958 classic, *The Politics of Inequality.* It deals with the general elections of 1966 and 1970, and, while the author constantly refers to the general political scene, the study he reports is based on a survey of some 600 voters in three Johannesburg constituencies. Henry Lever, now professor of sociology at the University of the Witwatersrand, is somewhat apologetic about writing a book readable for laymen as distinguished from a book for sociologists, but that is certainly no loss.

Methodologically, Lever was inspired by *The People's Choice,* but in fact there is much less continuity in the use of a "panel" than would appear at first. Only one of the three constituencies is common to the 1966 and the 1970 surveys (which took in two constituencies each). The sample was entirely different from one study to the other. In the first survey, voters were approached twice, before and after the election; in the second they were not. In short, it is the author who provides most of the temporal continuity, not the voters interviewed. The book also contains a chapter on the effect on racial attitudes of a terrorist bomb attack in 1964, which has only a tenuous relationship to the voting study.

The topic of the book, however, is too grimly serious to warrant a reviewer's harping on methodological points. Substantively, the book is depressing. It documents in painstaking detail the incorrigibly racist attitudes of the South African electorate. While the 1970 election marked an infinitesimal reversal in nearly a quarter century of rightward drift in white South African politics, the change is so ludicrously small as to make speculation concerning its causes seem futile. Lever anticipates this criticism when, in discussing the *verkrampte-verligte* split within the National Party, he berates those who view this internal quarrel as "an inconsequential dispute between persons holding the same fundamental views" on the ground that "inability to differentiate between shades of political opinion is frequently evidence of a bias or the holding of an extreme viewpoint (p. 162).

My "extreme viewpoint" is that South Africa is long overdue for nonracial democracy on the basis of universal adult franchise. That makes me a Communist and a lunatic-fringe extremist by white South African definition. From that perspective, I do not even find the differences between the National and the United Parties, nor indeed between the United and the Progressive Parties were relevant. Any book on South African politics that treats 82 per cent of the population as a shadowy background which only appears indirectly in the form of white prejudices about the Black Menace and the Race Problem is seriously flawed in my view.

The author painstakingly tries to hide his own political opinions, which, in some quarters, passes for objectivity. Yet, he unwittingly reveals himself a victim of the rotten system he describes. On p. 142, he makes the astonishing statement concerning the assassination of Verwoerd: "There were very few, if any, South Africans who welcomed his demise at the hands of an assassin." I was in South Africa during the abortive assassination attempt on Verwoerd, and I can testify that the reaction of Africans and Indians ranged from guardedly to openly jubilant and hopeful that Verwoerd would die. The only misgiving was that his replacement would probably turn out to be worse yet, in the tradition of South African prime-ministerial succession. As Verwoerd did little to endear himself to nonwhites in the last five years of his life, I should be surprised if the reaction to his successful assassination was much different.

Lever's statement is either patently false, or else, in good white South African practice, he unwittingly restricts the definition of citizenship to whites only. I am, of course, not attacking Lever's integrity, but simply pointing out that permanent immersion in a racist society makes it almost inevitable that even social scientists should implicitly accept the premise on which such a society is organized. The most insane aberration of human arrogance comes to appear to the ruling class as a God-ordained and inalterable state of nature.

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The Writing of History in the Soviet Union. By Anatole G. Mazour. (Stanford, Calif.: Hoover Institution Press, Publication 87, 1971. Pp. 383. $17.50.)

Historiography, the study of the methods and history of historical scholarship, has been a favored subdiscipline in both imperial and Soviet Russia. The Historiographical Sector is a major component of the All-Union Academy of Sciences' Institute of History of the U.S.S.R., and a Learned Council on Historiography, created under the auspices of the Academy, sponsors and coordinates historiographic research on a nationwide scale. Numerous encyclopedias, chronicles, and memoirs of historians are available as sources. As a result, historiography is a major dimension of almost all monographs, and most major scholars, at one or another stage of their activity, try their hand at it. It would be difficult for a historian not to fit his own work into the history of the study of a given problem and not to be aware that his own conclusions have a genealogy of their own.

In the West, Soviet historical scholarship has received a due amount of attention, moreso than any other academic discipline, although in the last decade the study of Soviet sociology has moved into the forefront of Western studies of Soviet thought. In the study of historical scholarship, the political scientists' perspective has been ascendant; the state domination of historians and the concept of party-minded scholarship have predominated. "Scholarship as a barometer of politics" was the chief concern as long as the concept of totalitarianism guided Western studies. To the degree that Soviet historiography has been studied as a system of assertions about the past, rather than as a political barometer, it is the work of Anatole G. Mazour, Professor Emeritus at Stanford University, that has been most important. His articles and two editions of his *Modern Russian Historiography* have been indispensable introductions and guides to Western scholars. Although Mazour has written on a variety of topics in Russian history, his most important contribution has been in the field of historiography.

The book contains a brief history of the Soviet historical profession and surveys of major auxiliary disciplines: historiography, archeography, regional history, and archeology. Chapters III-VIII discuss the literature on the history of pre-Soviet periods; Chapters IX-XIV (pp. 191–359) cover the Soviet period. There is no chapter on the Russian Revolution. Presumably, nothing other than a companion volume would have sufficed.

Bibliography is an important feature of Mazour's study, which contains thousands of footnotes, some containing more than a hundred titles. Mazour's chief concern, nevertheless, is with the findings of historians, their facts and conclusions. He frequently conveys information about the evolving source base of a given subject, the range of disputation, and the changing and sometimes contradictory assumptions of historians. The work is a deeply personal account of the author's own study of Russian history. It is a description of how Soviet historians have sought to answer his and their questions about Russia. The author has shared long years of reading and reflection with his readers. This is the distinctive mark and principal virtue of the book.

Specialists in Soviet historiography and history will return to this volume again and again and keep it near at hand in seminars. It will be only slightly less useful for specialists on pre-Soviet Russia. For political scientists, sociologists, and economists the book will be useful as a reference work, containing bibliographies and brief accounts of how others have investigated some matters they are now studying. For scholars with only passing interest in Slavic studies and communist affairs, the book contains useful information about social scientists and interesting material about the political and social determinants of thought.

What Mazour has failed to provide for his reader is an historical account of the rise and evolution of Soviet historical scholarship, or a systematic explanation of Soviet historians' activities. A major reason for this omission is the author's lack of clarity in formulating his assumptions about ideology, politics, and scholarship.

> Sympathetically analyzing the situation, one is willing to concede that ritualistic Marxist obeisances to Marx, Engels, or Lenin as the source of eternal historical wisdom are the price the Soviet historian has to pay if he is to publish the result of his research. These curtsies may be overlooked if they are not forced into the interpretation to the exclusions of historical evidence. But it is a deeply offensive thing when the historian is denied the right to act as dispassionate reporter, and it becomes downright intolerable when he must become an ideological champion using history in the service of party goals (p. xiii).

That is to say—were it not for politics and ideology Soviet historians would be like their Western counterparts and operate as we do; as though we are motivated only by respect for reason and love of the past, and are uninfluenced by existential circumstances and particular interests. Mazour's lack of rigor at this level hinders him from distinguishing Marxist ideology from political pressure when it might be suitable to do so and from conceiving them, in turn, as a unity when this seems appropriate. We get only an impressionistic account of how, singly or together, they have affected Soviet scholars.

It is not a reviewer's task to dispute the author's assumptions but rather to show their impact on his findings. In this case they are minimal. They do weaken the introductory chapters and slightly mar the work's thematic unity. They do not impair the presentation of what was most important to the author. The uses of this book are numerous, and its value is immense.

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Nationalism. By K. R. Minogue. (New York: Basic Books, Inc., 1967. Pp. 168. $4.95)

In this concise work, a Senior Lecturer in Political Science at the London School of Economics offers an interpretive and thematic sketch of the historical development of nationalism. His principal thesis is that nationalism—far from constituting a single phenomenon—has been only a semantic mask disguising the most divergently motivated movements. To illustrate the inconsistencies both within and among movements commonly viewed as nationalistic, the author places particular stress upon the intellectual and political roots of (1) the French Revolution, (2) German unification, (3) the independence of colonial India, and (4) what the author terms "African Nationalism." Having demonstrated the tendency of political leaders of the most diverse persuasions to cloak their movements in nationalistic attire, Minogue concludes that the only universal in nationalism is its rhetoric. Nationalism *per se* is thereby reduced to techniques by which leaders appeal to the passions of the masses. "Nationalism is a set of ideas, but as they travel from continent to continent, these ideas add up less to a theory than to a rhetoric, a form of self-expression by which a certain kind of political excitement can be communicated from an elite to the masses. These ideas are chameleons that take on the colour of the locality around them . . ." (p. 153).

Minogue's basic criticism of scholarship concerned with nationalism is that authorities, having failed to perceive the purely facadal nature of nationalism, have found it wherever nationalistic jargon has been utilized. This error is manifest in the exaggerated notion that nationalism is the chief motivating force of modern history. "If we ask what electricity it is that moves so many different men in so many different places, the simplest answer, the one that allows us to stop thinking most comfortably, is Nationalism" (p. 22).

While Professor Minogue's work constitutes a lucid antidote for a too uncritical readiness to perceive nationalism in its trappings, I cannot share his conclusions about the nature of nationalism. The analogy of democracy might be helpful. Democracy's trappings—such as elections and plebiscites in Nazi Germany, ostensibly representative governmental bodies on Taiwan, constitutions containing impressive individual liberties as in the Soviet Union and the PRC, and titles such as "President-for-life"—have often been employed in a most farcical manner to hide highly authoritarian political systems. Moreover, there have been scholars who described such systems in democratic terms. But does it follow that democracy can be reduced to rhetoric?

The essence of nationalism is not to be sought in the programs of leaders but in its popularity. The real question is why do leaders find it advisable to cloak their movements in nationalistic garb? To what do they appeal? Minogue makes a promising start by stating (p. 25) that nationalism is "a political movement depending on a feeling of collective grievance against foreigners." But who is the "we" that experiences this feeling, and how does one explain the ubiquity with which this we-they syndrome is manifested? This is, of course, but to raise again Ernest Renan's labyrinthine question: *"Qu'est-ce qu'une nation?"* And Minogue does indeed address himself to the question, dedicating a chapter to "the equipment of a proper nation." It is evident, however, that he is soon mired in the same semantic quicksand against which he warned all students of nationalism in his opening section. Though carefully noting the distinction between nation and state (pp. 10 and 135), he employs the word nation all too often when actually referring to a state (e.g., pp. 115–117, 126–131, and 133). What, for example, is one to make of the sentence: "The Olympic Games, revived in 1896 as an arena in which hostile *nations* might meet in a politically neutralized atmosphere, have provoked strenuous *national* effort by any *country* with a chance of shining" (p. 117)? Or: "Each of these nations contained large minorities" (p. 137)?

Professor Minogue is certainly no more guilty of careless terminology than many another authority on nationalism, and I call attention to the semantic difficulties in his book only because of their substantial impact upon his central thesis. It has led him sometimes to confuse loyalty to the nation and loyalty to the state. Thus, nationalism, which was earlier defined as "a love for an abstraction of the nation" (p. 23), elsewhere becomes "one country learning from another" (p. 132). This confusion is also manifest in the author's preoccupation with what he terms "Indian nationalism" to the exclusion of concepts such as Kashmiri, Naga, or Tamil nationalism; it would be more accurate to treat the former as a multinational alliance which is increasingly threatened by the growing nationalism of India's various nations. The author's semantic confusion also leads him to dismiss ethnonationalism within non-Arab Africa as tribalism. Admittedly, those who are desirous of preserving the multinational (or, more accurately in the case of prenational situations, the multiethnic) states which emerged from colonialism, have purposefully linked nationalism with loyalty to the state-structure. But as evidenced by an impressive and expanding list of states troubled by ethnonational discord, the dynamics of the proposition that one people ought not to be ruled by those deemed aliens, operates quite independently of the statements of state officials and scholars. Bengali nationalism proved no less real because of official and scholarly panegyrics to a "Pakistani nationalism."

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Politics and Society in Mexico. By Martin Needler. (Albuquerque: University of New Mexico Press, 1971. Pp. 143. $6.00.)

El Sistema Político Mexicano: Las Posibilidades de Cambio. By Daniel Cosío Villegas. (Mexico City: Editorial Joaquín Mortiz, 1972. Pp. 117. 15.00 pesos. Earlier version published at Austin: Institute of Latin American Studies, University of Texas, 1972. Pp. 71. $2.00.)

These two brief books provide useful—and to some extent complementary—additions to the political literature on Mexico. In the first place, Needler provides an introductory work suitable for undergraduate courses. It is more up to date than Frank Brandenburg's *The Making of Modern Mexico* or L. Vincent Padgett's *The Mexican Political System,* and lacks the gossipy, emotional tones of Kenneth Johnson's *Mexican Democracy: A Critical View*—the major alternatives.

Second, though short on basic facts, Needler's compact book provides more propositional analysis than many country studies twice its length. Needler himself contributes little new field research; but his writing is valuable for the incorporation of others' recent research findings into a general introduction. The materials on the military and on urbanization are particularly refreshing.

Third, Needler seeks to develop a significant, forward-looking—and debatable—theme. Mexico, he argues along with others, is in transition from a democratic single party to a new kind of system. The official party has had great historical success in governing and developing the nation in a fairly democratic manner. That very success, however, is leading to the erosion of its power and the obsolescence of its structure. The basis future alternatives are: evolution toward a two- or multi-party system that includes an internally more democratic PRI, or resort through force to an undemocratic single-party system.

These three general strengths, however, are insufficient to overcome a variety of important weaknesses that limit the book's usefulness for advanced analysis. To begin with, a number of important individual propositions suffer either from insufficient research or else from incomplete analysis. The study, for example, minimizes the relative productivity of the *ejido* system; yet other available analyses reveal that where production inputs are equally good, *ejidos* perform as well as comparable private properties. In addition, Needler cites findings to show that by the 1960s the rich were not getting richer while the poor were getting poorer, yet he neglects the striking and politically significant degree of income inequalities, one of the highest in Latin America. Such shortcomings make the chapter on economic policy the weakest in the book. But some political points are also problematic. Thus as discussed (pp. 44–46), the PRI indeed plays limited, often sidelined roles in processes of political "executivism," interest consultation, and demand management. But his arguments that the party "as such does not take part" (p. 44) tend to slight the critical roles of sectoral leadership in managing discontented peasant groups that have serious, potentially violent, conflicts with powerful economic interests. As a final example, aside from one passing reference, Needler's chapters on oligarchy and the presidency neglect a fundamental Mexican concept—the so-called Revolutionary Family—useful for understanding unique relationships between top-level elite integration and political stability.

Second, there is little explicit coordination between chapter conclusions and the central theme. For example, one intriguing chapter treats the contemporary political role of the military and corrects some misconceptions. The author concludes (p. 72): "Thus rather than regarding the achievement of civilian control in Mexico as a *definitive* accomplishment, it is probably wiser to think of it as provisional." Quite right: though highly depoliticized, the army still has significant residual roles, especially in provincial politics. (Indeed, to add a new point, it may not be uncommon for discontented peasant groups to turn to the military zone commander as an alternate channel for interest articulation if they feel that the state governor has obstructed their petitions to the president.) If, however, civilian control is indeed "provisional," then one wonders about the implications. What might happen to military roles during a transition from a democratic single-party to another kind of system? Though a military coup might be virtually unthinkable, even during a period of political unrest, Needler does not consider whether less significant role changes might be possible. Other chapters, especially on economic policy and national character, also leave open the potential bearing of specific propositions on the main theme.

Thus, although providing a sense of the possibilities for change, Needler's book is an uncertain guide to current trends in institutional developments. In contrast, the monograph by Cosío Villegas, the grand sage of Mexico's social sciences, may well provide the best discussion available in either Spanish or English on precisely these concerns.

In Cosío's view (p. 68), "the most important and urgent political problem in present Mexico is to contain and even reduce that excessive power" which is concentrated in the presidency and sustained by the PRI. While the presidency has become the powerful court of last resort for serious problems arising even from the municipal level, the official party has entered an institutional crisis state. In particular, the party lacks policy independence from the presidency, yet has little effect on it. Certainly major economic decisions are taken outside the party; and their major beneficiaries are not, and never have been, its members. Indeed, if the party effectively represented its vast worker and peasant membership, the extremely inequitable income distribution patterns would be markedly different. But the PRI has become little more than an electoral machine. As constituted, it cannot hope to attract majority membership among the expanding voter population of the future. Meanwhile the history of graft, repeated failures at internal democratization, and widespread dissatisfactions have cost the party its prestige and may place in doubt even its legitimation effect on government candidates.

Thus, Cosío observes, in order to limit official power the party needs a thorough democratizing transformation What this means is unclear, but it certainly needs to include an end to its greatest obstacle, *tapadismo*. That practice, the hidden elitist selection of government-party candidates, may dangerously intensify elite infighting and thus tempt aspirants to the presidency (and governorships, Cosío should add) to manipulate public disturbances for personal ends.

Might democratizing changes originate from forces outside the party? Doubtful, argues Cosío. Opposition parties lack the strength and the leadership; and the creation of a new party independent from the PRI is remote. Certain economic pressure groups—bankers, merchandisers, industrialists, commercial farmers, and "oppressor groups"—have the capacity to limit official power, through threats to restrain investment and export capital. But having a stake in the existing system as its inequitable beneficiaries, they lack interest in democratization. Finally, public opinion, whether expressed through the press or violent acts, has not served to modify the government.

Might change originate from forces within the party? Doubtful, concludes Cosío. The discontented but disorganized peasant sectors, the somewhat better placed workers sectors, and the comparatively well-off popular sectors, all lack either the capacity or the will to effect reforms, as does the presidency of the national executive committee. Ironically, then, only the president of the republic may possess the capacity to implement fundamental changes, provided he were willing to trim his own powers.

As a result, though not without hope, Cosío is quite pessimistic. Moreover, he worries that serious confrontations may develop among major social sectors—the economically and culturally isolated upper classes, rising middle-class technocrats who may want control of the top posts, radicalizable landless agricultural workers, and conservative land-blessed peasants. Should violent conflicts arise, the government's already powerful role as the court of last resort would be reinforced, thus stimulating authoritarian, undemocratic, and regressive tendencies that are still held in check.

If, as both authors claim, Mexico is entering an institutional crisis, the outcome cannot fail to concern us all. And if, as Cosío argues, no single source is likely to foment democratizing reforms, perhaps less noticeable pressures from a variety of sources will nevertheless have positive, cumulative effects. At least presidential activities in three areas seem promising: a very large circulation of political, bureaucratic and military elites in favor of younger, better educated generations; the assertion of progressive agrarian and agricultural policies that may gratify many peasants; and the nationalistic formulations of a more outward, multilateral foreign policy. My own guess is that internal democratizing reforms are needed to improve and strengthen the PRI's performance from a popular perspective, but that change now to some balanced multi-party system would increase rather than resolve Mexico's political problems.

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Structural Change and Economic Policy in Israel. By Howard Pack. (New Haven, Conn.: Yale University Press, 1971. Pp. 273. $9.75.)

The purpose of this book is to evaluate the performance of the Israeli economy in the 'fifties and 'sixties in terms of the policy goals stated by the successive governments of the country.

Broadly the aims of economy policy, as outlined by the author are: full employment; a reduction of the heavy import-surplus; and the geographical dispersion of the population. Later in the book, two other aims are stated: income parity of the agricultural sector with other sectors, and an income distribution that will be egalitarian enough to avoid social tension, and yet, have an upper tail which will provide an adequate incentive for the managerial and the professional labor force not to emigrate. The author evaluates the performance of the economy in the light of these policy goals in two stages: first, the consistency between actual economic development and the stated goals is examined, without trying to identify the forces which caused this consistency (or inconsistency). At the second stage some direct government policy tools are analyzed, and again the author asks whether the government's actual policy was consistent with its stated aims. At each stage an additional question is, whether the changes taking place in the economy were "efficient," i.e., whether there was no unnecessary waste of resources.

The book starts by describing briefly the growth of GNP and structural changes in the economy. The growth of total national product is described in terms of the increase in the quantity and quality of factors of production. Then the changes in the sectoral structure of production and in the uses of output in each sector are analyzed. It is concluded that the major sectoral change which took place in the 'fifties and 'sixties was the relative rise of manufacturing and the (relative) decline of agriculture and services. In terms of uses of output, expanding local demand was the major driving force to production, but in addition import substitution was very important in the 'fifties, while export expansion took its place in the 'sixties.

Next the growth of manufacturing is analyzed in the context of the stated policy goals of the government. The questions asked are, whether the growth of industrial production contributed to a reduction in the import component and whether it was labor-intensive (thus helping toward full employment). The conclusion is that the development of manufacturing contributed to the reduction in the import surplus, but was not labor-intensive. Professor Pack then asks whether the direction of development was efficient in terms of comparative advantage in the international market. No single answer, but rather a presentation of different criteria is suggested. Broadly the same kind of discussion is held on agriculture, i.e., whether its development contributed towards the reduction in the import-surplus, whether it was labor-intensive, and whether it was efficient in terms of maximum possible agricultural production. There is also a discussion of the change of the income of farmers relative to other sectors.

After examining the consistency between the growth of production and various stated policy goals, the author tries to identify directly the role of the government in achieving its goals. The main microeconomic tool of the government is the Development Budget, and the author discusses the allocation of the government's Development Budget among different uses, and the effect it had on the balance of payments, the employment situation, and the dispersion of population. He concludes that investments were allocated so as to achieve the policy goals stated, except that of full employment. In allocating the funds of the Development Budget, however, efficiency was lost, because the allocative mechanism did not rely enough on market forces; specifically the interest rate was much too low to guarantee efficient investments and adequate savings.

Though microeconomic tools were directed to achieve a reduction in the import surplus, this goal was not attained because of macroeconomic factors, mainly the growing local demand for everything, including imports, and because of persistent inflation. In fact, the author implies that the increase in the import surplus helped to finance investments, and also to ensure a relatively egalitarian and improving standard of living. Thus it is not so clear whether the reduction of the import surplus was in fact an unequivocal and high-priority policy aim.

This is a part of a broader question which I had in reading the book. While three major and two subsidiary policy goals are assumed, too little attention is paid to the relations between the goals themselves, i.e., whether they are consistent with each other, and if not, how one can evaluate their optimal combination. One example is the possible contradiction between income parity in the agricultural sector and economic efficiency. Because of these inherent contradictions in policy goals, very little can, in fact, be concluded about the overall performance of the economy and its shortcomings.

In summary the book as it is is very interesting (and readable), and provides a useful basis for discussing the interaction between policy decisions and market forces.

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Rajahs and Rebels: The Ibans of Sarawak under Brooke Rule, 1841–1941. By Robert Pringle. (Ithaca, New York: Cornell University Press, 1970. Pp. xxi, 410. $15.00.)

The "White Rajahs" of Sarawak are always an interesting subject. The lost, and therefore romantic, Victorian age seems sometimes to be typified in a special way by the Brooke rajahs. But they were more than idealistic Victorian gentlemen in breezy shorts, and their subjects were more than head-hunting savages. The Brookes and the Ibans, who made up the majority of their subjects, are as susceptible to rational historical inquiry as anyone else. The results of such a study can have a significance beyond the intrinsic historical interest of the Brooke period; it can be significant also for the history of European colonialism in Asia and for an understanding of the contemporary situation in Sarawak.

Dr. Robert Pringle's study of the Ibans under Brooke rule is a superb book. The author, Second Secretary of the U.S. Embassy in Jakarta, appears to have read virtually every document of significance for his study, and he combines this enormous mass of historical material with an intimate acquaintance with the Ibans and a wonderful sense of time and geography. He writes extremely well, and makes clear the influence of climate, terrain, location, social environment, and personalities on the events and relationships he describes. He wisely underplays the generality of his study, for in important ways both the Brookes and the Ibans were atypical: the Brookes were a "family of benevolent adventurers" (p. 97) whose personal idiosyncracies were of fundamental importance in the history of Sarawak; the Ibans knew no political organization above the level of the longhouse, and were given to migration, warfare, and especially head-hunting.

The book is written with a sympathetic understanding for the main characters of the story. It is highly detailed, and from time to time the reader may find it difficult to pick his way through unfamiliar personal names, ethnographic terms, and place names. Fortunately the book is provided with five excellent maps, and the author occasionally steps back from the detailed narrative to consider the broad themes and implications of his subject—and he always does so sensibly. His sympathy for the personalities of the Brookes and their assistants does not prevent him from launching some very solid criticisms of the consequences of Brooke rule.

The Brooke administration was always small and usually impoverished. One of the merits of this book lies in showing that even such a limited colonial regime could fundamentally affect the lives of its subjects. The Brookes rather admired Iban society as they found it, and in consequence of this admiration and of their usually limited resources they did little to "modernize" Iban society or to raise its standard of living. But they did exercise an important influence on the Ibans; in Dr. Pringle's view this influence was to a large extent detrimental.

For instance, the Brookes used Ibans to fight Ibans, and in doing so they "encouraged and perpetuated the traditional Iban glorification of bloodshed" (p. 240). Indeed, there is much bloodshed in this story, as Iban levies respond to the Rajah's call. Many heads roll, afterward to be dried and placed in the longhouses. It is the author's thesis that the Brookes made social change more difficult by such encouragement of Iban traditions. Dr. Pringle says of Brooke policies in general that "gradualism carried to excess could be interpreted as tolerance of the intolerable" (p. 347).

The structure of communal relations was also influenced by the Brookes, and again disadvantageously. Dr. Pringle typifies the early stages of Brooke rule as a competition between the White Rajahs and a Muslim "Malay" ruling class ("Arabs," Brunei nobles, and lesser Malay chiefs) for influence over the non-Muslim Ibans and Melanu. As Brooke rule matured, the Brookes followed the principle that good fences make good neighbors, and they increasingly encouraged a sense of communal differences. They also encouraged the growth of the Chinese population in Sarawak. The net result was a restructuring of Iban-Malay-Chinese relationships, and a heightened sense of "Iban-ness," which has greatly exacerbated the problems of adjusting to the changing circumstances of the present century.

The book is full of excitement, as the author probes the forces at work behind the scenes of an area in which, as he puts it, so little appeared to "happen." Sarawak comes alive here.

The book is unusually well produced, although of course it is not without technical faults. The footnotes appear in their correct place at the bottom of the page, although for no apparent reason they are sometimes divided from the text by a horizontal line and sometimes not. There are excellent maps, thirteen illuminating photographs, and a quite fascinating set of appendices. The bibliography is superb, and the index detailed. There are, of course, typographical errors in the text, but their number is small. In the bibliography a few entries are annotated but the majority are not, and something curious has occurred to the second bibliographic entry under H.A.A.N. on p. 383. Nevertheless, by contemporary criteria the book is technically well above standard.

This is a colorful and important story, and Dr. Pringle has told it well.

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Rice Economy, Employment and Income in Malaysia. By J. T. Purcal. (Honolulu: University of Hawaii Press, 1971. Pp. 248. $9.00.)

Although Malaysia has among the highest average per capita incomes of any Southeast Asian states, relatively little of this "development" has filtered down to the rice-growing villages. This book constitutes an incomes analysis of the peasant rice economy of four villages in northwestern Malaya. As such, it is a pioneering work of village-economic analysis. The topics covered include the utilization of labor, male and female, in the village economy; village income accounting; household expenditure; and marketing arrangements. For each of these the study provides detailed comment, based on local surveys and questionnaires, on resource use and money flows, comparing single- and double-cropping areas. Unfortunately for the readers of this journal, the book does not delve into the larger issues of agricultural policy, rural development, and the political consequences of ethnic-occupational stratification between Malay peasantry and predominantly non-Malay urban sectors.

One very significant finding of this study, in terms of its policy implications, is that the "spread" between village earnings and the retail price of rice was apparently not unreasonable. From the evidence it appears that the much-maligned market intermediaries, usually ethnic Chinese, have not exploited their potential economic power. Dr. Purcal attributes this less to their altruism than to an awareness of possibly adverse communal and political consequences (p. 124).

The question of possible exploitation of peasants, however, is left unresolved. The study points to a serious shortage of cultivable land, widespread tenancy, insecurity of tenure, and a resulting high annual turnover of holdings (p. 107). We catch a glimpse of a village society cut by classes: of land-hungry poor peasants; of middle peasants hiring land and working it partly for themselves, partly for rentiers; and of absentee landlords. Yet while tenancy is apparently common and subject to some of the age-old agrarian evils, we are told virtually nothing of the complexity of peasant-land-landlord relations. Similarly, agricultural credit problems are mentioned, but we are not informed about the substance of credit relations. The resource-incomes analysis focuses narrowly, excluding the needed institutional context. This narrowness is to be regretted, since the year spent by the author in the intimacy of the village economy might have provided considerable new insight into its legal-institutional structure. Instead of portraying the rice economy as a village *economic system,* Dr. Purcal limits his treatment primarily to flows of income and resources.

This shortcoming weakens the brief prescriptive element of the book. The author concludes that the problem of Malayan rural poverty would be solved "if the government could provide the padi planters . . . with suitable land for padi or rubber and the necessary amenities" (p. 129). Of course, if government could simply do that, agrarian poverty would not exist in the first place. The point is that critical social, attitudinal, and political constraints affect the formulation of agricultural policy in Malaysia, as elsewhere. By ignoring the societal framework, this study evades the policy questions concerning rural development.

To some degree this study, carried out in 1962–63, has been outpaced by subsequent economic events. It stands, nevertheless, as an example of the strengths and weaknesses of the incomes-accounting technique applied to peasant agriculture. On the one hand, the book contains a wealth of detail on the income flows with the Malayan village economy. On the other, it abdicates consideration of the real-world setting and related policy dynamics.

Martin Rudner

The Hebrew University of Jerusalem

The Mechanics of Independence: Patterns of Political and Economic Transformation in Trinidad and Tobago. By A. N. R. Robinson. (Cambridge, Mass.: The MIT Press, 1971. Pp. 200. $8.95.)

The author of this book is, as West Indians would say, a sometime "blue-eyed boy" of Dr. Eric Williams, Prime Minister of Trinidad and

Tobago. He has written an insider's view of the opportunities for—and obstacles to—economic reform facing a nationalist regime in a former British colony newly tranformed into a politically independent state. Robinson knows the facts well. He was deputy leader of the nationalist party and he served as Attorney General, Minister of External Affairs, and Minister of Finance.

The book is divided into three parts. The first contains a brief summary of the Spanish and British colonial periods, the latter being separated chronologically into events of the 19th and 20th century, and a somewhat more detailed discussion of the formation and collapse of the West Indies Federation, some of which is based upon the author's own participation and observation. Like drug-store Beaujolais it does the job, but one can find better elsewhere. In Part 2, however, interest picks up. Here are details of independence and economic reform that are often difficult or impossible for a political or social researcher to obtain through his own field work. How does one go about creating a new savings and investment program? How are the former subjects whose interests were neglected under the colonial system brought into the emerging and changed economic activities of the country? How can economic reforms promote economic and social mobility while increasing economic growth? How can the new state gain monetary control from metropolitan powers? How can the control of internal economic structures be transferred from external to internal management?

These and related questions are answered by a review of major reforms of which the author was one of the architects, an analysis of opposition to them, and a discussion of strategies and tactics by which compromise was reached. Included, among other things, are tariff reforms, tax reforms, banking acts, an insurance act, a finance act, and the negotiation of related treaties with foreign governments. There is more than enough documentation given to show both how the colonial economy was stacked in favor of metropolitan interests and how political independence itself did not eliminate such interests. They still must be dealt with, and in Trinidad they were managed with incredibly reasonable negotiation and compromise. All of the data presented are useful grist for the social scientist's mill.

In Part 3, Robinson takes up the dynamics of transformation—its goals, its internal and external constraints, and its economic and social needs. It helps, especially in reading about internal constraints, to know that Robinson had had a falling-out with Williams. Thus, the emphasis on "the leader"—in this case, Williams—and what he should or should not do, and what he must guard against is understandable as is a somewhat pessimistic view of the general change in nationalist leaders in the years since independence. A loyal but often dissident patriot speaks: ". . . the qualities of leadership required to attain independence are wholly unlike those required to succeed in independence" (p. 130). "As political strategy develops, government as well as party powers are used against party dissidents" (p. 130). "The [political] climate and the system will have begun to eliminate all but party hacks from positions of power. The survivors will tend to be tough people skilled in political intrigue, highly ambitious, but hardly dedicated to the transformation of the society" (p. 131). "With the growth of the authority and stature of the leader, dissent becomes progressively less tolerated, and it is equated with disloyalty" (p. 131). "The first road to success is adulation of the leader, and one must know how to read the road signs" (p. 132). The leader ". . . must be a shrewd enough judge of human nature to distinguish the false from the genuine" (p. 132). If the leader errs, "a scapegoat must now be found to blame" (p. 133). "If the leader senses the dry rot early enough, his earlier qualities may reawaken and inspire him to arrest the deterioration" (p. 133). Sour grapes? Perhaps. But it all sounds suspiciously familiar and probably is another case of a quite general phenomenon.

A chapter on "The Future of the Caribbean" concludes the book. Robinson sees three courses that the peoples of the Caribbean can follow. Two of them he rejects: "collaboration with the Communist bloc along Cuban lines" and "reversion to colonial dependence—this time on the United States" (p. 167). What he sees as necessary for the achievement of nationalist goals of genuine local control, continued development, and equality of opportunity are the political integration of the English-speaking Caribbean and a larger integration, economically, "with either the Central American Common Market or Latin American Free Trade Association" (167).

This is a good book. There are some useful raw data. There is another statement of the nationalist cause, goals, and effort. Yet one can hope for more. Someday, I hope Robinson will tell all he knows, without defensiveness or acrimony, about his participation in the decisions of nationhood in Trinidad and Tobago.

WENDELL BELL

Yale University

Managers of Modernization: Organizations and Elites in Turkey (1950–1969). By Leslie L. Roos, Jr. and Noralou P. Roos. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 292. $14.50.)

The two authors have actually written two books in one. One is a fascinating account of how, beginning in 1950, "ruralizing elections" in Turkey decreased the position and power of the bureaucratic elite and how, in turn, the members of this elite reacted to modernization and their loss of status. The second is an attempt to test certain hypotheses from organization theory in the setting of a declining administrative elite in a developing country. The authors have quite deliberately taken both approaches since their "study aims to link the discussion of macro- and micro-level problems. Certain hypotheses from organization theory are relevant to problems of mobility and social change. On the other hand, particular kinds of system-level development may modify organization theory propositions which have held true in other settings" (p. 15). The result of combining the two objectives is an unintegrated but important study of one aspect of Turkish modernization.

Ruralizing elections, according to Huntington, represent the coming to power of new rural voters and the replacement of the urban-based modernizing elite by a noncosmopolitan, local elite. According to Quandt, other countries with a declining cosmopolitanism of elites include the United States, India, the Philippines, Ceylon, the Soviet Union, China, and Indonesia. The behavior of the administrative elite may be extraordinarily important in response to ruralizing elections. For example, the authors contend that the stress upon administrators "helped create a climate facilitating the military coup of 1960" (p. 7).

The Turkish case is an apt one, since the bureaucracy in Turkey thrived during the Kemalist era of social and economic reforms. Bureaucrats, particularly in the Ministry of the Interior, and military men were charged with implementing the reforms and controlling conservative reaction. The very success of the Kemalist policies in bringing about structural change, however, helped lead to the eventual displacement of the bureaucratic elite. Kemalist economic policies establishing public enterprises created "another center of influence largely independent of the central bureaucracy" (p. 35). To compete with the private sector for technical personnel the public enterprises were "forced to upgrade the positions of its technical and professional personnel . . . the upgrading took place at the expense of the person who had long been at the center of the Ottoman-Turkish government—the generalist administrator" (pp. 38–39).

The status of the bureaucrat was further diminished by the implementation of Inonü's policies to develop multiparty government in Turkey. The major contribution and the most fascinating theme of the book is that the democratization of Turkish society resulted in the decline of power and prestige of the bureaucratic elite. The peasant vote curtailed the power of provincial government and hence the prestige of district and provincial governments, and resulted in a sharp decline in the number of administrators and bureaucrats elected to parliamentary positions.

Chapters 4 and 5 discuss in greater detail how various organizations were affected by the changes set forth above. Chapters 6–9 treat the behavior of Turkish administrators during the period of declining power and prestige. In 1956 administrators were still reeling under the impact of modernization and political competition; by 1965, they appear to have adjusted to their new environment. Three reasons are given for the diminished conflict experienced by the administrators: (1) a lessening of local interference in administrative matters, (2) acceptance by the administrators of their diminished role, and (3) growing acquaintance with the ways of local leadership.

Many of the attitudes expressed by the administrators seem determined by the individual's experience at his place of work. For example, the type of job situation was a primary determinant of the administrator's interaction and friction with peasants and politicians. On the other hand, what was an intolerable situation for an administrator was often solved by leaving the central bureaucracy for jobs in the new noncentral organizations, including public and private enterprises as well as the State Planning Organization.

Chapter 10 looks mainly at the attitudes of administrators, on the one hand, and villagers, on the other, toward their relations with each other and toward development. The administrative elite tended to see the needs of the villagers with respect to development quite differently from the way the villagers saw their own needs. The administrative elite saw education as the key to development, whereas the headman saw water, roads, and land as the keys. It is interesting that, since the ruralizing elections, the emphasis in development programs shifted exactly toward those objectives that the villagers and peasants desired. Furthermore, the district governors and other members of the administrative elite have had to abandon the high-handed approach of telling the peasants what to do in favor of a gentler method based on persuasion.

The use of organization theory has provided the book with a rich framework for analysis of an important transitional period in Turkish development. This framework has permitted and encouraged examination of variables ordinarily omitted from studies of modernization. However, there are two pervasive difficulties with the book. The first has to do with the definition of the bureaucratic elite. The second, already mentioned, has to do with the difficulty of implementing the dual objectives of the authors.

The basis of the bureaucratic analysis is, for the most part, a panel study of graduates of the Political Science Faculty based on two sets of questionnaires (1956 and 1965), and a second 1956 survey of the social backgrounds, time-budgeting, communication patterns, and activities of Turkish district and provincial governors. The Political Science Faculty graduates have been singled out for study before, and there is a wealth of data available about them. In the past, this small, elite corps has constituted the largest group of administrator-politicians. The authors follow Frey in identifying the Political Science Faculty graduates with the elite. This definition made sense until the early 1960s, when economists and engineers with training abroad or Middle East Technical University graduates became highly sought after. The authors do not consider the alternative hypothesis that since 1960 the composition of the elite has changed. They are prevented from doing so by their narrow identification of the elite with the graduates of one school.

The chapters dealing with the testing of organization theory are tedious. Each follows a similar format: There is first a brief general statement or hypothesis drawn from organization theory, amply documented with references to the literature. There is then an attempt to determine if the Turkish data are consistent with these statements or hypotheses. The result is a technical discussion, the substance of which is not very different from that already drawn in the first three chapters.

Nonetheless, the application of organization theory to the task undertaken by the Rooses is fruitful. On the whole, the book will influence research in this area and has added considerably to our knowledge about the effects of modernization and political competition. It is unfortunate that the dual goals pursued by the authors resulted in an unintegrated book, marring an otherwise exciting study.

James W. Land

Rice University

Legislative Recruitment and Political Integration: Patterns of Political Linkage in an Indian State. By Richard Sisson and Lawrence Shrader. (Berkeley: Center for South and Southeast Asia Studies, University of California, 1972. Pp. 54. $3.00, paper.)

This monograph attempts to "explore the character of elite-mass relationships and their salience for political integration in an Indian state" (p. 2). The state is Rajasthan, on which both authors have previously written. The elite-mass relationships are explored entirely from the perspective of "the elite looking out" (p. 54) through analysis of the background characteristics and interview responses of Legislative Assembly members (MLAs). The social background analysis covers the first three assemblies (1952–57, 1957–62, 1962–67), but the interviews apparently cover only the third.

The general argument of the book is that though MLAs differ from the general population in their education, mobility, and caste composition (chapter 2), they are "linked" to the public order through their general agreement on basic policy issues (chapter 3), to their constituents through ties of birth, language, and constituency contact (chapter 4), and to each other through their membership in cross-cutting secondary associations, party organizations, and legislative factions (chapter 5). The data supporting these arguments are presented in twenty-two tables which show the percentage distribution of MLA responses within the categories of party, caste, rural/town/urban residence, and political generation. Among the authors' findings are (1) that caste representation among MLAs has tended over time to become more like that of the general population; (2) that legislative factions within the Congress Party have become less numerous and more pluralistic in their regional and caste composition; and (3) that earlier recruits to political life have tended to be more involved in social uplift associations while later ones have come more from caste associations or government-sponsored *panchayat* and cooperative organizations.

Though the authors wish to explore relationships, their approach is limited almost entirely to the interpretation of frequency distributions. Occasionally they discover a relationship, but most of their observations feature vague phrases like "noticeable proportion" (p. 44) and "reasonably high level" (p. 52) because of the lack of clear criteria for determining whether a specific distribution is indeed high or low. The further absence of statistical measures of relationship or significance leaves the authors with inconclusive statements like "While only 55 per cent of Congress legislators spend over half their time in their constituencies, 78 per cent of opposition MLAs do." In several of the tables, moreover, the number of responses is so low that percentages are inappropriate. Table 19, for instance, shows the distribution of MLAs who have changed parties. It reveals that 7 per cent of those now in Congress and 5 per cent of those now in Swatantra were formerly in Jan Sangh, while 17 per cent of those now in Jan Sangh were formerly Socialists. Since the totals for Congress, Swatantra, and Jan Sangh are respectively 15, 21, and 6 persons, however, these impressively diverse percentages, we discover, represent only one MLA each (p. 43).

The greatest shortcoming of this work is the authors' failure to utilize their data as effectively as they might have. They show, for instance, that 69 per cent of the MLAs were born in their present constituencies (Table 12) and that 59 per cent list local dialects as mother tongue (Tables 12 and 13). They then argue that "This pattern of close social linkage between the MLA and the mass society is paralleled by the way the legislator spends his political time," and show that "67 per cent spent over half their time within their constituencies" (p. 33). The implied relationship between social affinity and constituency contact, however, is left untested. Many additional questions relating constituency linkages, attitudes, organizational ties and patterns of political recruitment remain unexplored, despite the book's title and declared intent.

Such criticisms should not detract from this work's contribution: its application of a sizable amount of integrative theory to elite interview data and its identification of several interesting questions which more demanding analysis might seek to answer.

William L. Richter
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Victor Emanuel, Cavour, and the Risorgimento. By Denis Mack Smith. (London, New York, and Toronto: Oxford University Press, 1971. Pp. 381. $22.50.)

In the eyes of a political scientist concerned with the problems of today's Italy, this book by historian D. Mack Smith (the renowned specialist in Italian modern and contemporary history) performs a valuable service. It shows poignantly how weak was the basis of representative government set up in Italy when the country was freed from foreign rule and unified in the last century. Indirectly, it helps to explain certain unfortunate developments of subsequent Italian history as well as the deep roots even of some of its present troubles.

The book's fifteen chapters (some of which reproduce essays already published elsewhere) deal with the contributions to the shaping of the new state made by the two most powerful Italian political leaders of the period. The author chooses a number of controversial episodes and historical problems involving the king and Cavour and looks at them in the light of the documentation now available. But he is also able, through this analysis of scattered episodes and problems, to give an indirect idea of almost all the features of the new political organism that emerged from the Risorgimento and of its shortcomings.

He points out, for instance, how dangerously incomplete was the checking of the powers of the Crown within the frame of the liberal constitution adopted since 1848. He traces back the origin of "transformism" (a method of parliamentary government that became a common practice still occurring in Italy, instead of the more orthodox rule of the alternation in power of parties clearly opposed to each other) to a political strategy devised and followed by Cavour. He reminds us that the "Risorgimento" was a series of civil wars as well as of wars to gain independence from Austria; that in the struggle the "moderates" managed to exploit shrewdly to their ends both the enthusiasm of the more idealistic and truly patriotic democrats and the exasperated local reactions of rural masses oppressed by poverty; that they thereby managed to establish a political system generally responsive to the needs of the upper middle class but unable to attract national consensus and threatened internally by deep social cleavages. He is able to show how fraught with dangers was the choice made by Cavour and his successors to set up a strong, bureaucratic central government, instead of adopting "regionalism" as the structure of the newly established Italian state.

If one may venture a criticism, the trouble with Mack Smith's interpretation is that at times he gives the impression the "Risorgimento" might have had a more advanced outcome than the one it actually had. He is well aware, of course, that (as he puts it at page 35) the remarkable fact "was not that the liberal institutions," introduced in Italy in the period, "were left imperfect, but rather that

Cavour left them as healthy as he did." Yet he does not seem always to remember this unquestionable truth. He tends to evaluate the conduct of the men and social groups that unified Italy overlooking in part the dramatic efforts they had to make in order to conquer, within their very selves, many old habits pertaining to an old mentality and the terribly complicated and unfavorable circumstances under which they had to operate. As a result, his judgments on the "moderates," on the king, even on Cavour, seem here and there to be prompted by an idea of what would have been politically desirable in the abstract, rather than being the product of a genuine, sympathetic historical understanding.

Having said this, one must add that the book remains nevertheless an important contribution to the shaping of a more realistic image of the Italian "Risorgimento." Italian nationalist historians have often drawn a golden picture of it, which was basically false and, although by now generally out of fashion, should be set aside entirely. To this end Mack Smith's book will be of great help. Inasmuch as the present cannot be fully understood but through the history of the past, it will also be a useful tool for every student of contemporary Italian politics.

GIOVANNI BOGNETTI

University of Pavia, Italy

The Geography of Modernization in Kenya: A Spatial Analysis of Social, Economic, and Political Change. By Edward W. Soja. (Syracuse, N.Y.: Syracuse University Press, 1968. Pp. xi, 143. $11.00.)

Areal variations within nations in the process of modernization greatly influence politics in many societies, particularly those characterized as nonindustrial, so many political scientists should find this analysis by a geographer of spatial patterns of modernization in Kenya useful. Concepts such as "nodal region" and "density surface" will be helpful for political scientists wishing to employ spatial rather than historical approaches to the study of modernization. Moreover, for those particularly interested in Kenya, this book focuses on variables that, along with those associated with personality, best explain Kenyan politics in the 1960s. The development of political parties in Kenya, for instance, can be explained to a large extent by regional differences in modernization.

Two of the themes running through Soja's book, the interrelationship among variables associated with social mobilization and the saliency of communications in modernization, are familiar in political science, because of the wide circulation within the discipline of the works of people such as Karl Deutsch and Daniel Lerner. Soja's third main theme is the spread of modernization by spatial diffusion, which is drawn from the discipline of geography and, in particular, from the work of Peter Gould. One of Soja's contributions to comparative government is that he applies concepts like social mobilization to intrastate analysis, in this case to a study of Kenya, rather than at the international level.

Data on items ranging from telephone traffic to ethnic breakdowns of educational achievement were collected from a variety of sources, including the 1962 Kenya census, statistics provided by various government ministries and organs of the East African Community, interviews with district officers, and, for a map of Somali insurgency, innovative coding of newspapers. The author maintains good control over his data, as is not always the case for large collections, and one has to go to rather tangential footnotes to find any errors. The author states in one note (p. 67) that the parliamentary seats for the Northeastern Region were finally filled in February of 1964, whereas Wajir South remained vacant until October. The most serious problem with the data stems from a factor over which the author had no control. The data are based on districts as they were drawn before independence in 1963. Unfortunately, from a scholar's point of view, district lines were modified after independence, thus frustrating comparison of Soja's data with those collected after the publication of his book, particularly those based on the 1969 census.

Forty-eight figures illustrate well the interrelationship among variables, but Soja also identifies patterns in the modernization process by applying factor analysis to 32 variables measured by district. The particularly high loadings for nearly all communications indices seems to confirm the saliency of communications in modernization. In addition, because of the relationship of many aspects of modernization to variables on "connectivity to the core area" (Nairobi), the author feels that his study strongly supports the view of modernization as a process of diffusion. However, in a country like Kenya with an economy based on agriculture, climate greatly affects settlement patterns and agricultural potential. Thus, development in contiguous areas is virtually inevitable and does not necessarily confirm any general link between diffusion and modernization.

Except for the addition of a somewhat extraneous tenth chapter on implications for further research and some refinements in the graphics, the book is identical to the author's 1967 dissertation. On the whole, the book is a valuable contribution to the study of modernization and Kenyan politics. The data presented are interesting and well used. The illustrations are attractively presented and instructive. The methods, though applied to a single country, are truly comparative. These strengths justify the increased accessibility of this work that its liberation from the microfilm collection makes possible.

Jay E. Hakes
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Decentralization and Self-Government in Russia, 1830–1870. By S. Frederick Starr. (Princeton, New Jersey: Princeton University Press, 1972. Pp. xiii, 386. $15.00.)

Professor Starr's splendid book studies a poorly understood, much neglected, and yet vital aspect of Russian institutional history in the 19th century: local government. Both Russian and Western historians have focused their attention on events in the capital and slighted the local scene, in spite of the marvelous pictures given of it by the great Russian writers. Mr. Starr goes a long way in righting the perspective. To this end he has gone through a vast amount of new archival and long-neglected printed documentation, using to excellent advantage the voluminous files of various government commitees as well as private papers of leading statesmen and publicists. He thus gives depth and density, as well as an intensely human quality, to a story that heretofore has been told only superficially and schematically.

Mr. Starr starts his discussion of the preparation and enactment of reforms in the domain of local administration with a detailed picture of the local scene during the reign of Nicholas I. He substantiates with figures and official reports the dismal state of provincial government that we learned to infer from the masterpieces of Gogol, Saltykov-Shchedrin, and Turgenev. The original conclusion he reaches is that the main reason for the maladministration of the Russian provinces was the fact that they were undergoverned, with many needed services and functions completely ignored by St. Petersburg. The defeat suffered in the Crimean War pointed to the need to "modernize" the economy as well as the army and administration and provided the stimulus for reform. In a refreshingly novel chapter (II) Mr. Starr describes the discussions concerning local administration, deconcentration, federalism, and self-government in the 1850s. He singles out the intellectual influence of Tocqueville's *L'Ancien régime et la révolution* and the idealized picture of English circumstances held by the Russian elite on the basis of French political tracts and contemporary press reports, and he also stresses the experience gained by provincial society in organizing the militia and other auxiliary services during the Crimean War.

With the factual and intellectual background clearly sketched in, Mr. Starr gives a detailed, sophisticated analysis of the meandering path toward the reform of local administration which culminated in the *zemstvo* legislation of 1864. The story develops like a fugue, the two main themes in contrapuntal tension: deconcentration of administration within the bureaucratic framework and drive for self-government or devolution of authority from the bureaucracy to "society." Mr. Starr's account is concise and clear, but all the nuances and contradictions are analyzed with a sharp eye for and understanding of the period and the main actors' positions. The volume closes with a rapid survey of the subsequent evolution of the *zemstvo* reform and a convincing explanation of the dynamics of its modest success and eventual "perversion." Here again Mr. Starr's strength lies in his understanding of the historical antecedants and the traditional patterns of thought and behavior that caused the *zemstvo* institutions to produce a split, rather than reconciliation, between society and the state.

It is not the least of Mr. Starr's merits that he treats an essentially political problem in terms of politics. We learn of the frequently contradictory interests, perceptions, and motivations that determined courses of action, and we get to understand the hesitations of those who were in power as well as of the leaders of "public opinion." As a genuine historian, Mr. Starr tries to understand, analyze, and explain the actions of the contemporaries in terms of what *they* knew and of the options *they* perceived. As a result, without his giving us biographic details or psychological speculation, the main participants emerge as live persons (e.g. Count S. S. Lanskoi, the "hero" of the first stage of reform), and we catch a glimpse of the human dimension of the problem. How much fuller and more convincing this picture is compared to the fashionable schemata of socio-economic dynamics and hypothetical projections of group consciousness and "objective class interests" derived from abstract notions resting on hindsight!

The publishers have served Mr. Starr well, producing a book with most attractive and legible type and page format. Unfortunately, they have not been very meticulous in correcting typographical and spelling errors in the footnotes and foreign words. Four minor mistakes might be noted: the "Marshal Weilopolski" mentioned on p. 257 was, of course, Marquis Alexander Wielopolski; the assailant of Alexander II in 1866 was Karakozov (not Karakazov); Nicholas Turgenev was already abroad when the Decembrist revolt occurred in 1825 (pp. 266–67 note); the governor of St. Petersburg cited on p. 299 was A. A. Suvorov, Count Rymnikskii (not Suvorov-Rumynskii). Minor quibbles in a first rate and extremely intelligent volume!

Marc Raeff

Columbia University

Directory of Selected Scientific Institutions in Mainland China. Prepared by Surveys and Research Corporation. (Stanford, Calif.: Hoover Institution Publication Series #96, 1970. Pp. 469. $19.50.)

This volume attempts to present the organization of Research and Development in the People's Republic of China in the period prior to the Cultural Revolution. It contains information about 490 selected scientific institutions concerned with the physical, biological, medical, and agricultural sciences. These data include the name, location, year established, type of facilities, nature of work, major activities, publications, and brief biographical sketches of the known directors. The introduction provides an evaluation of the management of the science and technology sector; the role of the academies of science and of government; the purpose of the science information system and a tabulation of the discipline concentration and geographic location of all the institutes described.

Although the directory is dated, it does present a fairly comprehensive view of the development of science and technology in China. It is also useful to China watchers who can quickly refer to its pages to discover changes in the scientific establishment in the post-1966 period. The introductory material is, however, laborious rather than illuminating. The organization of the book is, when not obvious, seemingly arbitrary. Other drawbacks arise from the lack of information necessary to make the compilation uniform, a problem beyond the authors' control. Despite these pitfalls, the directory does remain a volume of great usefulness to specialists.

Susan Beth Rifkin

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Labor and Society in Tsarist Russia: The Factory Workers of St. Petersburg, 1855–1870. By Reginald E. Zelnik. (Stanford, Calif.: Stanford University Press, 1971. Pp. 450. $15.00.)

In recent years a spate of books has appeared on what used to be called "the lower orders," groups largely ignored in traditional histories except as a faceless mob which appeared periodically to mess things up (1789, 1830, 1848, etc.) then to withdraw and leave the management of history in charge of its betters. All the works of this kind with which I am familiar share a romantic view of the working man, whether peasant or industrial laborer. He is virtuous because he suffers, and he suffers all the time: historically his condition deteriorates continually, and no matter how bad one day, the next it is worse yet. He has no feelings, either elevated or base, no loyalties to the established order: resentment occasionally exploding into violence is his only response to life, and the task of the historian is to take periodic readings of the temperature of his hostility. I may add that this "noble savage" view of the working man bears little relationship to psychosociological studies of the laborer such as the eye-opening *L'ouvrier d'aujourd'hui* by A. Andrieux and J. Lignon or to economic analyses of the progress of real wages.

Professor Zelnik's work is in this tradition. It is an extremely thorough study of the minuscule industrial working class of St. Petersburg (a mere 18,000 strong in 1861) at the point in time when government and society in Russia first became conscious that they had a "labor problem" on their hands. The dearth of material on the daily life, mentality, and aspirations of the workers forces the author to concentrate on government commissions and the various circles of public opinion. The factual evidence is analyzed with exemplary thoroughness and caution. The author stops in 1870 when the strike of spinners at the Nevskii factory seemed to presage the end of an era characterized by "weakness of spirit of protest" and the beginning of an age of industrial agitation. The present volume is to be followed by a sequel devoted to the second half of the reign of Alexander II.

The methodology adopted by the author is old-fashioned. The basic assumptions are those formulated by Russian Social Democrats at the end of the nineteenth century, and so is the terminology. The emancipation of serfs in 1861, for example, is depicted as an event which only superficially altered their condition, "replacing the former bondage of the peasants to their landlords with an almost equally restrictive, legally binding, and virtually inescapable attachment to the village commune" (p. 203): i.e., we had no *"osvobozhdenie krest'ian"* (liberation of serfs) but only a *'krest'ianskaia reforma'* (peasant reform). We encounter on the pages of the book "utopian socialists," "populists," "class conflict," "revolutionary democrats," and all the other familiar friends of orthodox Marxist historiography. And November 1917 always looms in the background as the dénouement of the drama.

In Professor Zelnik's approach I was most troubled by his tacit assumption that Russian industry, like its Western counterpart, was essentially an urban phenomenon. In fact, most Russian industrial establishments in the period with which Mr. Zelnik deals were located in rural areas and drew their labor directly from the village. The history of the Russian labor movement is perhaps as much a chapter in the history of the *muzhik* as in that of the "proletariat." Had the author taken this fact into account, he would have had an easier time explaining the mind and psyche of the Russian industrial worker because he could have drawn on the excellent belletristic and sociological literature on the rural population.

The most valuable parts of the book are those devoted to the intellectual aspects of the subject, namely, the initial reactions to problems created by industrialization of a society accustomed to regard them as irrelevant to Russia. To my knowledge there is no study that tells this story more fully or better. I was particularly struck by the ideas on mutual aid of one F. E. Terner which seem to have exercised much influence on worker circles of the 1880s and 1890s. In other words, the value of the book lies in its contribution to intellectual rather than to social history.

RICHARD PIPES
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The Political Elite of Iran. By Marvin Zonis. (Princeton, New Jersey: Princeton University Press, 1971. Pp. 389. $12.50.)

That His Imperial Majesty stands at the epicenter of Iranian political life few question. More in doubt are assertions about who shares the Shah's extensive powers and what accounts for the regime's relative stability. Controversy also extends to the legitimate modes of political inquiry. Pet theories of causation are commonplace. Observers frequently contrive elaborate competing, usually conspiratorial explanations to fit virtually every political event. Even astute commentators are sometimes seduced by the self-serving logic of the regime's detractors and its apologists. Professor Marvin Zonis's study of the Iranian political elite refrains from simplistic theories and familiar polemics. The book admittedly carries its own analytic bias, namely, that the values and attitudes of elite actors offer the most valid means to understand contemporary Iranian politics. But Professor Zonis has few disciplinary blinders. He draws intelligently on historical examples and literary sources to suggest a dominant elite political culture. He exhibits sensitivity to power relations and acknowledges the significance of informal networks in reinforcing and transmitting elite values. He is a discerning student of political strategies and their supporting resources. Ultimately, however, it is Zonis's skills as a social scientist and the empirical evidence he offers on the composition and orientations of the political elite that determine his contribution to the scholarship on Iran.

Borrowing liberally from personality and attitude theory for his concepts and instruments, Zonis identifies four major elite character orientations—political cynicism, personal mistrust, manifest insecurity and interpersonal exploitation. Zonis thereby departs from most political elite studies in his reliance on structured interviews for data. To select his respondents the author employed an expert panel whose attributions of power narrowed the elite to 307 people, of whom 167 were finally interviewed. The decision to reject an issue or decision approach as a guide to the politically influential can be appreciated by anyone who has observed the normal masking of responsibility in the Iranian hierarchy. At the same time, the reader is left somewhat uneasy with Zonis's failure to explain how a reputational method manages to overcome many of the same problems of perception and information. The findings themselves are necessarily qualified by the Shah's prohibition against the interviewing of active military personnel. Yet Zonis's careful and self-conscious procedures, his independent analysis of nonrespondents, and the very opulence of his data relax most doubts about design. Indeed, the author shows praiseworthy skills in penetrating surface attitudes and opinions within this ordinarily guarded population.

A central theme pervading the book is that "the longer and more thoroughly a member of the elite participates in the Iranian political system, the more he manifests personal attitudes of insecurity, cynicism and mistrust" (p. 15). Ample support for this proposition comes from associations found between these three character orientations and elite career backgrounds and attitudes. Still, Zonis's findings are not always consistent, nor are all his explanations similarly compelling. For example, the data reveal that the level of cynicism fails to rise as anticipated with the study's measures of participation and general exposure to the political scene. The author's explanation argues that his respondents include the as yet less active but highly cynical "new men" (most well educated but of more modest social origins) along with more active but less cynical older elite types. By this reasoning, then, cynicism seems more likely to be a generational or social phenomenon than it does an experiential outcome of immersion in the political system. Also, it may strike some as odd that personal exploitation, the fourth orientation identified, is not related to the larger character syndrome and correlates with a different set of social background factors. In fact, the description of an elite element positively oriented toward personal exploitation is anticipated from other studies. Most recently, James Bill in *The Politics of Iran: Groups, Classes and Modernization* distinguishes at length between the system's manipulators and its more newly recruited technocrats. But armed with interview data on communication linkages, Zonis is able to show that "politicos" are usually isolates in the major networks of political influence.

Readers inclined to more traditional analysis will find provocative the book's early chapters dealing with the Shah's methods of elite and counter-elite control. In all probability, Zonis's observations on recruitment to the elite offer the most illuminating descriptions of the cooperative technique found anywhere in the developing nations' literature. By themselves, these chapters furnish a highly readable introduction to the major personalities and events in recent Iranian history. Unfortunately, the text too often reflects the fact that the bulk of the author's research was undertaken in 1963 and 1964. At least several statistical tables warranted updating for a volume written for the 1970s. The post-1965 economic boom has not been overlooked, but the author treats too lightly its implications for urban classes less subject to financial insecurity. Moreover, he fails to recognize the role of the government's Iran Novin party in the processes of elite recruitment and popular supervision. And no mention is made of the still counterfeit but potentially instrumental elective local and provincial councils created in the late 1960s.

Zonis turns in a final chapter to an intriguing question. Why has this largely cynical, mistrusting, and insecure political elite not subverted the complex political system? His conclusion that the Iranian elite has chosen to cope with the system rather than to challenge it fundamentally is perhaps self-evident. Nevertheless, the author's analysis greatly sharpens our understanding of this remarkable *modus vivendi* between the Shah and his elite. In what Zonis calls the "flight from responsibility," much of the elite is seen as reaping economic benefits in exchange for their sacrifice of principle and loss of community. The cost to the nation in terms of human resources and intellectual growth is staggering. But the resulting bureaucratic personalization, conservatism, and inefficiency assure the Shah's indispensability and his continued grip over decision processes. Zonis argues persuasively that the very success of the Shah's policies have slowed Iran's transformation to a modern state. The same elite orientations that bring regime stability have also effectively precluded realization of many of the Shah's most ambitious developmental goals.

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Cuba 1933, Prologue to Revolution. By Luis E. Aguilar (Ithaca, New York: Cornell University Press, 1972. Pp. x, 256. $9.50.)

It is probably no mere coincidence that some of the best books on today's Cuba have been written by historians. As brilliantly demonstrated by Hugh Thomas, Fidel's revolution has been "the culmination of three generations of revolutionary activity." Luis Aguilar, a Cuban exile and during its first year, a sympathizer of the revolution, has written a well-documented and perceptive book on the 1933 revolutionary episode which adds depth and perspective to our understanding of Castroism and Cuba's obsession with freedom.

After a brief summary of the first years of independence which transformed the Cuban republic into "a political instrument for the protection of many foreign economic interests and some national ones which had allied themselves with the conquerors," Professor Aguilar analyzes Machado's rise to power in the 1920's and the way his dictatorship confirmed the traditional axiom of Cuban politics before Castro —to rule was to obey, "to harken to the demands of the socio-economic complex which actually controlled the republic" (p. 29). The author then describes the main currents which swept Cuba from the 'twenties on and the various social forces which were to lead the fight against the dictatorship and American domination: the intellectuals, the University, labor, and the fledgling Communist party. In the following chapters, he gives an original and vivid account of the main episodes of the 1933 revolution, Sumner Welles's mediation and constant meddling in Cuba's political life, Machado's downfall, the failure of the provisional de Cespedes government, the sergeants' coup and the takeover by a "strange triumvirate" Grau San Martín, Batista, and the true power behind the revolutionary government, Antonio Guiteras.

The chief reasons for the final defeat of this revolutionary regime are clearly stated: the intrigues of U.S. ambassador Sumner Welles and his encouragements to Batista. Welles's hostility towards the Cuban government was partly personal—had he not constantly misrepresented the situation in Cuba, Roosevelt would perhaps have recognized Grau San Martín—but also derived from deeper motives: Grau's government (which, according to the American ambassador could not last more than a few days, stayed in power for more than four months, carried out the most radical and profound process of transformation in the history of the republic" [p. 167]) abolished the Platt amendment and began a program of land distribution. No wonder that U.S. ambassador, Jefferson Caffery, shared the contempt of his predecessor in Havana for this government of "ignorant masses."

Professor Aguilar, however, is too good an historian to rely on a simplified explanation of the failure of the 1933 revolution. Welles's meddling succeeded largely because of the depression which aggravated the island's economic dependence on the U.S. and because of the endemic Cuban political instability: not only did the revolutionary government lack the help of a party to organize political support on its behalf, but revolutionary forces—the Student Directory, Guiteras' *Joven Cuba,* the ABC Radical, the Communist Party—were fighting among themselves. Aguilar's analysis is particularly enlightening in that respect.

The last chapter of his book, to me at least, seems the weakest part of this sympathetic and intelligent account of a crucial episode in Cuban history. After explaining at length why and how the 1933 revolution was finally defeated, the author tells us that Cuba emerged from these four months of revolutionary rule deeply and radically transformed. Then he wonders why "after such remarkable progress brought about by a revolutionary process, a nation considered prosperous by Latin American standards . . . was to enter, after a few years, into the terrible throes of a socialist revolution" (p. 238). The answer lies perhaps in two contradictory aspects of the 1933 episode. As Professor Aguilar saw very well, the revolution reawakened the forces of nationalism and reform which were to rally around Fidel Castro a quarter century later. But was Cuban society really deeply and radically transformed? The 1933 revolution provided an opening to new elements in Cuban political life, students, professionals, lower-ranking army officers, but this process was later corrupted and closed off entirely after Batista's coup in 1952. Further, as noticed by Bryce Wood in *The Making of the Good Neighbor Policy,* the postrevolutionary agreements of 1934 left untouched the pre-1930 economic and social structure in Cuba as well as the domination of the Cuban economy by foreign interests.

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Nuclear Proliferation: Prospects for Control. Edited by Bennett Boskey and Mason Willrich. (New York: The Dunellen Company, 1970. Pp. 191. $7.50.)

It is difficult to be optimistic about the possibility of preventing nuclear proliferation. The intractability of many conflicts among nonnuclear states, the increasing amount of fissionable material which is becoming available to potential nuclear powers, the fact that it is now probably possible to develop nuclear weapons without going through a detectable test program, and the continued status and prestige associated with nuclear power—these and other reasons hardly suggest that proliferation can be prevented for very long.

Does it make very much difference? To those who believe that the process of diffusion can be successfully "managed"—in the same way that the international system managed to adapt to innovations like gunpowder, the dreadnought, and the airplane—the answer is no. In a similar fashion, advocates of neo-isolationism for the United States believe that the dangers of proliferation are overrated; after all, even if proliferation does occur, there is no reason to presume that the United States need be implicated in whatever nuclear conflicts ensue. Thus the arguments that proliferation cannot be stopped and that it is unlikely to lead to American involvement in a nuclear conflict join to lessen official and public concern with what may be the preeminent issue of the next two decades.

The authors who have contributed to *Nuclear Proliferation: Prospects for Control* do not share these views. Although writing in the immediate aftermath of the negotiation of the Non-Proliferation Treaty (NPT), they remain very aware of both the dangers of proliferation and the weaknesses of the treaty itself. They could be described most accurately as mildly optimistic, in the sense that they believe the treaty has bought us a little time to work out a more thorough and profound approach to the political, economic, and psychological problems that might make taking up the nuclear option look increasingly attractive.

None of the essays breaks new intellectual ground in the proliferation debate. Nevertheless, they are all useful, for they summarize and analyze a great deal of material in a clear and persuasive style that should make the book particularly useful for the non-specialist. For example, George Rathgens's comments on the possibilities of stabilizing the strategic balance in the context of the interaction between ABM's and MIRV's. His essay provides an excellent framework for anyone interested in understanding the background to the current SALT negotiations. George Bunn's essay analyzes some developments which might undermine the Non-Proliferation Treaty itself: e.g., the dissolution of NATO, a continued strategic arms race between the United States and the Soviet Union, the acquisition of nuclear weapons by certain "threshold" states like India or Israel, and the actual use of nuclear weapons against a non-nuclear state.

The middle section of the book is concerned with the relationship between peaceful uses of nuclear power and the possibility of diverting enough fissionable material to acquire secretly a weapons capability. Obviously, a civilian program cannot help but make more fissionable material generally available, and thus enhance the possibility that one constraint against proliferation will gradually disappear. Moreover, no safeguard system can prevent substantial diversion by any state so inclined: the most that a safeguard system can do, as John Gorham Palfrey notes, is improve the likelihood that diversion will be detected. Within this context, Herbert Scoville provides a somewhat optimistic analysis of the safeguards built into the NPT. Victor Gilinsky, in an essay which may be quite useful to the non-expert, illustrates the technology of civilian reactors and the ways in which they might be used to shorten the time necessary to acquire nuclear weapons. And David Brooks and Henry R. Myers discuss the economic utility of the peaceful atom for such programs as the extraction of gas from difficult rock formations and oil from shale, and the feasibility of large earth-moving projects like canals and harbors. While they point out the many intangibles involved in such analyses, one of their more interesting conclusions is that none of the peaceful uses look economically interesting unless the costs of developing fissionable material can be charged off to a pre-existing weapons program.

The last section of the book contains essays by Joseph Coffey and Richard Falk. Coffey discusses some of the ways in which non-nuclear states could be assured of support against a nuclear threat. He thus discusses alliance commitments, guarantees, troop deployments by nuclear powers within a non-nuclear state's borders, and the dissemination of nuclear weapons to non-nuclear states, with a sophisticated awareness of the limits of each option. Falk discusses the renunciation of first use of nuclear weapons, an interesting idea which has been around for some time, but which has always been rejected by the United States because of our presumed need to assure our European allies that we will come to their aid—by using nuclear weapons—in the event of a Soviet attack in Europe. It will probably take a more profound detente, as well as a major reorientation in strategic dogma, before this suggestion receives the attention it deserves.

Perhaps the major deficiency in this collection is its failure to examine the political interactions that might affect the decision to go into nuclear weaponry. Often the arguments are too abstract and too far removed from the political process itself. In a way, the strategic level of analysis may have been taken about as far as it can usefully go; what we now need are detailed analyses of, for example, the ways in which the nuclear issue might interact with the domestic and security concerns of Japan or India. The one essay in this book which responds to this criticism is Lawrence Scheinman's analysis of the political bargaining between the members of Euratom, and between Euratom as an institution and the International Atomic Energy Agency. This is a useful and interesting analysis of the mix between different levels and kinds of politics, and clearly shows how a presumably technical issue concerning safeguards cuts across distinctions between domestic and foreign policy, "high" and "low" politics, and tangible and symbolic interests. Scheinman's essay, in my opinion, is not only the most stimulating in the book but also points in the direction that future analysis of the proliferation problem ought to go.

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The Future of Law in a Multicultural World. By Adda B. Bozeman. (Princeton: Princeton University Press, 1971. Pp. xvii, 229. $6.50, cloth; $2.45, paper.)

To read Adda Bruemmer Bozeman's insightful treatise on the future of Western concepts of international and constitutional law in a world in which non-Western peoples are asserting themselves is to contend with doubts about Western assumptions and values. Somewhere across the oceans on the nether side of the Iron and Bamboo curtains, there is a "free world" being defended by scientists and technicians, generals and management experts. Yet, as Kipling seems to have foreseen, East has remained East throughout the relatively short period of Western imperialism. And southern Africa remains un-French, un-English, and, since independence, un-American. Except in rhetoric, non-Western society, politics, logic, and law rest on traditions other than the rationalism pioneered by the Greeks.

Professor Bozeman's argument that the non-Western world cannot be analyzed by Western methodologies has been underlined by the Pentagon papers. People who knew extremely little about Indochina ignored even intelligence reports to proclaim all sorts of gospel about states of mind of rulers and peoples and about the probabilities and appropriate means of pacification and democratization. They proved that they knew as much about Tonkinese, Cochin-chinese, and Cambodians as they did about American blacks; however unfortunate the reality of the analogy. Dichotomizing the world into democracy and communism (including neutralist coalitions) and America into black and white only conceals complexity. Hence, as Arthur Schlesinger, Jr. (*A Thousand Days,* p. 351) tells us, Kennedy ignored de Gaulle's warning that the states of Southeast Asia were "fictitious" nations and not good terrain for Western politics.

As Professor Bozeman demonstrates, neither the idea of the territorial state, distinguishable from the personalities who govern it, nor that of the nation, based upon a loyalty transcending village, self-sufficient city, tribe, clan, caste, or corporation, are indigenous to non-Western cultures, analyses to the contrary notwithstanding. Nor have Occidental concepts been so deeply emplanted as to oust traditional principles of social organization and social conflict.

Peoples for whom the Western idea of the state is not the basic principle of political organization obviously cannot conceive of a world composed of such entities, let alone their territorial integrity. In the absence of Western concepts of contracts and their obligations, a law of treaties and its normative cornerstone of good faith are unlikely guides for the conduct of foreign relations. When the individual is not conceived as a citizen in the Western sense of being a carrier of rights and duties, it may be doubted that diplomatic appeals cast in the language of human rights have the same meanings as they carry in the language of Western constitutional law, which transformed into the Euro-American phenomenon of international law, has only a limited intercultural validity.

On the basis of the information that Professor Bozeman has collected on the thought patterns of the Islamic Middle East, Africa south of the Sahara, India and Indianized Asia, and China, two possible conclusions emerge. Either law in its Western sense has no future, or the Western authorities did not stay in their colonies long enough to lay foundations for legal thinking similar to those that Rome established in Western Europe. Both conclusions are disturbing. The latter, not one of Professor Bozeman's expressed conclusions, suggests the ironic possibility that Pentagon hawks may be right to view force as the best defense of American values. For Professor Bozeman's demonstration of the merger of Leninism and Sun Tzu's doctrines in the writings of Mao Tse-tung, together with her presentation of Hindu and Arab thought on war as a condition superior to peace and the state as an organization for waging war, offers no hope for the survival of Western ideas through peaceful processes of agreement, adaptation, and socialization.

Professor Bozeman's forecast of the future of law comes close to a view that law, as developed in the West, has no future. For if, as she says, a "taking culture" borrows traits from a "giving culture" and reinterprets them in terms of its own system of thought, then the more that the Orient and Africa borrow from Occidental legal and political thought, the greater the undermining of Western concepts. Moreover, since Western egos maintain the universalism of their concepts by adapting meanings to accommodate non-Western divergencies, what remains to be defended? If one compares the thought of Cicero or Ulpian with that of Cassiodorus in the 6th century, after the barbarian invasions, or considers the impact of China on Buddhism, the probabilities are appalling.

What is extremely challenging about Professor Bozeman's study is that so much of what she describes as non-Western principles seems to characterize American society, which, as she points out, has tried to extend its idealized national model of a polity transcending history and culture to the global organizational level. She writes of loyalties to caste and other subnational groups. Professor Bozeman cites Middle Eastern techniques of cunning, simulation, ruse, and the "secret society" as the organizational model for political activism, and these seem to be techniques not only for a Vietnam War but also for city governments, university administrations, and corporate management. References to the nonterritorial millet of the Ottoman Empire, in which the religious head of the community exercised civil functions, suggest the new, boundary-transcendent, secular millet known as the international corporation. China's use of the *panca-sila* to distort the value-language of Buddhism at India's expense brings to mind the value-professing behavior of politicians in public life, labor unions, consulting agencies, and academia.

One wonders whether an analysis of behavior might reveal more similarities between Orient and Occident than does analysis of philosophies. Professor Bozeman's work will, I hope, stimulate more types of studies of substratal cultural forces than she may have sought. At the least, she has shown us that the ritualistic reiteration of Western liberal principles will not create one world. But she advises much more, namely, the recognition of non-Western systems for what they are, a respect for diversity, and a focus on the cultural and psychological properties of each controversy so that different approaches to the control of conflict might have a chance, given time, to produce yet undiscovered types of meaningful consensus.

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The Dissolution of Power: A Sociology of International Relations and Politics. By Silviu Brucan. (New York: Alfred A. Knopf, 1971. Pp. 388. $10.00.)

In this book Silviu Brucan, the Romanian social scientist and former diplomat, synthesizes an independent Marxist approach with recent empirical, theoretical concepts from Western political science in an attempt to evaluate international relations and politics from a sociological viewpoint. He is concerned with the evolution of societies into nations, nations into supranational groupings, and these into an eventual single-world community. The book has two major analytical objectives: (1) an examination of the contemporary international system, with particular focus on the nature and role of the nation, and (2) an evaluation of the variables conditioning a movement toward supranational integration, which is defined as "the transfer of decision-making power from the nation to a larger entity" (p. 234).

In the Preface and Chapter 1, Brucan presents a cogent overview of his subsequent analysis. From Chapter 2 through Chapter 7 the author, building upon the idea that politics is "the struggle to change or maintain inequalities" (p. 113) argues that "just as social inequality is at the origin of politics in society, so national inequality is at the origin of politics in the world arena" (p. 105). Although he believes the nation is ultimately to be absorbed in supranational groupings, at the present time it remains the primary actor in the international system, which is undergoing a period of national resurgence.

Brucan is very critical of contemporary Marxists who have ignored the importance of the nation and nationalism, particularly as these are found in the Communist world. These individuals fail to comprehend the relationship between the "class-ideological" variables and the "national-strategic" variables. The author sees the current national resurgence as being based on a greater desire for national equality and a rejection of superpower hegemony. (As a Romanian, Brucan knows whereof he speaks.)

In analyzing the nation, the author devotes much attention to developing a theory of foreign policy, because foreign policy analysis "cannot go further without a theoretical framework" (p. 202). Brucan's framework, which owes much to the work of James Rosenau, states that foreign policy is shaped by: natural-material variables (technology, natural environment and population), societal variables (classes and nations, with their ideological consciousness), contingency variables (economic, political or military crises), governmental machinery variables, and leadership variables.

The author, perhaps reflecting Marxist influences, has a bias against the leadership or idiosyncratic factor as ever being of primary importance in shaping foreign policy and asserts that the technological-scientific factor is predominant in its influence, although it works through classes and nations. The influence of personality can never exceed "the sphere of autonomy prescribed by" the political system (p. 226), and the system itself, in its foreign policy, responds more and more to "technological-interdependence pressure" which "in its one-directional sweep and onrush stubbornly drives things toward integration. . . This is an objective trend that will ultimately prove overpowering" (p. 32).

It is disconcerting to find such sweeping determinism in an otherwise penetrating analysis of the linkages between the internal and external orders. It is also surprising because Brucan himself criticizes other Marxists for applying economic determinism to the study of international politics (pp. 69–70). Brucan later backs away somewhat from such deterministic assertions.

From Chapter 8 through 10 there is an examination of the process of supranational integration. Selectively drawing from the work of Deutsch, Etzioni, and Haas, as well as from his own concepts, Brucan argues that integration can take place only among national units which themselves are integrated, are relatively equal in economic development, and have a homogeneity of class structure. In these conditions, the interdependence deriving from the scientific-technological revolution will make the nation-state increasingly anachronistic so that "power and its modern embodiment, the state, will perish as a result of their own development" (pp. 301–302) and supranational groupings will *voluntarily* emerge.

Brucan says that integration "can be achieved only in parallel with the equalization of nations" (p. 362). But in a significant shift, we now find that the technological-interdependence pressure is not as one-directional as previously asserted. The author admits that disparities between nations, deriving from technology, are actually growing and that "equality cannot be reached by letting things take their course" (p. 363). He advocates that the developed nations bring the developing nations into the technological era, that "this is what we actually mean by equalization" (p. 363). It is a strange concept of equalization and a far cry from the author's earlier determinism. The inevitability of integration remains dubious.

Although not without certain conceptual problems, this is an important book, particularly for those interested in the role of the nation. Brucan is one of the few East European Marxists who has familiarized himself with non-Marxist empirical political theory. The result is an honest and often provocative attempt to analyze behavior in the international system.

ROBERT L. FARLOW

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Strategic Power and National Security. By Joseph I. Coffey. (Pittsburgh, Pa.: University of Pittsburgh Press, 1971. Pp. 214. $9.50.)

Through the twists and turns of the nuclear arms race during the 1950s and most of the 1960s, the United States retained a large margin of strategic superiority over the Soviet Union. By the time the Nixon Administration had begun, however, the USSR's missile buildup threatened to challenge the U.S. position, and a great nuclear policy debate was underway. Written in the midst of this environment, Professor Coffey's study attempts to refute those who claim that a continuation of U.S. strategic superiority is essential for the preservation of American security and the security of its allies. The author's informed and interesting investigation addresses a broad range of questions including the requirements for nuclear deterrence, the role of strategic power in U.S.-Soviet relations, and the effect of the strategic balance on crises situations.

Underlying Coffey's analysis is the premise that the U.S. and the Soviet Union each have strategic forces capable of destroying the other and that "both countries have the technical skills, the industrial bases, and the economic resources needed to maintain this capability, despite anything the other can do" (p. 39). Given this condition, the central point of the study can be simply stated: It is imperative that the U.S. maintain effective strategic retaliatory capabilities, but "deterrence does not necessarily depend on the ability to inflict high levels of damage on an opponent nor on ensuring an advantageous outcome to a nuclear exchange" (p. 101). Strategic power is only one among many factors influencing a nation to initiate military actions or to follow an aggressive foreign policy. Citing past confrontations and Soviet as well as Chinese behavior patterns, the author demonstrates that the dynamics of deterrence and crisis management depend upon a host of variables including threat perceptions, interests at stake, conventional military force balances, and domestic political constraints.

For these reasons, Coffey concludes that it is unnecessary as well as futile for the United States to seek numerical strategic advantage, attempt to build large damage-limiting capabilities, or insist on the capacity to inflict greater damage on the Soviet Union than the Soviet Union can inflict upon the United States. Going one step further, the author argues that there is no need for the U.S. to be concerned over the loss of numerical superiority vis-á-vis the Soviet Union or to question whether deterrence will operate against China, even though America cannot defend itself from attacks by small numbers of Chinese nuclear-armed missiles. Both our principal adversaries, Coffey contends, have behaved cautiously in the past and will not be prone to take risks to test the United States as long as we maintain even a minimal retaliatory capacity. Furthermore, changes in the U.S.-Soviet strategic force balance will have little effect on the viability of America's alliance commitments or the credibility of the U.S. nuclear guarantee; these questions, the author claims, are far more influenced by perceptions of our will and overall relations with our allies.

In a chapter dealing with strategic arms control, the study covers an important set of issues starting with the purposes of strategic arms limitations, moving on to the various types of limitations which could be developed, and concluding with what has turned out to be accurate estimates of the probable outcome of the first-phase SALT negotiations. In a line of argumentation strangely inconsistent with advocacy of arms control, however, Coffey proposes that the U.S. revamp its defense policy for NATO by restructuring its forces to "favor concepts for the quick (if limited) employment of nuclear weapons against large-scale assaults, rather than those calling for protracted conventional operations" (p. 117). This recommendation rests upon the understandable desire to reduce the level of U.S. conventional forces and lower defense spending, taking into account the new situation of detente in Europe. Nevertheless, rejection of "flexible response" with conventional forces in favor of a tactical nuclear weapons doctrine could increase the risk of nuclear war and would seem to be incompatible with stable deterrence on the strategic level.

On balance, Coffey's analyses may succeed in persuading most readers that U.S. strategic nuclear forces "do not necessarily have to be larger and more powerful than those of an opponent in order to deter him from starting a war or risking a confrontation" (p. 169). By the time this book was published, however, the Soviet Union had already equaled the United States in nuclear power, and the Nixon Administration had proclaimed sufficiency as the new U.S. strategy. One year later, the Moscow SALT accords formally codified a situation of overall U.S.-Soviet strategic equality based on mutual deterrence. Accordingly, American policy makers no longer face the relatively simple question of whether to reject superiority, but the more difficult issue of how to manage our strategic posture within the framework of SALT and a broad nuclear parity relationship.

Many defense planners and strategic experts share Coffey's view that deterrence involves political as well as military dimensions and agree with the goal of nuclear stability. In the view of these analysts, however, U.S. strategic capabilities must be shaped by criteria which extend beyond minimum deterrence. Indeed, a credible case can be made that a truly "sufficient" U.S. strategic force posture should be designed conservatively, to guard against misperceptions or miscalculations which could lead to a breakdown in deterrence, and should not appear to be markedly inferior to Soviet forces, for that might reduce the political credibility of our deterrent or weaken our bargaining position.

Questions of nuclear strategy are admittedly complex, but the issues can and must be grasped by anyone making a modest effort at education. *Strategic Power and National Security* is a work which can contribute to this educational process. At the same time, it must be recognized that the tools presented in this study can be applied in many ways, that analyses of political-military problems have no simple or single answers, and that each reader should form his own judgments regarding the broad direction and specific decisions which should guide America's strategic nuclear policies in the decade ahead.

JEROME H. KAHAN

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Patterns of International Cooperation in the Caribbean, 1942–1969. By Herbert Corkran, Jr. (Dallas, Texas: Southern Methodist University Press, 1970. Pp. 285. $8.50.)

The recent surprising upset of the conservative party by the return to power of the Popular Democratic Party on the island of Puerto Rico has brought this book back into attention. One hopes it will be studied by the new generation of *Populares* who are now in government so that they may avoid a repetition of the brashness and the resulting mistakes of the 1964–1968 period, and, of course, reverse the anti-Caribbean policy of the pro-statehood administration of 1968–1972.

The often vague patterns of international cooperation in the Caribbean which Herbert Corkran, Jr. studies have sometimes been superimposed on the region by outside powers in pursuit of their own interests and sometimes have been nationalistic and competitive rather than cooperative. Four related agencies, each evolving into its successor, are analyzed: the

Anglo-American Commission, the Caribbean Commission, the Caribbean Organization, and, last and certainly least impressive in conception and performance, the Corporacion de Desarrollo Económico del Caribe (CODECA).

In each case the author describes the organization's structure and purpose and then evaluates its performance within the region and the degree of international cooperation it has helped achieve. Two chapters (8 and 12) deal with the internal political development of the communities in the Caribbean which moved during this period from the status of colonies with no political rights or power to either full independence or autonomous states. The text of the book is enhanced by an appendix reproducing for the specialist in international affairs eight of the international agreements which established the Caribbean agencies.

The author offers a straightforward account, taken from official reports, of the creation and activities of these organizations. Although Corkran interviewed several of the leading participants in the key posts, for the most part their observations are cautious and noncommittal. Such diplomatic reserve contrasts markedly with the flamboyant projections launched on paper by the director of the last agency under study, projections which, Corkran rightly observes, were never carried out. Except for a few comments concerning the separation of Dr. Eric Williams from the Caribbean Commission, the author does not give us much insight into the behind-the-scenes planning for the creation of these admittedly unique experiments in international cooperation. But, even the formal story is important, and another writer may one day delve into private and personal documents for the more complete history.

Corkran's facts and insight are weakest in the two chapters on political developments in the Caribbean communities. For example, he has erred in his several references to the Jones Act as the legislation which granted to Puerto Rico the right to elect its own governor. This was the Crawford-Butler Act of 1947 and not the "Jones Act of 1947" (p. 68). Concerning the political climate in the French West Indies the author has misjudged the sentiment for autonomy. At this writing in the post-de Gaulle period and with the Messmer plan for slight concessions to the autonomist point of view, it is easy to see this trend toward an acceptance of autonomy, but there were some of us who saw this coming seven or eight years ago. These are minor points, however, and do not detract from the importance of the book, except that one should keep in mind that the author too often was willing to accept at face value the information he was given. Particularly in the field of international affairs and foreign diplomacy, this could be a fatal weakness.

In November, 1972, upon returning from an international conference in Jamaica which had been convened to discuss the problems of pollution in the Caribbean Sea, I found myself in the company of Clovis Beauregard who has been one of the key leaders in the Caribbean Commission and the only secretary general of the Caribbean Organization. As we both returned to our islands, he to Martinique and I to Puerto Rico, our conversation related to the current need for some regional international organization to curb the destruction of the Caribbean Sea and its resources to the demise of the Caribbean Organization. I had been strongly critical of the Organization and impatient with its lack of results and Beauregard had suffered under the constant surveillance of the French foreign office and had been subjected to the criticism of West Indian and Puerto Rican leaders. In retrospect we came to agree that all of us should have exercised even more patience with that highly defective and weak international agency because as events are beginning to show, the problems which brought about the termination of the Caribbean Organization have diminished, and such an agency today could provide the basic machinery to deal more effectively with current problems such as sea rights, pollution, and the devastating effect of tourism. Thus many lessons can be learned from the reading of Herbert Corkran's study of the attempts at international cooperation in the Caribbean; and it is hoped that the next generation of Caribbean leaders will ponder the past and thus plan more adequately for the future, so that international cooperation will become more effective in the Caribbean.

THOMAS MATHEWS

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Crazy States: A Counterconventional Strategic Problem. By Yehezkel Dror. (Lexington, Mass.: Heath Lexington Books, 1971. Pp. xvi, 118. $7.50.)

Professor Dror tells us in his preface that, as an Israeli, he is particularly sensitive to the emergence of "multi-actors" who engage in unconventional, "seemingly irrational" international behavior. He calls upon us to share his concern, and his volume here is a brief "think piece" delineating the problem for us. International craziness, as Dror defines it, is an attribute composed of five analytically separable elements: unreasonableness in goal aspirations, propensity for high risk-taking, acceptance of unconventional and often ritualistic political styles, and possibly noninstrumental attitudes toward means-end relationships. The possible permutations of craziness that could be formed by mixing varying magnitudes of these elements is infinite, so Dror instead sifts out plausible prototypical crazy states for consideration, ranging from "somewhat crazy" multi-actors (e.g., a revanchist Mexico, embarked on an accelerated nuclear program) to "crazy noninstrumental martyr" multi-actors (e.g., Palestinian extremist groups). It is not argued that craziness is a new problem in international politics. European and Asian varieties were encountered in World War II. But in Dror's view, craziness is a matter of increasing gravity for two reasons—because one of the conditions which appears to promote it, rapid social transformation, is so pervasive in the contemporary world, and because the spread of weapons and their technology, especially nuclear technology, raises to alarming levels the potential damage that crazy multi-actors may bring about.

What are we to do about craziness? Obviously if we are to undertake preventive measures, we must learn more about the causes of craziness, and presently we know little enough about them. Yet even with such knowledge, Dror cautions, the problem may prove intractable, since by definition crazy multi-actors will behave bizarrely and will present us with the formidable task of trying to anticipate and prepare for the unexpected. Dror suggests, however, that there are "preferable countercraziness strategies" open to us. He provides a quick, though somewhat general, listing of some sixteen "substrategies," ranging from the attenuation of conditions producing craziness (if only we knew what they were) and the formation of alliances to counterbalance crazy multi-actors (being careful of course not to arm those who may themselves become crazy later), up to, in the extreme, capitulation ("after all other substrategies fail, and if it does not involve giving up values which one holds more dearly than survival itself" [p. 87]).

Having proffered his list, Dror nonetheless tells us he holds little hope that these strategies will be adopted, for craziness demands unconventional countermeasures, and normal nations —out of policy conservatism, preference for low risk-taking, and domestic political as well as moral inhibitions—are disinclined to adopt the unconventional. The point can be put more strongly. Crazy multi-actors, and especially noninstrumental ones, cannot be dissuaded by methods of defense and deterrence designed for "normal" adversaries, so they will have to be dealt with by extraordinary means, including *major interference in and manipulation of* their internal affairs, exemplary attacks against crazy populations, or political assassination of crazy leaders. And normal nations just don't have the stomach for such methods. Moreover, Dror presumes "a *Pax Americana-Sovietica* as a main condition of preferable countercraziness strategies" (p. 90), and this condominium would be dependent upon an identity of interests and perceptions that neither has arisen yet nor is likely to be tolerated by other nations if it should arise.

As the frequency of craziness and its consequent damage gain in magnitude, policymakers conceivably may learn through hard experience that normal nations will pay for their policy inertia and inhibitions at a very high price (as scenarios of catalytic war caution us). But Dror is pessimistic about even this prospect: "The characteristics of policy-making in nearly all contemporary societies include so many barriers that the probability that preferable countercraziness strategies will be adopted is nearly zero" (p. 93). On that downbeat note, Dror closes his essay admonishing us again that "crazy states constitute a subject deserving close attention. . . . They should be recognized as a potential main danger to humanity and to each and every international multi-actor" (p. 101).

Dror is rightly pessimistic about the chances of his "preferable countercraziness strategies" being adopted, though perhaps not for the reasons he appears to regard as most important. A full twenty per cent of *Crazy States* is devoted to cataloguing "fallacies" in "most contemporary strategic studies" (the reader hungers for more specific citations)—fallacies which Dror suggests have produced a conceptual blindness toward the problem of international craziness. However, it is not our inability to see the problem which inhibits policy changes of the sort recommended, for as Dror's own bibliography attests, the best analysts in the field have recognized the problem of crazy behavior and taken it seriously for some time. Nor is it simple policy inertia or moral disinclination which impedes adoption of Dror's advice, for nations have a demonstrated aptitude for reformulating policies and normative constraints in response to forces which seriously disrupt the international system.

Instead, nations are unlikely to take up Dror's "preferable strategies" because it has not really been demonstrated that the effects of international craziness are consequential enough to warrant overturning the fundamental bases of current international politics. Dror acknowledges that acceptance of his strategies would entail major revisions in prevailing notions of sovereignty and the equality of nations. Almost parenthetically he too observes that "preferable countercraziness strategies may interfere with other needs and goals of the multi-actors. Multi-actors may therefore prefer to take the risks of a crazy state emerging rather than give up what they regard as their overall interests" (p. 95). Moreover, one might even argue that in the one contemporary instance where tactics resembling Dror's strategies have been taken up, i.e., by Israel, it is far from clear that they have been productive of greater security. The possibility that his countercraziness strategies might aggravate rather than attenuate craziness is a paradox not thoroughly considered by Dror, nor is the corrosive effect that adoption of his strategies might have on the relationships among "normal" states.

One must grant to Dror that there are circumstances in which the consequences of craziness could be great indeed. Nations might see their interests differently if a major power became crazy, or if nuclear weapons should in fact become easily accessible to minor actors. Still, it is hard to see how present strategic planning could go much further that it already does toward meeting the first eventuality without becoming more provocative in the process. The second eventuality is a sobering prospect, and Professor Dror's "preferable countercrazinessness strategies" call our attention to the kind of world we may come to live in if we do not take steps now to avoid it.

DONALD L. HAFNER

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The United States and China, 3rd ed. By John King Fairbank. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 500. $9.95.)

This is the third edition, completely revised and enlarged, of a famous book, which since the first edition appeared twenty-five years ago, has stood out like a beacon in the murky field of contemporary Chinese studies. Today the field is clearer; and so Professor Fairbank's guidance is all the more illuminating. Ranging over the whole compass of Chinese studies—historical, geographic, social and political—this book concentrates in the second half on the revolutionary period. It concludes with an appreciation of the Chinese scene in 1970, when "Peking, at odds with the U.S.S.R., was willing to talk to Washington" (p. 416). The far-reaching consequences of this change of line, the visit of President Nixon and the subsequent detente and virtually mutual diplomatic recognition, had not yet become the determinate facts in the current scene. The book is full of the clear insights of its distinguished author. The reasons that capitalism did not develop in a society which was technologically advanced in the pre-modern sense (pp. 36–41), have rarely been explained with such convincing clarity, and so neatly linked with the "non-development of science" (p. 66). The difference between Chinese humanism and the classical Western tradition is examined (pp. 118–120); and on a very different subject, the significance and differences between the Ming maritime expeditions of the early 15th century and those of the Portuguese in the latter part of the same century show that the Chinese, although able to carry out very long sea voyages with large numbers of ships, "lacked any incentive to reach Europe round Africa . . . the Chinese simply lacked the expansive urge which the Europeans had . . ." (p. 130). The history of 15th-century Asia would have conformed closely to the fears of contemporary domino theorists if the Chinese had had that urge.

In the second half of the book, the problems of the modern age prevail. The analysis of American policy toward China, from the "inherited China policy" (p. 283 ff.) to "the fall of China in America" (p. 315) is dispassionate, profound, and (for a non-American reviewer, at least) most illuminating: "Behind our lack of a conspiratorial apparatus lay the more general lack of any philosophy or method for forced-draft economic development and political collectivism" (p. 311). Professor Fairbank very justly feels that American inability to provide such a model is not something which the American people should regret.

"To the outside world [the Chinese Revolution] has been the least known event of modern times," says Fairbank (p. 324). Very true: not only the least known, but an event about which very few people wished to become informed. They will have less excuse since the publication of this book; for in the last four chapters Professor Fairbank has distilled the detailed research of many scholars, from all countries, into a clear and logical story of how the revolution developed, the motives and forces which moved it, and the "Perspectives on China and Ourselves" (p. 401) which can be deduced from this analysis. Written before the fall of Lin Piao—an event which no one seems to have expected—the story ends with the Ninth Party

Congress of April 1969, which established the Army and its personnel in a very strong political predominance. Some things have changed since then; and perhaps the most conspicuous and interesting has been the rehabilitation of some of the leading figures cast down during the Cultural Revolution; Teng Hsiao-p'ing, the erstwhile Secretary of the Communist Party, and Liao Cheng-chih, former Head of the Overseas Chinese Commission. Many lesser figures, clinging to the hairs of the dragon, have risen from obscurity with these men. In 1970 no one could doubt that the judgment that "In effect the Cultural Revolution transferred power to the military leaders" (p. 399) was valid. But once again one cannot be sure where power has lodged: is it with Chou En-lai and the civil service, or with a reviving Communist Party; Mao Tse-tung still lives in good health, but does he still inspire the latest moves, or is he less active? We do not know.

In the last chapter Professor Fairbank assesses the relations which now subsist and may be expected to develop between the new China and the West, taking into account many inherited fears and prejudices on both sides. "The first step toward a realistic perspective is surely to recognize that it was the West that invaded China, not China the West . . ." (p. 402). It is indeed the first step, but one which few Western political leaders have hitherto seemed capable of taking. Repudiating his own judgment of 1948, undoubtedly widely accepted then, that China would be reorganized "under the dominant influence of either the U.S.S.R. or the U.S.A." (p. 411), he now sees China as having gone her own way, put into her own orbit by the mass of her momentum. Like others who have studied Chinese history, Professor Fairbank does not see China, once more strong, following the path of expansion and aggression which those other reviving powers, Germany and Japan, so fatally pursued in the past. "The Chinese tradition of self-sufficiency and the virtue of self-reliance have made a comeback under the Communists" (p. 416), and Maoist thinking is still in this tradition. The internal problems, which are outlined and examined in these chapters, are great; there is no other government in the world, nor has there ever been one, which had the responsibility for governing 800 million people. This problem will not reach a stable and unchanging solution in our time, or perhaps for many decades to come; it is quite enough for the Chinese to cope with without planning chimerical schemes to add millions more, most of whom, Indonesians or Indians, Malays or others, would be alien to the Chinese mode of thought and way of life. "I conclude," says Fairbank (p. 418) "that America can expect from the Chinese roughly the degree and kind of response that we provoke. We, not they, have been the most expansive power of recent times."

C. P. FitzGerald

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The International Law of Civil War. Edited by Richard A. Falk. (Baltimore, Md.: The Johns Hopkins University Press, 1971. Pp. xix, 452. $15.00.)

"The conclusion that 'policy, not law, determines the actions of states with regard to intervention in civil wars' may come uncomfortably close to describing the present. (It may be more accurate to say that law plays an undetermined but probably a very subordinate role in the establishment of policy with regard to intervention in civil wars)" (p. 428, citing an article of the late Wolfgang Friedmann). These words, which appear on the last page of the text of this volume in the chapter entitled "Summary and Interpretation," written by Edward Brown Firmage, correctly sum up one of the results of the case studies, written by different contributors, of six civil wars, ranging from the American Civil War in the nineteenth century to the fighting in Vietnam. These studies form the bulk of this volume published under the auspices of the American Society of International Law. They are preceded by a penetrating analysis of the whole problem in the "Introduction," written by the editor, Professor Richard A. Falk, whose views on the part played by legal considerations in determining national policies with respect to intervention in the affairs of other states, including civil wars, are somewhat, but not much, more optimistic than those of Firmage.

The legal aspects of the policies of states with respect to civil wars in other states are not, however, the sole focus of the case studies. The case studies also deal with the extent to which the laws of war, particularly those of humanitarian nature, are applied or are applicable in civil wars, and with the role of international organizations.

The first case study, that of the American Civil War by the late Quincy Wright (pp. 30–109), is probably the best short account and analysis of the international law problems involved in that classic case which raised most of the issues (except that of the role of international organizations) that reappeared in subsequent civil conflicts. This war was notable for the creation or strengthening of many norms and precedents frequently, though not always successfully, invoked on later occasions.

The classic pattern of recognition of belligerency of the two parties in conflict and nonintervention by other states was not followed in the other cases examined in the volume. "Nonintervention," though solemnly agreed upon by the major European powers, proved to be a mockery during the Spanish Civil War of 1936–1939, in which, furthermore, belligerency was not recognized despite the magnitude and duration of the conflict. As Ann Van W. Thomas and A. J. Thomas, Jr., point out in their case study (pp. 111–178), massive aid to the Franco insurgents extended by Italy and Germany despite the nonintervention pact decided the outcome of the struggle. International institutions failed to play an effective role. Nor were the laws of war much respected, although to a minor extent they appear to have alleviated the fate of some individuals and protected some art treasures. In this conflict, unlike others covered in the volume, diplomatic asylum, usually considered a peculiarly Latin American institution, was widely practiced. This generally excellent study is somewhat marred by the authors' rather doctrinaire insistence that "the established government is entitled to partial treatment from third powers" (p. 143 cf. p. 145) and that failure to recognize belligerency was a violation of international law (p. 176). On both points, the law remains controversial, as indicated elsewhere in the volume.

The late Arnold Fraleigh's study of the Algerian struggle for independence (pp. 179–243) shows once again that decisions to intervene or not were made by states on political rather than legal grounds, although the governments which gave various kinds of aid to the insurgents, including recognition before the conflict was over, appealed in justification to the principles of self-determination and the right of self-defense against colonial oppression. Their position encountered support in the United Nations and was practically vindicated by the insurgents' ultimate success. This conflict, consequently, served significantly to promote the widespread acceptance of these principles, though still controversial, as positive legal norms. As Fraleigh rightly points out, however, there is "little political support for noncolonial wars of political separation," such as those of Katangans and Biafrans (p. 184). This observation confirms the selective nature of applicability of the right of self-determination in contemporary international law. France, on the other hand, resorted to the questionable practice of seizing or diverting on the high seas foreign merchant vessels suspected of carrying supplies to the insurgents, although it refused to recognize insurgents as belligerents. In a generally adequate description of this practice, Fraleigh fails to mention the special security zone declared by France in the waters off Algeria, and the interference by the French in February 1961 with a Soviet air transport carrying high Soviet officials to Morocco over the high seas of the Mediterranean. The humanitarian laws of war, though not recognized to be fully applicable, did seem to exert some influence and to prevent or alleviate some unnecessary cruelties.

The postindependence internal war in the Congo, studied by Donald W. McNemar (pp. 244–302), was distinguished by the massive and probably decisive action of U.N. forces almost from the beginning. It is, therefore, not fully comparable with the other civil conflicts analyzed in the volume. As in the other cases, however, the basic decisions of other states with respect to intervention in the Congo and the role played by the United Nations appear to have been primarily determined by political motivations, although the form and extent of the U.N. intervention were partly affected or at least rationalized by legal considerations. Largely because of the U.N. presence, the laws of war, and particularly humanitarian norms, were honored to a greater degree than in most other contemporary civil wars. With respect to the legal limits on intervention by the organized international community in essentially internal conflicts, the Congo precedent left many difficult questions unanswered. The disagreements between the major powers reflected in the Expenses crisis served to hobble rather than encourage further peacekeeping operations.

As indicated in a study by Kathryn Boals (pp. 303–347), international law seems to have been least influential in the internal war which broke out in Yemen in 1962 and in which the United Arab Republic and Saudi Arabia intervened on opposite sides. Although, as elsewhere, prisoners derived some benefits from the presence of International Red Cross personnel and perhaps from the Geneva Conventions of 1949 (pp. 315–316), this war was marked by the use of poison gas in some Egyptian bombing raids against royalist villages.

The most original and intriguing feature of Boals's study is her advocacy of "the principle of modernizing intervention" which would legitimize foreign intervention based on "a reasonable expectation that it would contribute to the modernization of the society in which it took place" (p. 344). But who would define "modernization," and how? In a world divided by opposing ideologies, national interests, and conflicting ideas of what kinds and tempos of "modernization," and what means of accomplishing it, are desirable, this well-meaning suggestion, appealing as it may be at first glance, must be regarded as hopelessly utopian. Indeed, if taken seriously, it would merely provide another rationalization for intervention.

In contrast, Percy E. Corbett, in his study of the Vietnam struggle (pp. 348–404), while cautiously (and perhaps still too optimistically) hopeful that in the long run collective action can replace unilateral intervention (p. 376), warns that no "supranational organization should attempt to determine the issue of the struggle. Its function would be to prevent foreign intervention on either side, to see that the laws of war are observed," and to protect foreign interests (p. 403). He reminds us of "the most essential principle" reflected in the U.N. Charter, i.e., "that no state should be free to use force in its international relations for self-determined purposes or to impose self-defined values" (p. 404). His meticulously careful and objective study, covering the years before as well as after the beginning of American military intervention, shows, however, that international institutions, including the United Nations, played a progressively declining role in Vietnam. Only the humanitarian rules of war and the International Red Cross can claim some credit, however limited, for ameliorating the fate of many individuals and reducing unnecessary suffering and destruction. The International Commission for Supervision and Control set up under the agreement of 1954 also played, for too brief a time, a useful role.

In recapitulation, these case studies confirm the already widespread impression that international law is least effective in influencing the decisions of states to intervene or not in foreign civil wars. Such decisions are always determined by political factors. However, despite the uncertainties of applicability in internal conflicts of many legal norms designed expressly for international wars, international law does play a role, to a limited and varying extent, in alleviating the sufferings of many individuals and occasionally in protecting works of art. The current efforts, initiated largely by the International Red Cross and some traditionally neutral states (especially Sweden), to strengthen and broaden this area of international law, intimately connected with the concern for human rights, may not be, after all, entirely futile.

Although the case studies assembled in this volume do not pretend to make important contributions to the general theory or methodology of political science, they present and analyze valuable samples of interaction between law and politics in a very sensitive area of international relations and thus throw additional light on the functions of law, with all their limitations and weaknesses.

OLIVER J. LISSITZYN

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The Art and Practice of Diplomacy: A Selected and Annotated Guide. By Robert B. Harmon. (Metuchen, N.J.: Scarecrow Press, 1971. Pp. 355. $10.00.)

This is a reference guide, of specialized interest but considerable value to students of contemporary diplomacy. Aside from providing succinct interpolative statements identifying and interrelating bibliographical and documentary content, this volume consists largely of three types of information: approximately 900 bibliographical entries with brief annotations, a glossary of 140 diplomatic terms, and a collection of illustrative documents. Citations refer primarily to English-language books and monographs, although a few journal articles and some foreign language items are included.

There are seven chapters and a fifty-page author-title index. The first four chapters deal with the nature and objects of diplomacy, its historical evolution, twentieth-century diplomatic methods (embracing personal, popular, and conference diplomacy, negotiation, and treaty making) and foreign affairs administration (which incorporates a comprehensive section on domestic agencies for the conduct of foreign relations covering thirty-three countries—principally European and Western Hemisphere, but also including China, India, and Japan).

Chapter 5 presents a glossary of terms, ranging from general international relations concepts to precise diplomatic titles and documentary designations. Chapter 6 lists nearly 200 published sources grouped as abstracts, digests, dictionaries, directories, periodicals, yearbooks, and the like. The final chapter provides a small number of references on diplomatics, a collection of diagrammatic charts, and some fifty basic documents illustrating diplomatic and consular credentials, international conference procedure, and treaty documentation.

Several bibliographical listings are inclusive while others—such as those on diplomatic history, personal and popular diplomacy, and treaty making—are merely illustrative, and the menu of diplomatic memoirs is fragmentary. Especially for research purposes, this volume needs to be supplemented by guides to official sources and the rich reservoir of essay and article literature. Nevertheless, the serious student of diplomacy will find much of value in this handy compilation in directing him not only to contemporary, but also to much of the older, classical literature in the field.

ELMER PLISCHKE

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Troubled Alliance: Turkish-American Problems in Historical Perspective, 1945–1971. By George S. Harris. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1972. Pp. 263. $4.50, paper.)

Much in the fashion of the London presentation of Agatha Christie's "Mousetrap," the Turco-American alliance continues its run well beyond everyone's expectations. But the London play is taut with suspense, with a scenario that unambiguously informs the viewer when the play is over and it is time to go home. Not so the Turco-American alliance. With a script that too often includes stale cold war dialogue, the alliance must continually be rewritten to prevent its metamorphosis into anachronistic meaninglessness.

How this alliance came into being and how it has met the myriads of difficulties awaiting it at every turn, as well as what the alliance is supposed to mean today are the topics examined by Dr. George S. Harris in the book under review. In this regard he has done his job well. He writes with economy. He reasons carefully. When called upon to do so, he marshals his data well and presents a persuasive argument. His style is even and shorn of hyperbolic expression or melodramatic forebodings. He is at times frustratingly prosaic. The alliance "is in transition" (p. 201); it is subject to "pressures for further disengagement" (p. 210); and "it is as much the myth as the reality that frays the bonds between the United States and Turkey" (p. 210). Armed with these conclusions, few will storm the barricades. It is the type of study that stirs the mind, not the heart.

Harris opens his study with a review of the alliance's genesis, noting Turkey's role in "lobbying" the U.S. into the alliance. To be sure the unique confluence of foreign policy goals made it relatively easy to "lobby" the United States into underwriting Turkish military security and, ultimately, her economic development. What is of interest is the subsequent political action on the part of both parties to get involved in the domestic affairs of the other. In the case of Turkey this would involve such tactics as retaining Thomas Dewey at a cost of $100,000 to represent "Turkish interests" in securing economic assistance in the early 1950s. In the case of the United States, our presence in Turkey would be ubiquitous and, at times, highly disruptive. Here the author does a yeoman's job of chronicling the political, economic, and educational effects of the American presence, particularly during the 1950–1960 "Menderes Decade." In a slavish attempt to adapt to American military organization patterns, the Turks even published translations of military manuals calling for regimental *imams,* as counterpart to the American chaplain system, a practice totally at variance with Turkish laicism.

Each of the several instruments that give visibility to the alliance are carefully examined by the author in terms of their historical evolution. NATO, American participation in Baghdad Pact/CENTO committees, U.S. aid agreements, the 1959 Bilateral Treaty, the Status of Forces Agreement, and the 1969 Defense Cooperation Agreement constitute a web of formal arrangements that are supplemented by a complex array of informal agreements. All of this is analyzed, discussed, and then evaluated in the total alliance picture. The author is to be commended for doing this task so well. If he is to be faulted, it is only, perhaps, for paying inadequate attention to the role American politicians, particularly Senators, play in setting the parameters within which these instruments and arrangements have to operate and have to be justified.

After the alliance reached its plateau in 1963 (according to the author), the international context, plus rising anti-Americanism in Turkey, have generated pressures on the alliance that have continually grown more strident, even violent. From the so-called "Johnson letter" of 1964 warning Turkey to keep her hands off Cyprus to contemporary allegations of CIA activities within Turkey, radicalism has been fed political ammunition with disturbing effect. Issues such as the question of dual control over nuclear weapons used in Turkey or the use of Turkish air bases as staging areas for possible U.S. action in the Arab world provide additional fillip to contemporary anti-Americanism. In the light of a growing Turkish detente with Russia one is tempted to ask, Against whom is the alliance directed?

With this question in mind, I would have liked to see more fully explored the effect on the alliance of America's switch from a military policy of "massive deterrence" to one of "flexible response," particularly as the Communist world, itself, moved towards multipolarity. The author only *notes* the American identification of Turkey as an "unsinkable aircraft carrier" and *hints* at the initial value of Turkey as a possible *point d'appui* from which to attack the Soviet Union and *suggests* that weapons technology altered this perception and with it much of the premise upon which the alliance is predicated. As it stands the study is an informative work of scholarship well worth including in courses on international politics or area studies.

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The New Politics of European Integration. Edited by Ghita Ionescu. (New York: St. Martin's Press, 1972. Pp. 278. $10.95.)

The politics of European integration are hardly "new"; they have been in the making for more than twenty years. This book brings together essays and studies previously published in two issues of *Government and Opposition* (July 1967 and October 1971). The materials include historical studies which retain their relevance and several evaluative pieces on regional political processes that are likely to gain in importance as the European Community develops its programs in additional economic areas. Whether this contributes anything substantially "new" to our understanding of the interplay between regional economic policy and process and the evolution of a political community is a more difficult point to which we shall come back.

Ionescu presents us with thirteen separate essays and a rather bland introduction of his own. Introductions to edited volumes can and should serve a number of useful purposes: they should isolate the theme which—supposedly—unifies the volume, present the historical background underlying the volume, summarize institutional information readers should have before plunging into the discussion, give a résumé of important policies or decisions made by the agencies whose work is being discussed, and even offer a critique of some theories which may relate to the subject of the volume. Ionescu does none of these things. Hence the utility of this volume for classroom use is in doubt; nor can it be considered a comprehensive treatment of specialized themes relating to overall European integration since it discusses mainly the European Economic Community.

The thirteen essays cover diverse materials. Altiero Spinelli and Denis de Rougemont offer interesting personal reminiscences about the role of the World War II European Resistance and the postwar European Congresses. Richard Mayne's essay on Jean Monnet alludes to the important political convictions and theories of the "father of Europe" but concerns itself mostly with describing his working style. Etienne Hirsch, not unnaturally, makes a plea for the independence of EEC officials from national influences, while Emile Noël and Henri Etienne demonstrate in a brilliant study of the EEC Permanent Representatives Committee how instructed national delegates, under certain conditions, can make joint policy. Stephen Holt and Michael Wheaton contribute rather superficial and dated descriptions of British attitudes and policies toward the EEC, while Michael Steed questions, carefully and intelligently, the oft-claimed virtues of direct elections for the European Parliament. The Dutch parliamentarian H. Vredeling offers a refreshingly candid account of why major European parties are not interested in such elections or in the formation of all-European party federations. J.-R. Rabier tantalizes us with a potentially excellent public opinion survey on the various dimensions of support for European integration; but the analysis is rudimentary since nothing more than the percentage distribution of replies to the question is given, a situation since rectified with the publication of a much more comprehensive analysis of this data. Stanley Henig describes the evolution of the EEC's Mediterranean policy, which he finds too preoccupied with questions of trade; he advocates the marriage of economic policy with political efforts to arrive at a European solution to Middle Eastern problems, though he manages not to mention such issues as the relations between Brussels and the Greek colonels, French military aid to Libya, and policies that might be made jointly vis-à-vis the Palestinian terrorists. Helen S. Wallace seeks to assess the impact of the EEC on national policy making—a much neglected area of study—but she comes to no very crisp conclusions. Terkel Nielsen, on the other hand, contributes a very informative case study on the role in EEC policy making of the all-European organization of agricultural producers, the most highly developed regional interest group.

There is little that can be considered "new" in any of this. The relatively recent studies of David Coombes, Leon Lindberg and Stuart Scheingold, as well as Pierre Gerbet and Daniel Pepy, all present much more informative and focused analyses of the European Community. One might have hoped that the potential novelty of regional, as contrasted with the more familiar style of national politics, might have been examined, particularly in terms of the themes of "postindustrialism." At least, newness in the sense of assessing the character of supranational policy making now, as contrasted to the hopes of Jean Monnet and Walter Hallstein, could have been expected as a focus for the volume.

Five of the authors do indeed concern themselves with this problem. With respect to parliamentarians and electoral practices, for instance, we can understand better why direct elections to the European Parliament, far from providing the "European electorate" with a more visible method of participation, might result in more confusion and more bickering as long as the parties in the competition do not view their interests in terms of such electoral participation. Even the social-democratic groups prefer more direct and oligarchic means of asserting themselves nationally and regionally; direct elections alone would make little difference. Vredeling, therefore, recommends a confederative scheme for linking the socialist parties more closely. The Wallace and Nielsen essays give us a sharp contrast of the kind of regional politics possible when the policy issue in question is highly visible and salient (agriculture) as opposed to being elusive and "technical" (monetary problems). In the first case general participation is hard to avoid for governments and the Commission; in the second, high civil servants at home and the Council of Ministers in Brussels dominate, more so than in the early years of the EEC. Hence students of new or old supranational processes must come to realize how and why institutionalized and routinized decision making by national delegates can and cannot work in the solutions to regional economic problems. Noël and Etienne give us much of the answer in their careful sorting of issues which are dealt with successfully and unsuccessfully by the Permanent Representatives Committee. They show why it is a serious mistake to interpret the "new" politics of European integration simply as a reassertion of national interests, yoked to each other in perpetual combat in the EEC Council of Ministers.

Ernst B. Haas

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Pacific Estrangement: Japanese and American Expansion, 1897–1911. By Akira Iriye. (Cambridge, Mass.: Harvard University Press, 1972. Pp. ix, 290. $12.00.)

In his most recent contribution to the study of American-East Asian relations, Akira Iriye argues that the roots of Japanese-American estrangement can be found in the increased contacts between the two peoples as they expanded at the turn of the century. Increased awareness of each other brought not friendship or mutual respect but rather a sense of rivalry and inevitable conflict, felt especially by Americans. Japanese and Americans, whether they met in San Francisco, Honolulu, Manila, Mukden, or Shanghai, were unable to conceptualize their relationship except in terms of mutual antagonism.

Deftly, Professor Iriye analyzes the Japanese vision of expansion as the source of the West's wealth and power and indicates the admiration expressed for the American example of peaceful expansion. He describes the way the surge of American imperialism in 1898 undermined the anti-imperialist cause in Japan, depriving it of its most apparent model. At the same time he notes that Japanese expansionism in the 1890s influenced the course of America's empire, most obviously in the decision to annex the Hawaiian Islands.

Iriye leaves no doubt that Japan's preference was for peaceful expansion and that the Western Hemisphere and the islands of the Pacific were targets at least as attractive as the continent of Asia. Expansion was considered necessary for Japan to play a role as a great nation —and the Japanese had won the right to that role by their victory over China. But the Japanese found the West—and especially the United States—unwilling to allow them to demonstrate how progressive a people they had become. Emigrating to the United States where their intellectuals assumed they would bridge the gap between two cultures, between East and West, the Japanese found Americans defining progress and civilization in racial terms, denying the Japanese a civilizing role, insisting that the two cultures were incompatible. Having saved Hawaii and the Philippines from Japan, Americans were not about to concede to the Japanese a place in the melting pot, nor was cultural pluralism ever highly prized by Americans.

Driven back by the United States, unable to colonize the American West Coast, the Japanese government reluctantly focused its outward drive on the Asian continent. There, during the years of Theodore Roosevelt's presidency, the United States acquiesced in Japan's dominance of Korea and penetration of Manchuria. But in Manchuria, despite victory in the war with Russia, Japan did not find a clear field. First, the Chinese themselves resisted, and then, to Japan's dismay, the United States launched a deliberate campaign to block the development of Japanese interests. The Taft administration, driven by its assumptions of American liberal exceptionalism, tried to expand American economic interests in Manchuria, viewing such expansion as the defense of China against Japanese imperialism. Without dwelling long on oft-told stories, Iriye exposes the absurdity of American pretensions and establishes the Japanese sense of having been treated unjustly.

Pacific Estrangement is an important book: brilliantly conceived, like Iriye's work in general, researched in a variety of archives and languages unequalled by any other student of the subject. If there is a flaw, it is in Iriye's acceptance of the idea that Japan had to expand, that great nations must expand. He argues that expansion is a pervasive and basic phenomenon of the human experience—that it will go on. But implicit in his argument is not only acceptance of this "law" of history, but acceptance of some dubious Japanese aspirations. Japanese expectations for colonizing the West Coast of the United States appear to have exceeded the fears of most specialists in the "yellow peril." The idea that Japan would send immigrants to the United States as colonists, as permanent unassimilated sojourners, to provide markets and capital for Japan while being protected by the Japanese government, was absurd. The implication that because they were forced to surrender this dream in America, the Japanese had no alternative but to try it in China, colonizing Manchuria, is surely questionable.

Iriye has demonstrated clearly and persuasively the influence the United States had on the course of Japanese imperialism. He has demonstrated the unreasonableness of the American insistence on an open door in Manchuria while closing the door to Japanese expansion in the United States and its possessions. But for all its sophistication, his treatment does not solve the problem posed at the time by the simplistic Philander Knox, who professed not to know "why the Japanese need Manchuria any more than does China who owns it now."

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Confrontation With Pakistan. By B. M. Kaul. (New York: Barnes and Noble Books, 1972. Pp. 338. $11.50.)

The late Lt. General B. M. Kaul had a unique place in the brief military history and political life of independent India. Of the group of Sandhurst-educated officers who dominated the Indian military establishment until recently, he was the most politically minded. Kaul's sudden fall from power came in the autumn of 1962, when forces under his personal command were routed by the Chinese in one sector of the northern frontier, during the short war of that year. Beyond the charges of military failure, his critics argued that through political maneuvering Kaul achieved both his rank and his 1961–62 position as Chief of the General Staff, then the second most important Army post. He was also accused of being reluctant to jeopardize his career by forcing unpleasant truths about Indian military capabilities on Nehru before the 1962 debacle.

This is Kaul's second book, the first having been a memoir and defense of his actions. Its title is somewhat misleading since only in part is it a discussion of Indian military dealings with Pakistan. Loosely constructed, the book also contains additional material on the Sino-Indian conflict, and is mainly concerned with presenting Kaul's views on the many ills affecting India. Although ostensibly designed to shed light on basic political, economic, and societal influences on India's defense capabilities, the book is really a general statement of Kaul's outlook on his country. As such I consider it valuable, since I feel it provides insights into the political and social orientation that the Indian Army's officer corps is acquiring, as the influence of the British period wanes. This impression is based both on personal experiences, and various published writings on the Army.

Kaul does not favor military rule of any kind, although he does recognize that a breakdown in the civilian political process some day is a distinct possibility, and might well lead to an Army takeover. He expresses some admiration for Mrs. Gandhi, and for her father, but he despairs of Indian civilian politicians generally. Their shortcomings are many, including: lack of training for the positions they hold and the administrative powers they wield, oversensitivity to interest-group demands and enticements, a tendency to seek popular favor and votes through image making rather than deeds, indecisiveness, lethargy, and corruption. But Kaul does not see such traits as confined to politicians. They are national character defects, to be found within a nation generally lacking in initiative, resourcefulness, and self-discipline.

The correctives Kaul advocates are mainly of the character-building type. For example: the schools must endeavor to impart patriotic fervor to students; a national militia of ten million persons should be raised, one which would also serve as a labor corps for public projects, and a source of discipline, motivation, and pride for the entire country; the virtues of the soldier should be widely emulated throughout the society, although Indian regimentation should have a "democratic flavor" (p. 329).

On military subjects, Kaul has some useful things to say, but among his remarks are signs of personal enmities, and this raises questions about his objectivity. Generals Chaudhuri and Manekshaw, members of a loose military faction opposed to Kaul's during his years of power and influence, are targets of occasional invective. Kaul contends that despite the expansion, reorganization, and modernization of the Indian Army since 1962, uninspired generalship remains a serious handicap. His primary method of supporting this point is analyzing India's performance against Pakistan in the 1965 war, under General Chaudhuri's leadership. It does seem likely that Kaul's reasoning partly reflects antagonism toward old foes who later achieved greater success than he. The 1971 India-Pakistan war, during which Manekshaw was the victorious and widely praised commander, apparently contradicts Kaul's arguments. That war had not yet occurred when *Confrontation With Pakistan* went to press.

Two chapters are worth the attention of those interested in the 1962 war with China and the events leading up to it. Here, Kaul responds to allegations made about him in two important recently published books on Sino-Indian relations between 1947 and 1962. These are: *India's China War* (1970) by the former London *Times* correspondent Neville Maxwell, and *The Chinese Betrayal* (1971), by India's former intelligence chief, B. N. Mullik. Although Kaul's remarks are sometimes mere repetitions of arguments made in his earlier memoir, and contain little new information, he does provide a detailed discussion of important issues. Moreover, since Maxwell and Mullik were each influenced by particular biases, and cannot be regarded as authors of authoritative works, it is highly likely that some of Kaul's rebuttals are valid, despite their self-serving nature. Judging from my own research, Kaul's comments on Mullik's role in the overall confrontation with China, and his defense of the Army against Mullik's charges, are particularly deserving of respectful consideration.

STEVEN A. HOFFMANN

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The Year Book of World Affairs, 1972. Edited by George W. Keeton and Georg Schwarzenberger. (New York: Praeger Publishers, 1972. Pp. vi, 380. $19.50.)

This is the twenty-sixth edition of a series begun in 1947 under the general sponsorship of The London Institute of World Affairs. This long record of publication and the continuity afforded by the continuous editorial direction of Professors Keeton and Schwarzenberger make the *Year Book* of considerable interest and utility to professional students of international relations. Since its inception, each volume of the *Year Book* has contained a variety of articles on international legal, organizational, economic, and political affairs. The present edition is no exception.

The 1972 edition contains nineteen articles on topics as diverse as "U Thant and His Critics," "Sino-Soviet Relations," "The World Council of Churches," and "Immunities of Officials Associated with Permanent United Nations Establishments." Each article is about twenty pages in length and in content and format is very close to what one may read in journals such as *Foreign Affairs*. To this reviewer's mind, what the *Year Book* presents is a well-edited journal published once a year, bound in hard covers, and therefore costing considerably more than a quarterly review. The *Year Book* is thus a "nonbook" that is extremely difficult to evaluate in a *book* review. One can, however, offer an evaluation in terms of the *Year Book*'s own stated objectives and thus say something about the premise behind this continuing enterprise.

According to the publishers, the *Year Book* contains "research articles covering important recent topics in international relations. Care is taken to preserve the balance between theoretical and practical approaches and to emphasize political, institutional, economic and other problems" (jacket cover). According to the Managing Editor, C. G. Burnham, "(t)he specific object of an annual publication is to make possible analyses in a wider perspective and on the basis of more mature reflection than may be possible in a quarterly or monthly journal. Thus, it is not the object of this *Year Book* to provide instant information on current issues of world affairs" (p. 3). The *Year Book* thus aims to publish well-reasoned articles on topics in international relations that are recent and important, but not "current issues." The individual articles should cover the major aspects of international relations from the perspectives of theory and practice. Finally, they are to be "research" articles as opposed to what, one does not know, but perhaps as opposed to "journalistic" articles.

When judged by its own standards, the *Year Book* is a qualified success. Most articles do deal with topics of recent interest, such as "The Washington Monetary Agreement

(1971)," and the authors try to avoid a current events or journalistic approach. The range of topics covered is quite good, as well. While seven articles deal with aspects of international law and organization, reflecting perhaps the interests of Professor Schwarzenberger, and three deal with European affairs, the remaining nine articles this time deal with Communist bloc affairs, arms control and deterrence, Africa, international economics, nongovernmental international organizations, and science and technology. One must question, however, how well reasoned the articles are, how well they mix theory and practice, and to what extent they are based upon systematic research.

If the reader of the *Year Book* considers systematic, replicable research techniques and theory construction to be the essence of the study of international affairs, then he or she will find no theory, very little concern with policy outcomes, and no use of contemporary research techniques and methodologies; therefore, this kind of reader will find the articles poorly "reasoned" as well. On the other hand, if the reader follows a "classical" approach to the study of international relations, as advocated by Hedley Bull in his "International Theory: The Case for a Classical Approach" (*World Politics,* April 1966), then he will find the articles well reasoned, insightful, and solidly researched. That is, the articles almost uniformly combine a close reading of recent events and legal materials with "mature reflection" on the meaning of these materials for trends in international affairs.

In brief, the articles in this 1972 edition of the *Year Book* are identical in approach and method to the articles in the first, 1947, edition. This does represent continuity, but one wonders if it represents progress. The nineteen authors, the majority of whom are British, as is Professor Bull, manifest hardly any interest in or awareness of the literatures of the behavioral and social sciences. The article on "U Thant and His Critics" by Alan James discusses the former Secretary General's "personality" as an influence on his performance in office without ever mentioning what personality *traits* were associated with his behavioral patterns. Nor is any reference made in this book to the political psychology literature. The article by Roger Sceats on "The Continuity of French Policy" uses selected references to *The Guardian, The Financial Times,* and *Le Monde* as its main sources. The various articles on international law and organization are replete with references to United Nations documents and international treaties, but they give no evidence of the authors' interest in or awareness of the sources for the study of international law and organization that are discussed by Gould and Barkun (*International Law and the Social Sciences,* Princeton, 1970). In sum, this *Year Book* publishes articles that from one point of view are terribly traditional and indistinguishable from high-level current history and journalism and from another viewpoint are well-written and often cogently argued "classical" essays.

If this is the case, what, then, does the *Year Book* offer to a reader different from what he can get from journals such as *Foreign Affairs* and *International Affairs* (London)? I believe there is nothing in the *Year Book* that cannot be found in a number of readily available and far cheaper journals. Yearbooks do not have to follow the format and concept of this *Year Book.* Neither the continuing *Yearbook of Political Science* nor the now defunct but still useful *International Yearbook of Political Behavior Research* did, and this has been to their credit. What these two yearbooks have tried to do is to survey and update fields of inquiry and to provide outlets for innovative research not open to publication in more conservative journals.

We must remember, however, that the field of international studies is not unified by an all-embracing paradigm. Many specialists in the field still agree with Professor Bull that a classical approach to the subject is most appropriate. Such readers will find *The Year Book of World Affairs* of use and they will want to see their libraries acquire it, even if they do not choose to pay nearly $20.00 a year for it themselves. The volumes are well made, well printed, and well indexed. The complete set of twenty-six volumes would provide a good source of commentary on world politics since the end of World War II. That the entire set probably does not contain a single validated hypothesis about international relations and hence no steps toward theory and no guides for practice in all likelihood would not make any difference to such readers in the first place.

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China and Africa, 1949–1970: The Foreign Policy of the People's Republic of China. By Bruce D. Larkin. (Berkeley: University of California Press, 1971. 268 pp. $8.50.)

For some reason the subject of relations between China and Africa, although it has aroused considerable interest on the part of both China specialists and Africanists, has so far evoked mainly second-rate analysis on both sides, with a few honorable exceptions such as Richard Lowenthal's essay in *Africa and the Communist World* (ed., Zbigniew Brzezinski). The book under review does not quite constitute another exception. It has a tendency toward superficial analysis, carelessness about detail, and wordiness. This does not apply to the long and useful (to the specialist) appendix on China's foreign policy machinery as it functions in relation to Africa, but it is based largely on information provided by Donald W. Klein, the leading academic authority in the United States on Chinese organizational and biographical matters. The bibliography is detailed and useful.

The analysis is badly organized, so that the reader is not led in a logical sequence through the parts of the subject. Following an introductory overview, the chapters deal successively with the early history of Sino-African contacts (through 1957, approximately), the historical details of the Chinese "presence" in Africa since about 1957, Sino-African economic relations, the general rationale of Chinese policy toward Africa (a particularly weak chapter), China's "disappointments and setbacks" in Africa (which the author tends to minimize unduly), the Chinese revolutionary model, China's relations with African states and revolutionary movements, and the outlook for Sino-African relations. One of the drawbacks of this design is unnecessary repetition.

The author handles with reasonable success the relatively straightforward story of China's relations with the African states, including Peking's celebrated commitment to build and finance the Tan-Zam (Tanzania-Zambia) Railway. He understandably has more difficulty with the question of China's support for "national liberation" movements (he does not use the term subversion), especially where these appear to be directed against already independent African governments. He admits that such activities appear to exist—a thorough search would probably develop more evidence on this point than he presents here—but he generally minimizes their importance and unwarrantedly belittles in some cases the view that Chinese setbacks in Africa have often resulted from the resentment of African elites (especially the military) at Chinese meddling.

The most interesting, and probably the most valuable, aspect of the analysis is the author's general thesis about the rationale and prospects for Peking's African policy. Chinese activity, he says, has not been very important to date, nor is Peking likely to "dominate" Africa in the future. Peking's policy he characterizes as essentially one of "preparation" against a long-term future when China may become, more through its inspiration and aid than by direct intervention, a leading influence in Africa. He apparently doubts that this optimal vision of Peking's will materialize, except in one of four hypothetical situations (pp. 203–04): a general war, a collapse of the international system for some other reason, major developmental failures on the part of the African states, or large-scale wars among them (he considers the last to be the most plausible of these).

The author has unquestionably made a contribution to the literature on Sino-African relations. Given the importance and interest of the subject, it could be wished that this were a better contribution in both conception and execution.

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The Strange Neutrality: Soviet-Japanese Relations during the Second World War, 1941–1945. By George Alexander Lensen. (Tallahassee, Fla.: The Diplomatic Press, 1972. Pp. 332. $15.00.)

The fluidity that has recently characterized the politics of the Far East and the obvious pointers toward some kind of rapprochement between the Soviet Union and Japan make an examination of their past relations of some topical interest. Professor Lensen, who commands both Russian and Japanese, claims that the period he deals with, that between the Soviet-Japanese neutrality pact of 1941 and Russia's intervention in the concluding stages of the Pacific War is ripe for new treatment; yet the efforts of the Japanese to stave off Russian intervention and to secure Russian mediation with the Western Powers have been fairly well known in the West, at least since the publication of Professor Robert Butow's *Japan's Decision to Surrender*. While Soviet sources do not reveal much more, Professor Lensen has been able to add some illuminating detail from the Japanese side, principally from two sources, an internal Japanese Foreign Office compilation on wartime relations with the Soviet Union based on reconstructed archives (after the destruction of the main foreign office series) and interviews with participants, whom Professor Lensen has been able to consult but not to quote directly, and the memoirs of Morishima Goro which deal with the scene as observed from the Japanese Embassy in Moscow.

The narrative that emerges does not give much help to the so-called revisionist historians of American policy in the period; there is no indication of any plot to turn the war against the Soviet Union or to prepare for a future conflict of this kind. Still less does it support the claim of someone rather flatteringly described by Professor Lensen as a "distinguished scholar" who "labels the contention that the United States played a decisive role in the defeat of Japan as a bourgeois falsification of history."

Three main points emerge. Neither side from the beginning had any more intention than the other of keeping to its agreement if it could profit by its breach. Matsuoka, the Japanese signatory was arguing for a Japanese attack on the USSR immediately after Germany invaded Russia so as to take advantage of the Soviet preoccupation with its western front. The Russians were much better at keping their own intentions to themselves and managed to keep Japan guessing long after they had made a firm agreement with their Allies to intervene in the war. The Japanese were largely guilty of self-delusion almost to the end. The Moscow Embassy was well aware after the summer of 1942 that the Germans were not going to win the war and that Japan should try to make an agreement with the Russians before they were free to strike at them; on the other hand, relying on the official versions of the Pacific War put out by the Japanese government, they had no idea that the war there had turned against them also.

The book does not read very well. Much space is devoted to minor holding operations: the North Sakhalin concessions, fisheries, economic matters. A good deal more of such matter appears in a string of appendices, together with some more general questions of Great Power policy that could well have appeared in the text. The author has a habit of putting his most controversial reflections into undocumented footnotes. The single map is a poor one in which to follow the campaign it is supposed to illustrate, and no map is given to illustrate the discussion of fisheries or of maritime danger-zones.

Professor Lensen's expertise on Japan does not extend to things British. He refers (p. 195) to "the noted English jurist Lord Maurice Hankey." Sir Maurice Hankey (later Lord Hankey —but never Lord *Maurice* Hankey which would have made him the son of an earl) was not a *jurist* at all, but a former Royal Marine officer who became the most notable British *civil servant* of the inter-war period. Alexander Werth was a Russian who became a (naturalized) British journalist. He would never have called himself "English."

Max Beloff

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Aerial Piracy and International Law. By Edward McWhinney. (Dobbs Ferry, N.Y.: Oceana Publications Inc., 1971. Pp. 213. $9.25.)

This book consists of eight essays on various aspects of aerial piracy (hijacking). Some are based on papers delivered at a conference at the Institute of Air and Space Law at McGill University, Montreal, in October 1970: others were specially commissioned for inclusion in the volume, which has been put together by Professor McWhinney, formerly Director of the Institute, but now Professor of Law and Director of International and Comparative Legal Studies at the University of Indiana, Indianapolis. Also included in the volume are no fewer than fourteen appendices containing the texts of the most important international conventions on the subject (Tokyo, 1963; and The Hague, 1970) as well as a number of draft conventions and resolutions of international organizations, both governmental and nongovernmental.

The first essay, contributed by Professor McWhinney himself, sets the problem in political and sociological perspective. This is followed by papers by Charles F. Butler, United States delegate to I.C.A.O., and Gerald F. Fitzgerald, Senior Legal Officer at I.C.A.O., which trace what might be called the legislative history of attempts to combat the menace of unlawful seizure of, or interference with, aircraft by means of international conventions. After these contributions comes a short but thoughtful piece by Michel Pourcelet, Professor of Law in the University of Montreal, entitled "Hijacking: The Limitations of the International Treaty Approach." John B. Rhinelander, Deputy Legal Adviser at the Department of State, then explains the efforts that have been made to improve the efficacy of the international treaty approach by following up the existing conventions with a Convention on Sanctions. The complications arising from such efforts are evident when it is suggested that they may involve amendments to the basic Chicago Convention on International Civil Aviation. Also a profound study is required of the respective roles of the United Nations and of I.C.A.O. if a policy of sanctions is to be seriously considered.

The question of enforcement, with special reference to Canadian initiatives, is considered by Mr. P. A. Bissonnette and Mr. L. S. Clark, both Legal Advisers at the Department of External Affairs in Ottowa, who are under no illusions as to the difficulties. Professor Haroldo Valladao of the Catholic University of Rio de Janeiro, in the only contribution in the volume which is in the French language, puts his faith in a policy of treating hijacking as "aerial piracy," and dealing with it in the manner that was formerly used by the international community to suppress maritime piracy. He assembles a collection of bilateral and multilateral agreements which give the impression that there is such a thing as an international criminal law, but this concept needs to be probed in depth to see what it really amounts to. Professor Valladao is, however, rightly critical of the articles on piracy contained in the Geneva Convention on the High Seas, 1958.

In the last two essays, Professor Lissitzyn of Columbia University and Professor Green of the University of Alberta come to grips with what is the most intractable aspect of the problem, that is, the reluctance of governments to commit themselves to the extradition, possibly to ruthless regimes, of hijackers who find in the execution of an act that threatens the safety of air navigation the only possible means of escaping from tyranny and seeking asylum. Professor Lissitzyn considers that the decision to extradite a hijacker or not must depend in the last resort on a weighing of values in terms of human rights. He concedes "that the weighing of the values involved is necessarily a subjective decision, as is the weighing of all ultimate values" (p. 122). Professor Green rounds the series of papers off with an excellent summary of the law relating to asylum and concludes with the important observation that "there is in international law as it now exists no basis for extending asylum to aerial hijackers" (p. 145).

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Organizing Mankind: An Analysis of Contemporary International Organization. By Lynn H. Miller. (Boston, Mass.: Holbrook Press, Inc., 1972. Pp. 365. $5.95.)

Scholarly writing about international organizations has mainly been of two types. On the one hand have been detailed case studies of particular institutions or episodes which usually have little or nothing in the way of an explicit analytical framework. On the other hand have been more ambitious, although not always more satisfying, attempts to generalize and build theories of conflict resolution, integration, or some other attribute associated with the phenomenon of international organization.

Lynn Miller's work is general rather than specific in scope, and it utilizes an analytical framework of which the first chapter constitutes an outline. It is not however a theory-building exercise in the grand manner. Rather its author has the more modest objective of reintegrating the study of international organization, which he argues has become so fragmented that the essential unity of the subject is lost sight of. He defines international organizations broadly as "institutional structures based on treaties which bind together several or many nation-states in long-term agreements designed to foster cooperation with each other, in specified ways, and for the promotion of general or particular political and/or social goals" (p. 5). Thus his concept includes not only the United Nations and its specialized agencies but economic blocs, multilateral alliances, and regional organizations, however narrowly defined their functions and however primitive their machinery. Only bilateral arrangements, such as the International Joint Commission, would seem to be excluded by this definition.

Arguing that a common analytical framework will permit comparisons among these widely differing organizations, Miller categorizes them in terms of what he calls their "approaches" (universal or regional) and in terms of their goals (conflict management or welfare). Conflict management includes pacific settlement as well as enforcement action, while "welfare" organizations may have integrative goals or merely cooperative ones. Integration and cooperation, like enforcement and settlement, are viewed as points on a continuum rather than as dichotomous opposites; in fact this is also true of the primordial distinction between goals of conflict management and welfare, as suggested by the ambiguity of United Nations goals in regard to southern Africa.

Three chapters of the book are devoted to conflict management and three to welfare goals. Each chapter analyzes and compares the activities of universal and regional organizations in pursuit of particular goals. A final chapter discusses the prospects for international organization. Reference is made throughout to the apparent paradox that "international organization is the product of national independence, and at the same time the potential vehicle of supranational integration" (p. 8).

To succeed, such an analytical framework must facilitate genuine comparisons and provide new insights. Miller's attempt to integrate regional and universal approaches in a comparative framework is least successful in the chapter on peacekeeping. Beginning with a useful although fairly conventional discussion of United Nations efforts, he then stretches the concept of peacekeeping to include such "regional" actions as the invasion of the Dominican Republic in 1965 and of Czechoslovakia in 1968, thus implying that multilateral participation is enough to turn an illegal and coercive intervention into a "peacekeeping" operation.

The framework is more successful in dealing with "welfare" issues such as economic development, human rights, and protection of the environment, for which more meaningful comparisons between universal and regional approaches seem to be possible. Of particular interest in this connection is Miller's critique of functionalist theory and his argument that the politicization of such allegedly "nonpolitical" issues may in fact facilitate integration rather than preventing it.

Miller's book largely avoids the Washington-centered perspective and cold war rhetoric that still characterizes much of the literature in this field. It is not, however, free of a more subtle bias that is perhaps endemic to students of international organization: the tendency to ascribe all evils to assertions of national sovereignty. This is especially noticeable in the chapter on "Technology and World Society." It is surely misleading to say that "the decentralization of the international system . . . has produced the growing danger to the world's ecological balance" (p. 269). The pursuit of economic growth at any cost would produce this danger whether the system were centralized or not. To take another example, Miller's condemnation of "increasing national encroachment on the community trust, the high seas" (p. 286) reflects the Anglo-American point of view but hardly does justice to the other side of a complex and important issue.

Organizing Mankind is a well-written (despite some rather trite literary references) and interesting introduction to the field of international organization. The addition of an index, inexplicably omitted in this edition, would increase its usefulness. Supplemented by one of the better books of "readings," it would make a stimulating framework text for an undergraduate survey course on international organization.

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Law and the Indo-China War. By John Norton Moore. (Princeton, N.J.: Princeton University Press, 1972. Pp. xxxiii, 794. $22.50.)

This ponderous book is largely a collection of earlier productions, put together without any visible attempt at economy of time or space. The result is a great deal of tiresome repetition, as the reader meets again and again, frequently verbatim, the same hypotheses and conclusions supported by the same arguments and the same footnotes.

This is a pity, for there is much in these 800 pages of very high value indeed. The 171 pages of Chapter IV, for instance, present a veritable anatomy of intervention, followed by a concrete and admirably reasoned proposal for a closer approach to control of what has become a perilous and tragic aberration in the relations of states. Here the author comes out as a vigorous and well-armed champion of normative and structural reinforcement of the United Nations as the essential authorizer and moderator of external intervention in internal conflict. Always his proposals are put forward without dogmatism, and his mechanism of decision and control is one in which law is tempered with justice in an international system characterized by rapid and drastic change. In his elaborate set of distinctions between permissible and nonpermissible intervention, the point which seems most debatable is his tolerance of unilateral counterintervention on the side of a widely recognized government. This surely risks competitive escalation. Except where the original intervention amounts to an armed attack, unilateral counterintervention should be permissible only after an appeal to the United Nations citing the necessity of immediate measures. Action taken would of course be subject to United Nations scrutiny.

Not satisfied with clarifying and strengthening the role of the world community in conflict management, Moore proposes changes in governmental structure designed to increase the measure of deference to international law in national decision making. His plan to promote the Department of State's Legal Adviser to the rank of Under-Secretary of State for International Legal Affairs and to make him an *ex-officio* member of the National Security Council aims not only at better handling of intervention problems but at a larger general use of international law in the determination of foreign policy.

All of this should go some way to confute, in any mind open to reason, the Soviet myth that American juristic writing, particularly as exemplified by the McDougal-Moore side of the debate with Richard Falk (much of which is reproduced here), is essentially special pleading in defense of the massive operations of the United States in Vietnam. The more recent part of Moore's work presented here is conspicuously even-handed. This is an important point. Even G. I. Tunkin, who had earned high respect in the Western world, has in his *Ideologicheskaya Borba i Mezhdunarodnoe Pravo* and again in the 1970 edition of his *Teoriya Mezhdunarodnogo Prava,* rallied to the officially dictated view of American writing as a defense of the "capitalist-imperialist conspiracy." But eventually, the efficacy of any attempt to establish a rational and operative code on intervention must depend upon an unprejudiced dialogue between international lawyers on both sides of the iron curtain. It is anything but certain that this can be achieved. What is certain is that it cannot be achieved unless our literature displays a judicial impartiality.

The final chapters deal, again repetitively, with the constitutional issues involved in the employment of United States armed forces abroad. The text once more is a reproduction of material used on various previous occasions, and the substance of it is clearly worth preserving. A special virtue of Moore's elaborate analysis of these issues is his exposition of the bearing of international law upon them. All of his work deserves a wide audience, which an integrated abridgment would have increased the chance of reaching. As an attack upon urgent problems, a mechanical collection of reprints can be counterproductive.

P. E. CORBETT

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Peace in Parts: Integration and Conflict in Regional Organization. By J. S. Nye. (Boston, Mass.: Litle, Brown and Company, 1971. Pp. 210. $3.95.)

Anything written by J. S. Nye is more than likely to make a contribution to the field (discipline? area?) of international relations, and the study under review is certainly no exception.

Briefly, what Nye attempts to do in his book is to answer the question: "Have international regional organizations created 'islands of peace' in world politics?" In searching for the answer he postulates five hypotheses which relate to the linkages between regional organizations and control of international conflict. These hypotheses, or "regionalist claims," are familiar to students of integration and include such arguments as the creation of new relations among states and between men and states, the development of "special capacity" for controlling conflicts, the improvement of the prospects for economic development, and the restoration of multipolarity.

Having stated his hypotheses Nye proceeds to test them with reference to several cases of conflict involving member-states of organizations such as the European Economic Community, Central American Common Market, East African Community, Organization of American States, Organization of African Unity, and the Arab League. For that purpose he utilizes an analytical framework which he calls the "revised version of the neo-functionalist model of the politics of regional economic integration" (p. 93). The resulting conclusions appear to be inconclusive. Nye rejects three of the five hypotheses while claiming that there is "some modest evidence" in support of the remaining two—improvement in interstate relations and in the capacity to control conflicts.

This brief summary does not do full justice to Nye's arguments which reflect as always the clarity of his thinking, his meticulous research, his impressive erudition, and analytical skill. There are a few points, however, which deserve special attention.

The first one concerns the "revised neo-functionalist" model which may well become Nye's most significant contribution to the study of regional integration. The model, which represents the culmination of a search to develop a "tool for making comparisons" of integration processes in different contexts, is a major refinement of the classic Haas-Schmitter paradigm. Its utility is bound to be appreciated particularly by those interested in integration processes that cannot be easily analyzed and compared within the conventional Europe-oriented framework. To be sure, not all of the "process mechanisms" and "integrative mechanisms" can be usefully applied; nevertheless they do represent a major analytical step forward as compared to the earlier attempts.

The same is true for Nye's gallant try at "dissecting the confusing concept of integration." Nye proceeds to break the concept down into three component parts—economic, social and political—and having developed means of measuring them, he leaves the interaction among them for empirical verification. This is a much more careful and cautious approach than some of the others, which, although methodologically sound, tend to be too formalistic and rigid, leaving little if any room for the analysis of deviant cases.

So much for the positive side of the ledger which is so impressive that one is hard put to find any weaknesses in it. At the risk of being accused of pettiness, however, let me say that the evidence submitted by Nye in support of two of his contentions (microeconomic organizations as conflict preventors and macropolitical organizations as conflict controllers) does not strike me as fully convincing. It seems to me that in the former case (for example with regard to the Saar conflict) it is really impossible to say which of the factors influenced the final outcome: was the conflict between France and Germany settled because of the Schuman Plan, or because of U.S. pressure, or perhaps because of the growing strength of Germany which was rapidly becoming indispensable for the defense of Western Europe? The decisiveness of any of these factors cannot be determined without additional evidence.

In discussing the role of the Organization of American States as a peace-maker, Nye lists ten separate cases of "serious" interstate conflict covering the period 1948–1969. Apart from a single case of the Honduras-El Salvador conflagration, which cost 2,000 lives, the sum total of casualties resulting from the remaining nine conflicts amounted to about 280. With due respect for the importance of each and every confrontation for the belligerents, one cannot help wondering whether the issues and total casualties involved justify the use of the term "interstate conflict" and the ensuing analysis, and whether Nye's assertion that the existence of OAS "does make a difference" carries enough punch to be convincing.

The analysis of the five conflicts handled by the Organization of African Unity which Nye considers much more "important" than those dealt with by OAS is inconclusive and also puts in some doubt the utility and validity of the entire enterprise. My feeling is that the root of the problem may perhaps be found in the definition of the conflict which can be tailored so as to enable one to come up with an impressive numerical sample. Anyone can play the "numbers game" and come up with different conclusions depending among other things on the mix of abstraction and realism. In this particular case Nye seems to choose the broadest possible definition of conflict which permits him to draw the conclusions mentioned earlier. Though formally correct, his analysis lacks realism.

The above comments should not be construed as a criticism but rather as questions which have arisen in the course of reading Nye's study. They do not detract from its considerable value; they simply reflect the complexity of the problems associated with the concept of regional integration. All of us ought to be grateful to Nye for greatly reducing that complexity and for moving the study of international integration a large step forward.

ANDRZEJ KORBONSKI
University of California, Los Angeles

Francophonie, Bibliographie 1960–1969. Prepared under the direction of Paul Painchaud. (Montreal: Les Presses de l'Université du Québec, 1972. Pp. xvii, 137. $4.00.)

Designed as only a provisional reference tool, this compilation deals with interrelationships among French-speaking countries in the 1960s. Because it emphasizes cooperation programs, it also covers ties between Black Africa and France's partners in the European Common Market, but it specifically excludes domestic politics, history and socioeconomic structures.

Its 1602 numbered entries drawn from books, periodicals, and newspapers are organized in two major categories: One, concentrating on the concept of *la francophonie,* is concerned with culture, language and international organizations. The other catalogs relations between states under separate general headings on cultural exchanges, education, cooperation, and social, economic, and political relations.

The deficiencies of this work stem from the inadequacy of its sources. It draws largely on other bibliographies, exploiting directly only nine periodicals, most of which are general in scope. The criterion for selections from relatively specialized magazines and newspapers published in France and Africa seems to have been ready accessibility in Montreal. This impression is reinforced by the sporadic nature of listings from such publications as *Présence africaine, Europe-France-Outremer* and *Marchés tropicaux et méditerranéens,* and by the absence of citations from *le Moniteur africain, Afrique actuelle* or *Nations nouvelles. The Public Affairs Information Service* is the only English-language source consulted.

The content is heavily oriented toward Canada, in large part because the *Index* of the Montreal daily *le Devoir* was a major source. The compilers' lack of familiarity with overseas personal and place names is apparent in such misspellings as Shuman (for Schumann, #599), Nonackkolt and Novachkott (for Nouakchott, #1240 and #1584) and in the repeated substitution of "v" for "w" in the name of Jean de Lipkowski, French Secretary of State for Foreign Affairs (pp. 105 and 114).

The book has an author index as well as an "analytical index," which merely lists geographic place-names, institutions, and associations.

This volume is intended to be the basis for an annual bibliographic *chronique de la francophonie* which is to include studies on major developments in the French-speaking world.

Edward M. Corbett
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Hungary and the Superpowers: The 1956 Revolution and Realpolitik. By János Radványi. (Stanford, Calif.: Hoover Institution Press, 1972. Pp. 197. $5.95.)

The author of this interesting book was in the foreign service of Hungary. In 1962 he became Chargé d'Affairs (ad interim) of the Hungarian Legation in Washington. After five years as a representative of the Hungarian Communist regime he quit. He found himself unable to agree with his country's policies. He was permitted to stay in the United States, completed his doctorate at Stanford University and now is Associate Professor of History at Mississippi State University.

Radványi begins with the Hungarian revolution, describes the events which preceded it and then presents an analytical survey of the after effects. In a sense, this book of less than 200 pages presents a picture of world politics, focusing on its most important protagonists: the United States, the Soviet Union, the People's Republic of China and the United Nations. The year of 1956 is the starting point and an eventful year it was: the Hungarian revolution, the Suez adventure, Mao's Hundred Flowers movement, the Twentieth Soviet Communist Party Congress (denigrating Stalin) and the resulting growth of polycentric communism which stirred the desire of the East European states for sovereignty and of the Communist parties all over the world for more independence from Moscow. The Soviet leaders, however, would not relax their control over Eastern Europe, and the political fallout from the slaughter of Budapest is a good example of the Kremlin's determination to retain control over its satellite states, even though Kádár slowly and carefully improved Hungary's economic situation to some extent during the sixties. Regarding Eastern Europe as their sphere of interest, the Soviets will never permit these countries to follow their national aspirations. Only twelve years after the Hungarian bloodbath, Czechoslovakia furnished another example, and the Brezhnev Doctrine makes it very clear that the Soviet demand for a status quo comprises the "bloc" states.

When Radványi was sent to Washington, his main tasks were to obtain UN recognition for the Hungarian delegation and to remove the annual discussions of the "Hungarian question" from the agenda of the General Assembly. He began by consulting with State Department officials and by introducing himself to Senators Ellender and Pell who in the past had expressed interest in Hungary. The American position hinged on the demand for amnesty for thousands of political prisoners who were incarcerated as "counter-revolutionaries." The Hungarians declared this to be a domestic matter; the Americans replied that it was the USSR which had intervened in Hungarian internal affairs. After several years of negotiations a compromise was reached. Radványi's role in this development was considerable but could not have been successful without Soviet consent: before agreeing to amnesty, Kádár first had to ask Moscow for permission which was reluctantly granted.

The particular value of this book lies in the fact that Radványi was an eyewitness to Communist policy formulation and decision making. He revealed what Western investigators could only guess. We can learn much from this authentic account of events behind the Curtain. In addition, as Radványi became more familiar with American views and policies, he could give us a better idea about Eastern interpretations of these policies. Thus he became knowledgeable of both Eastern and Western approaches. It is fascinating to follow the reasoning of this intelligent man, versed in two worlds, and it becomes implicitly clear why he prefers the West. He never preaches, just presents the facts.

Radványi's book is excellent in form as well as content. It is well written, concise, perfectly organized and extremely readable. It merits far more publicity than it has received so far. It is a good text for doubting Thomases, particularly students who euphorically take the present détente for more than "peaceful coexistence." Radványi's book helps to re-acquaint us with the continued open ended clash of social systems—lest we forget.

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Armed and Alone: The American Security Dilemma. By Dean William Rudoy. (New York: George Braziller Inc., 1972. Pp. 96. $4.95.)

As we try to disentangle from the Indochina conflict and find ourselves between Salt I and Salt II, we should pay particular attention to any book which addresses itself to the "American Security Dilemma." Rudoy sets out to investigate this "dilemma" by compiling the testimony of a series of "witnesses" who testified before the Citizens Hearings on National Security in Washington (1972). The idea is to piece together, in an integrated and readable form, the key notions presented by eminent social scientists, journalists, governmental actors and former administration officials. Unfortunately, the emphasis on style (to permit easy reading of complex testimony) leads to a product which is repetitive, cliché-ridden, devoid of any hard evidence, and connected only by the implicit thesis that our actions in the recent past have been perversions of our "true" national security interests.

The book is organized in four parts. Part I —"Threats to America"—tries to show how we have operationally defined the concepts of national interest and security. Here, the obvious devils are the military and the "military mind." Rudoy argues that we have gradually succumbed to a fear of any and all revolutionary movements, and as a result, we have developed a military mentality which has led us to protect other non-Communist military regimes.

Part 2—"Consequences of the Military Mentality"—examines the cost of perpetuating the role of military protector. These costs include: the creation of the military-industrial complex; the stifling of domestic welfare programs; neglect of our democratic way of life; and the "militarization of international relations" (p. 36). All of these consequences of our foreign policy are a function of militarized civilian advice, and the *capacity* of our military to intervene in nearly all conflicts.

Part 3—"Toward a More Rational World View"—suggests the paths to be taken if we are to shed our security dilemma. Generally, the answer lies in redefining our concepts of national security: "restoring the society's health and hope and building international structures by which people can cooperate in solving their global problems" (p. 10). Operationally, the formula entails the following: drastic cuts in military spending (i.e., the question "How much is enough?" is given the answer "This is too much!"), requiring allies to accept their own defense, redirecting our economy to peaceful tasks, seeking multilateral means to solve international difficulties, and returning to a "balanced" decision-making process.

Finally, the concluding section—"The Imperative Redefining of National Security"—reiterates more of the same by urging that our society's needs must become an integral aspect of our national security concerns, and further, that "we must replace the policies of intervention and over-extension . . . with a policy of internationalism . . . reject[ing] hypocrisy in our dealings with other nations . . ." (p. 81).

The book has two main strengths. First, it provides some interesting perspectives on our foreign policy-making process. For example, the discussion surrounding the growth of presidential powers and consequent suggestions for introducing a more "balanced decision-making process" suggest some interesting normative and prescriptive orientations to Allison's decision-making models (Allison, "Conceptual Models of the Cuban Missile Crisis," APSR, September 1969). On an explanatory level, the author offers an interesting hypothesis for the way in which decision makers contend with the high level of uncertainty in international politics. "To make international relations less complex, America has attempted, through the use of its massive influence, to make as much of the world 'American' as possible" (p. 78).

A second area of strength lies in the abrasive quality of the book. Contrary to the tendency of political scientists to qualify nearly all conclusions, the positions, assertions, etc. of author and contributors are so unqualified, so starkly stated, that the reader becomes emotionally involved with the commentary. While this may not be "appropriate" for science, it makes for exciting debates.

Unfortunately, the weaknesses far outweigh the strengths. Simply put, the book contains little more than conclusions and evaluations. In order to change clichés into serious assertions, one would need to supplement this manuscript with a very substantial series of papers which would marshal evidence for the point of view espoused throughout the book. Appropriately "fleshed out," this volume could have been an important and much-needed addition to the literature on national security policy. With supporting evidence and a clear and explicit statement of the nature of the "security dilemma" (the reader is never quite sure what the dilemma is about: at times it refers to a conflict between maintaining democratic processes and fighting the cold war, at times it refers to a conflict between domestic spending and spending for our military arsenal), it would have provided an excellent source for discussion and debate. Unfortunately, for those who are interested in such matters, this book does not go beyond much of the current rhetoric.

THOMAS J. VOLGY

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Intervention and Negotiation: The United States and the Dominican Revolution. By Jerome Slater. (New York: Harper and Row, 1970. Pp. 254. $7.95.)

Since the era of United States involvement in Vietnam began, a number of political scientists have become increasingly critical of American foreign policy. Seeking to avoid the mistakes of the past, they are vigorously questioning the merits of traditional objectives, the secrecy of the decision-making process, and the haphazard implementation of important policy positions.

While there have been extensive studies of the Vietnam conflict, Professor Jerome Slater of SUNY-Buffalo, with his excellent critique of the 1965 American intervention in the Dominican Republic, examines United States foreign-policy making from a fresh and thought-provoking perspective. His study of the revolt and civil war in the Dominican Republic and the controversial landing of 23,000 Marines to "control" the revolutionary situation serves as the setting for one of the more thorough and searching investigations to date of United States intervention. The book's importance is enhanced by Slater's going beyond the traditional historical analysis of the Dominican crisis with some sharp attacks on liberal critics of the United States' role in the revolution, on the limited participation allowed the Latin American community in the negotiations, and on the advisability of clinging to unrealistic views of Communist expansion and national interest.

Slater's main concern is to present an objective view of the Dominican revolution and the motivation for United States intervention. In the process he takes to task such recognized experts on the intervention as Theodore Draper, who failed to come to grips with the complexity of the political situation and the wide range of objectives that the Johnson Administration sought to achieve by landing troops in the Dominican Republic. Slater shows that, contrary to commonly held beliefs, policy makers were genuinely interested in curtailing the senseless killing, initiating free and open elections, and reforming the Dominican armed forces.

But the author is quick to remind the reader that despite these well-intentioned gestures toward peace, democratic practice, and military reform, the United States continued to formulate its decision to intervene from within the confines of the anti-Communist containment doctrine. From Embassy officials in Santo Domingo to President Johnson, all expressed fear that the revolution in support of ousted President Juan Bosch would be corrupted and taken over by influential Communist elements in the "constitutionalist" camp.

Slater emphasizes that the United States, instead of accurately analyzing the strength of the Communists, or more fundamentally working to re-assert the dominance of the democratic-Left Juan Bosch and his supporters, prematurely branded the entire revolution as "Communist" and called in the Marines. In the revolutionary crisis facing the Dominican Republic in April, 1965, the foreign policy apparatus of the United States was not able to act with any sense of unbiased judgement or foresight. United States policy makers acted on the premise that the internal disorders in the Dominican Republic made the country highly susceptible to Communist takeover. Few officials in the United States Embassy or in Washington made the effort to evaluate the Dominican situation objectively and formulate a policy that reflected the realities of the crisis.

Although the analysis of the motivational framework within which the United States made its intervention decision is the primary aim of the author's introductory chapters, the real strength of Slater's book lies in his description of the diplomatic negotiating process conducted in the postintervention period. Slater describes vividly the tension and animosity that developed between the United States and a number of Latin American countries involved in stopping the fighting and reaching an equitable settlement. Because of United States dominance in the talks and in the so-called "Inter-American Peace Force, most of the democratic countries of Latin America refused to cooperate in peace-keeping operations for fear of presenting themselves as tacit supporters of the intervention. The sad result of United States dominance in the negotiations was that the OAS, under whose auspices the political bargaining was being conducted, lost considerable prestige as an independent peacekeeping force. Once again the Latin Americans were exposed to the political "muscle" of the United States and its overwhelming influence in Hemisphere affairs.

The most crucial segment of Slater's analysis is his examination of the "No Second Cuba" doctrine as it was applied to the Dominican crisis. The author questions whether the so-called Communist takeover in the Dominican Republic would have threatened the United States or led to the development of an additional Soviet foothold in the Caribbean. Slater shows that the United States could have avoided a Dominican intervention had it thoroughly investigated the changing mood of the Soviet Union since the nuclear confrontation of 1962.

Perhaps the most tragic result of the United States intervention, as Slater relates, is that American foreign-policy experts continue to consider the Dominican policing action a "success." Their view is not based on any conception that the intervention righted past injustices or fostered a healthy change in the political climate. "Success" is rather seen as the retention of the status quo with some minor adjustments, and as the ability of the United States to extract itself "honorably" from the Dominican Republic, in contrast to the long-standing Vietnam imbroglio. This view of "success" ignores the hatred and massive social and economic dislocations that the United States caused in the Dominican Republic.

If there is anything lacking in Slater's study of the Dominican intervention, it is a hesitancy to place United States foreign policy in a broader context. The author does not analyze the Dominican action in terms of past United States domination of Dominican politics or as a deeper malaise that many see as characterizing our foreign policy in general. In fact, Slater on occasion seems more interested in extricating United States policy makers from criticism than in examining their decisions as symptomatic of more serious problems in our approach to revolutionary situations. Because of this narrow view, the Dominican intervention is portrayed as an isolated incident that was a reflex action caused by the fear of another Cuba. Yet for all the complexities of the Dominican crisis that establish it as a unique incident, the fact remains that the United States continued to reveal its determination to control Dominican affairs and its profound inability to cope with wide-scale social change in the Third World.

Despite the difficulties that arise from Slater's reluctance to discuss the larger implications of the Dominican intervention, his study of the Dominican Revolution and the United States intervention can only be described as a first rate piece of scholarship. The readibility of Slater's style, the keenness of his analysis and his determination to set the record straight combine to form a welcome addition to the literature on both United States-Latin American relations and the foreign-policy process more generally.

MICHAEL J. KRYZANEK
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Theory and Policy in International Relations. Edited by Raymond Tanter and Richard H. Ullman. (Princeton, N.J.: Princeton University Press, 1972. Pp. 250. $8.50, cloth; $2.95, paper.)

The Center of International Studies at Princeton University has again assembled a group of relatively young scholars to reflect upon some of the problems of international theory in the 'seventies, just as it did at the beginning of the 'sixties (Klaus Knorr and Sidney Verba [eds.], *The International System*, [Princeton: Princeton University Press, 1961]). Like the earlier volume, this symposium also formed the basis of a special issue of *World Politics* and has now been reproduced by Princeton University Press.

The thrust of this symposium, according to the editors, was to make a "very modest effort to bridge the gap between the community of policy-makers and the community of theorists," and I suppose that many from both academic and policy communities would agree that the results were very modest indeed, for most of the essays would seem to provide little guidance for the day-to-day choices confronting foreign policy decision-makers. The essays appear to be more successful in fulfilling the second purpose of presenting some new approaches to international relations theory. Five of the articles with quite distinct approaches to the topic provide the meat of the collection. These are followed by an essay by Oran R. Young which reviews the requirements for theory construction and two critiques of the five earlier studies by Davis B. Bobrow and Allen S. Whiting from their perspectives as former governmental decision makers.

The coeditor of the volume, Raymond Tanter, provides the opening study with his discussion of the CACIS project (Computer Aided Conflict Information System). With this system, which is only partially outlined in the article, the author hopes to develop a model for use in conflict modeling and management. The specific components of the model examined in this article include the effects of past behavior on current policy (organizational processes) and the interaction effect of the behavior of other states (event-interaction processes). Event data from the 1961 Berlin crisis are used to illustrate the effects of these two factors on the patterns of conflict. The results proved to be inconclusive, but this may have been partially due to the improper aggregation of events which were coded by state but aggregated to the alliance level. Since NATO is far from a monolithic actor and even the Warsaw Pact has demonstrated considerable deviancy, individual foreign policies can hardly be viewed as "standard operating procedures" for an entire alliance.

The study by Nazli Choucri and Robert C. North is an intriguing one, but like the Tanter contribution it merely provides a tantalizing sampling to what is a larger ongoing project. The main thrust of the study is to develop a predictive model related to the effects of population, resources, and technology upon the dynamics of international conflict. The empirical referents in this case include the major powers in Europe between 1870 and 1914. The study is placed in a policy context by attempting to isolate the more readily manipulable factors which tend to induce large-scale violence. The authors suggest that the earlier stages of competition are dominated primarily by dynamics internal to the nation such as population, resources, and technology. At later stages, competition, reciprocal comparison, threats, and counterthreats become more salient.

Edward L. Morse is concerned with the politicization of economic issues in his chapter entitled "Crisis Diplomacy. Interdependence, and the Politics of International Economic Relations." Whereas many writers view economic interdependence as having an ameliorating effect on interstate conflict, Morse sees interdependence as heightening crises. The treatment is largely anecdotal with particular attention given to French activities in EEC and the U.S. role in the international monetary crisis.

Just as economic models and theory were important components of the Choucri-North and Morse articles, Norman Frohlich and Joe A. Oppenheimer were influenced by economic exchange theory in their examination of entrepreneurial politics and foreign policy. Of particular concern is the construction of a formal rational model focusing on the entrepreneurial role in overcoming strategic interaction problems and furnishing goods for gain. The model is applied for illustrative purposes to the problems of alliance establishment and maintenance, foreign aid and resource mobilization, and intervention to remove an opposition. The model, which utilizes such indicators as education, health care, taxation, etc., is primarily useful for examining propositions related to changing or influencing behavior internal to another state and is not relevant for analyzing many forms of interstate interaction.

In contrast to the very abstract theorizing by Frohlich and Oppenheimer in which rational calculation is assumed, Graham T. Allison and Morton H. Halperin emphasize the many complexities involved in bureaucratic politics. To the extent that the bureaucratic model represents an accurate view of reality, the impact of international relations research and theory on foreign policy is likely to be minimal. The Allison-Halperin article itself appears to be more useful in providing guidelines for maximizing the power of an individual bureaucrat than in providing guidance for making better decisions.

The three articles reviewing the role of theory and its connection to policy are generally pessimistic about the current utility of international relations theory. Young even suggests that IR theorists should concentrate on deductive theory building without worrying about the issue of relevance in the short run.

Bobrow evaluates the relevance potential of four different types of research products: metatheory, theory, history, and engineering, drawing on the five basic studies. He appears most interested in developing systems engineering models capable of combining the macro-level approach of Choucri and North with the micro-level approach suggested by Allison and Halperin. It is probable that such models, even if perfected, would have minimal use for the day-to-day foreign policy decision maker about whom Allen S. Whiting seems to be more concerned in his evaluation of research relevance. Whiting is critical of the often more abstract modeling involved in game theory and simulation, favoring instead the contributions that might be derived from such research tools as information retrieval systems and content analysis. He suggests the utility of increased interaction between the IR theorist and the policy maker, but the gap is an enormous one as this collection seems to suggest.

One of the major strengths of the volume lies in the fact that it did not limit itself to theorizing about theory as so many symposia of this sort do. Instead, concrete data-based research projects are discussed to illustrate the sorts of findings that may eventually become relevant to the decision maker. As such, it would be a useful volume to introduce graduate students to different approaches to policy-relevant research, and all students of the subject can look forward to a fuller exposition of the various projects.

LLOYD JENSEN

Temple University

The Concept of Aggression in International Law. By Ann Van Wynen Thomas and A. J. Thomas, Jr. (Dallas, Texas: Southern Methodist University Press, 1972). Pp. 92. $6.95.)

This volume traces the history of attempts to define aggression in international law by national actors and implicitly reveals the problems of determining specific norms of behavior in global politics. While the book is a worthy documentation of these efforts and does provide useful material for studying the dilemmas of international norm construction, the authors do not utilize the information they present to develop a framework for understanding the problems of developing legal norms of international political behavior, leaving this task to other scholars.

The first chapter briefly describes the arguments for and against legally defining such behaviors, including disagreements in the literature about which kind of definition might be most useful. Arguments against definition include: (1) Agreement would not be possible; (2) national actors use formal attempts to define aggression as propaganda forums; (3) any agreement would be artificial, bringing violations that would weaken the symbolic power of international law; (4) a definition would advise actors on how *legally* to commit aggression; (5) since technology might provide new means of committing aggression, any legal definition of the behavior would soon become obsolete.

The case *for* defining aggression argues that definitions clearly restrict the behavior of national actors. Despite the ambiguities present in a definition, the possibility of being labeled a wrongdoer would inhibit the commission of acts that *could be* labeled aggression. The authors clearly favor this argument, and it has considerable support in the social science literature. Since the perceived social cost of behavior is a major determinant of the behavior, actors tend not to behave in ways that may be considered illegitimate. While a clear legal definition of aggression may inhibit aggressive acts due to social cost, however, violations will occur if the perceived reward (e.g., perceived national interest involved) is greater than the perceived cost. Such violations might serve to reduce the power and status of legal norms, weakening the structure of international law itself.

The important consideration is under what conditions will national behavior be restricted by legal definitions? To answer this question would require a thorough analysis of the political structure and climate of the global political system and of the political and diplomatic culture operating in international interactions (e.g., Walbek, "The Global Public Political Culture," *Peace Research Reviews,* V, No. 2, Nov. 1973). Unfortunately, the authors do not link their discussion to the political context and hence, do not help us assess the potential restrictive power of a legal definition of aggression.

The second chapter documents the history of conferences and debate attempting to define aggression in international law. They trace the debate from the World War I period through the recent attempts in the UN General Assembly's Special Committee on the Question of Defining Aggression (1956 to the present). This chapter is a useful compilation of the positions and arguments of key national actors and of the changing considerations of what to include in the concept of aggression as the means and technology of international conflict have evolved.

The third and fourth chapters present an analysis of the concept of aggression and summarize the major positions taken by national actors. These positions are grouped into two major categories, those who restrict the legal concept to the direct use of force and those who would expand the concept to include almost all kinds of "illegitimate" acts by national actors (e.g., economic and colonial imperialism, subversion, ideological aggression). Throughout the history of debate on aggression, the main agreement seems to be that armed attacks by one state against the "territorial integrity or political sovereignty" of another state is clearly aggression (p. 45).

In places, these chapters bring out some interesting observations, although no framework is presented to link or order these points. Intervention, for example, is specifically condemned by the UN General Assembly in a 1965 resolution. The authors show that while all acts of aggression can be considered intervention, not all acts of intervention can be viewed as aggression. They discuss how various regional organizations (e.g., the Organization of American States, the Organization of African Unity, etc.) have handled various kinds of political acts and have been less timorous in outlawing acts of subversion and intervention while the UN debates, where developed nations have more influence, have not yet expanded the concept of aggression to include more ambiguous forms of behavior. These two chapters contain the germ of a useful classification scheme that links types of behaviors to types of national actors. Such a scheme would help us understand just how international law is utilized by national actors to promote norms that would further their national interests in the international system while perhaps restricting their behavior at the same time. While each national actor attempts to promote a definition of aggression that would be least restrictive on its foreign policy interests, the continuing debate itself tends to enhance the importance and symbolic power of international legal norms.

The efforts of actors to define aggression are motivated by the fact that aggression, while not defined in the UN Charter, justifies both collective intervention by the UN (Article 39) and the right of self-defense by a member nation (Article 51). Nations clearly have a stake in this debate, and their continued efforts to promote a self-serving concept of aggression indirectly demonstrates their concern for the authority of international law and international organizations. Their efforts to formulate the content of the international political culture, the norms regulating their behavior, is a demonstration of the real power of international law.

This volume provides useful information for scholars interested in aggression as a legal concept. It also provides materials for investigating relationships between the development of legal norms and the political context in which these developments occur. Its utility for dealing with the larger questions of the significance of international law or the understanding of the present and potential impact of law on international political behavior is minimal at best.

NORMAN V. WALBEK

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The United Nations in Perspective. Edited by E. Berkeley Tompkins. (Stanford, Calif.: Hoover Institution Press, 1972. Pp. 155. $7.50.)

This book consists of eight essays that grew out of a conference commemorating the twenty-fifth anniversary of the United Nations, held at the Hoover Institution. While the standard of the contributors is uniformly high, a great deal of the symposium is dated today. It is a great pity that no effort was made to bring the book up to date. Some of the passages for example still predict the early seating of the People's Republic of China.

It is of course true that the changes that ve taken place at the United Nations during the past two years have been dramatic. One appreciates this fact most acutely when reading this book. The bipolar Soviet-American United Nations is over. A multipolar system which is emerging in the world is also reflected in the world body. No one single state has an automatic majority today, and hence no single state can determine the action of either the Security Council or the General Assembly. The old cleavages between capitalist and Communist states are gradually being superseded by new global divisions between the rich and the poor. Old alliances are breaking up and new ones are in the process of formation. A new generation has come to power with its own views of political priorities and its own definitions of political relationships. The postwar world, in short, is over.

The first essay, by Henry Cabot Lodge, presents a competent though unexciting review of the founding and performance of the United Nations. The former Ambassador to the United Nations presents some sensible recommendations for the United Nations' future, but the essay is seriously dated on the China question.

Charles H. Malik's essay on "The United Nations as an Ideological Battleground" rightly pays considerable attention to the rise of the nations of the third world. The essay makes some very interesting and controversial philosophical points. He points out for example that Western representatives are usually "licked before they start" in large gatherings, while Communist delegates, "in keeping with the philosophy of materialism, which recognizes only aggregates, masses and huge effects" (p. 25), do a great deal better in plenary bodies. The point is arguable but arresting.

Peter T. Bauer's contribution on "The United Nations and International Development Assistance" is a workmanlike presentation that correctly diagnoses the widening gap between the rich and the poor. The author advances useful and practical recommendations for increased multilateralization of economic aid.

Perhaps the most thoughtful contribution of the entire book is that by Inis L. Claude, Jr. on "The Peace-keeping Role of the United Nations." Claude speculates about the decline of peacekeeping as a major responsibility of the United Nations. He finds the roots of the problem in the divisive experience of the United Nations Congo Operation and then makes the excellent point that so far as peace keeping is concerned, "we have learned primarily not what to do again but what to avoid doing in the future" (p. 60). He notes, not without melancholy, that the United Nations Operation on Cyprus is the last major such peacekeeping body in the field. One could add to Claude's analysis that a triangular consensus including China, the Soviet Union and the United States is even more difficult to attain in the Security Council than the need to reach Soviet-American agreement of earlier years.

Harlan Cleveland's piece, "Can we Revive the United Nations?" advances eight recommendations, most of which are structural. They include streamlining the General Assembly; making the Security Council work; encouraging the Secretary-General to engage in fact-finding; establishing a real standby force; developing a consortium of the concerned; sponsoring a formal review of the Charter; carefully filling vacancies and assigning major jobs to the United Nations.

Charles Burton Marshall in his essay on Revision of the United Nations Charter wisely does not lose himself in legal niceties or institutional gadgetry, but correctly perceives that the problem is profoundly political. The fact is, for example, that weighted voting devices in the General Assembly can probably not be voted through because most of the small members of the General Assembly like the situation exactly the way it is. The author recounts some amusing anecdotes to support his persuasive analysis.

Carlos P. Romulo's five-page essay on "Crosscurrents in the United Nations" is too brief to be truly useful, but it reiterates the obvious danger that the United Nations may be reduced to marginal importance through desuetude.

The least dated contribution is that of Edvard I. Hambro on "New Directions in Planning for Peace." The author addresses himself to a number of problems that are highly relevant today. He advances again the Finnish recommendation that the Security Council hold periodic meetings, a recommendation that was actually put into effect. He also raises the possibility that the office of the Secretary-General might be given the right to have "a network of United Nations missions all over the world," thus making communication between the United Nations and its member states a two-way street. Finally, he devotes some very thoughtful passages to the establishment of a United Nations University.

Most of the authors either explicitly or implicitly recognize the basic truth that the key to the political resuscitation of the United Nations is the commitment of the membership. Recently member states have either avoided the United Nations on the great issues of power politics in today's world or else they have made it into a receiver in bankruptcy and then blamed it for being bankrupt. The dilemma on which the United Nations is presently impaled is the following: As it becomes more and more universal, no one single state regards the world body as a predictable instrument of national policy, and hence no one single state really pushes it to the forefront. Since most states still have a narrow definition of the national interest, and thus wish to use the United Nations as a multilateral fig-leaf for their unilateral objectives, they now permit the United Nations to languish. Only when perceptions of the national interest are broader and more generous can the United Nations be revived as a truly meaningful political reality.

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The Widening Gap: Development in the 1970's. Edited by Barbara Ward, J. D. Runnalls, and Lenore D'Anjou. (New York: Columbia University Press, 1971. Pp. 372. $10.00, cloth; $3.95, paper.)

This book includes the contents of papers given and the summarized debates that took place on occasion of a conference organized by Columbia University in February 1970 to discuss the implications of recommendations put forward by the Pearson Report (*Partners in Development: Report of the Commission on International Development,* Lester B. Pearson, Chairman (Praeger Publishers, Inc., New York, and Pall Mall Press, Ltd., London, 1969)).

The general outline of the Commission's suggestions determined the sequence of topics analyzed at the meetings and thus the composition of the volume at hand, leaving aside the introductory section in which W. Arthur Lewis describes "The Purposes of the Pearson Report."

In Chapter 1 (Growth and Structural Change) Hollis B. Chenery argues in his paper "Targets for Development" that a 6 per cent rate of growth, as globally recommended by the Pearson Report, needs to be differentiated according to the levels of income, economic structures, and development strategies pursued by different types of countries. In the following contribution, Stanislaw Wellisz comes to the conclusion that the application of efficient tools for solving urgent problems (population increase, lack of education, regional imbalance) may be even more important than the realization of a specific growth target. At the end of this section Gerald K. Helleiner's paper on structural problems in Africa attempts to generate debate on social, political, and institutional aspects of African economic progress.

Interrelated structural issues which threaten to obstruct development and to reinforce the crises in the Third World are dealt with in Chapter 2 (The Development Sectors). There are quotations from conference papers on specific topics: population, unemployment, education, green revolution, urbanization, and environment.

Chapter 3 (Trade and Liquidity) analyzes the scope for rapid growth of the developing countries given by the present structure of the world market and investigates the problem of inadequate foreign reserves on which the Commission had little to say directly. James A. Mirrlees ("The Terms of Trade: Pearson on Trade Policy, Debt, and Liquidity") studies what kinds of trade and monetary policies follow from general economic principles and identifies those proposals of the Pearson Report that should emphatically be supported in the long run. Under the regional point of view, Paul Streeten's and Akbar Noman's article on specified problems of the Indian subcontinent deserves particularly to be mentioned. How large a flow of resources? Which effects of increased aid must we be aware of? With reference to Latin America these questions are discussed by Laurence Whitehead.

Against the Commission's proposals to improve the climate for private investment in order to supplement public efforts it has been argued that foreign private capital can become too powerful and that it can stay too long. Harry G. Johnson ("The Multi-National Corporation as an Agency of Economic Development: Some Exploratory Observations") and Albert O. Hirschman ("How to Divest in Latin America, and Why") thoroughly investigate the involved problems of foreign business activities in Chapter 4 (Private Investment).

The fifth and final chapter (The Aid Relationship) examines the possibilities of establishing new types of economic relations to maintain and evolve a genuine planetary community. The starting point of the debate, emerging from a paper by Richard Jolly, was the prediction that, ". . . even with 6 per cent rates of growth, the gap between developed and developing nations . . . will be three or four times wider by the year 2000 . . ." (p. 280). The question of what sort of independence the less developed countries would achieve in view of this considerable gap constituted the second main issue of investigation.

Because of the obviously very broad scope of these discussions at the Columbia University conference, my critical evaluation can only raise some general points. The editors take the easy way out when they more or less just reproduce, in written form, the scientific events in the sequence of occurrence at the meetings. There was no need to repeat uncritically the unsystematic approach to the problems under observation as predetermined by the Pearson Report. The structure of the book is especially hard to understand in view of the basic statements in the introduction. There Lewis relates the widening gap between per capita incomes in developed and less developed areas to the relatively decreasing transfer of official development assistance by 1967 under the label "crisis in foreign aid" which led to the creation of the Pearson Commission. As is well known, the development of per capita incomes over time is subject to the countervailing and interactive forces of both economic growth *and* population growth. In addition to a systematic investigation of both of these forces, the reader would expect to find a detailed analysis of the impacts of development aid on the determinants of per capita income growth. The book, however, contains only four pages on population problems; it includes no substantial material on the relationship between aid and population growth; its presentation—which is biased in favor of certain selected topics on economic growth—is diffuse at many places so that the reader wonders what now really is important in the field; and its contents are analytically and empirically unsatisfactory for the student experienced in development economics (to a certain extent, Chenery's paper is an exception).

Moreover, the editors do not really launch a comprehensive critique of the Pearson Report, for instance, to serve as a basis for future improvement of aid policies. Such an analytical objective cannot be reached because of two circumstances: (1) Most of the volume is devoted to papers on specific subjects; their often limited critical contents have not been summarized. (2) With reference to the main ideological division of conference participants there is in all a clear bias in documentation of views in favor of those who accept the existing world economy of Western countries with developing areas as normal.

The book also pays little attention to criticisms of the Pearson Report from sources other than the conference participants. Thus, for example, substantial parts of the problem of development goals have again been excluded. The same is true of the neglect of operational problems of development aid activities.

The value of this work lies in its main papers by Chenery, Wellisz, Mirrlees, Streeten, Noman, Johnson, and Hirschman rather than in those sections summarizing discussions. Since a great number of insights and proposals has been gathered, the book may be stimulating for noneconomists and experts in development economics studying specific questions. For additional information, both groups might also look at Gustav Ranis (ed.), *The Gap between Rich and Poor Nations,* Proceedings of a Conference held by the International Economic Association at Bled, Yugoslavia, London: The Macmillan Press Ltd., 1972.

WALTER BUHR
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The European Community in the 1970's. Edited by Steven Joshua Warnecke. (New York: Praeger Publishers, for the European Studies Committee, Graduate Division, City University of New York, 1972. Pp. 228. $15.00.)

In its relations with the European Community, the United States has entered a phase which contrasts sharply with that of the 1960s. This volume, the product of a symposium held at the Graduate Division of the City University of New York in 1971, deals with the internal political and economic structure of the European Community, as well as its emerging relationships with outside powers, including the United States. The editor of the volume, Steven J. Warnecke, points to the discussions among Europeans about the alternative models for building more adequate European Community institutions. Quite appropriately, he concludes that the federalist approach, calling for the rapid creation of strong political institutions, has been superseded by the confederal model, which recognizes the continued strength of the nation state. In a perceptive analysis of the future prospects of the EEC, Uwe Kitzinger contends that the European Community, once a "forward-looking concept," has stopped far short of its original goals both in institutional development and in almost every economic sector except the customs union and agriculture. The entry of Britain, Ireland, and Denmark, however, by vastly enlarging the size of the EEC, will force the European Community to focus its attention on problems such as the development of transport policy as well as a reassessment of agricultural policy.

In an especially fascinating chapter, Heinz Hartmann analyses the evolution of industrial relations in the European Community, including the various forms of employee participation in management and the relationship between economic prosperity and class conflict. Increasingly, he says, the European Community is giving rise to a new working class, characterized by a high level of formal education or specialized training. The multinational corporation in a Europe-wide economy will pose major problems for industrial relations within the member countries and lead to increased transnational contact and standardization of labor organizations and practices. According to Hartmann, the new labor forces may be no less "class conscious" than their predecessors.

Several contributors address themselves to such problems as Community agricultural policy, the nature of existing tariff and nontariff barriers to trade, the Community association agreements with countries in the Third World, and the EEC policies toward the international monetary system as they affect the United States. As William Diebold, Jr., suggests, the enlargement of the Community "widens the area within which American goods are at a disadvantage compared with those of European origin" (p. 101). According to Harold B. Malmgren, the emphasis on internal integration and policy harmonization has led to an almost complete failure by the European Community to recognize the effects of its policies on the rest of the world. Writing as a member of the Commission of the European Community, Ralf Dahrendorf acknowledges that the Community is "an active force in the world, but it fails to speak or speaks only seldom, and then in an enigmatic language of little elegance and often obscure meaning" (p. 115). He concedes that the Community, as the largest world trading bloc, bears great responsibility for ensuring the expansion and liberalization of world trade.

In an analysis of the problems of European and trans-Atlantic technological collaboration, Christopher Wright assesses the lessons to be derived from the collapse of Rolls Royce after its abortive effort to provide engines of advanced design for the Lockheed Airbus. This experience, together with the joint effort of British and French aircraft companies to develop the Concorde, points up the need for "vertically integrated policy planning and management" (p. 161). Wright is correct in his contention that "new independent centers of initiative with rather broad mandates" are needed if Europe is to reap greater benefit from technological collaboration, especially in such advanced sectors as aircraft, in the 1970s. At this point in time, however, it is not certain that Europe will be able to develop such multinational collaboration.

Although several contributors allude to the need for foreign policy harmonization and assess recent efforts such as the Davignon Committee, Louis G. M. Jaquet addresses himself to both the substance of a European foreign policy and the institutional measures for its creation. His proposals, while useful, are by no means novel. They include the creation of a permanent secretariat to assist in the development of agreements among the foreign affairs ministers of Community members. Unfortunately, the problems of foreign policy coordination are likely to be increased by the enlargement of the European Community with the admission of Britain, Denmark and Ireland. Moreover, as Gordon Adams suggests, there is no certainty that external pressures will provide the catalyst to the development of a European foreign policy.

As Andrew Pierre concludes, in the absence of major strides toward political unification, Europe will continue to speak with many voices over the next decade. A politically unified Europe, faced with the need to evolve common foreign policies through a cumbersome process of consultation and trade-off comparable to that of the EEC, is no less likely than the EEC itself to become a difficult partner for the United States, even though European unification may accord with overall U.S. interests.

Thus, this volume provides a sober and realistic appraisal of the range of issues facing the United States and Western Europe in the years ahead. The book is marred only by unfolding of events since its completion, including the anti-EEC referendum in Norway in 1972. Another minor deficiency lies in the numerous misspellings and typographical errors in the text. (See, for example, pages 6, 16, 19, 21, 51, 149, 150, 214 and 216.) Nevertheless, the volume provides a useful survey of the recent development and future prospects of the European Community and their implications for the United States in the changing international environment of the 1970s.

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Law and Politics in Outer Space: A Bibliography. By Irvin L. White, Clifton E. Wilson, and John A. Vosburgh. (Tucson: University of Arizona Press, 1972. Pp. 176. $6.95.)

This work is an excellent research guide for both graduate students and professional political scientists. It reveals the complexity of space policy by listing such diverse subjects as appropriation of celestial bodies, communication satellite systems, legal controls and regulation, national security, and sovereignty. One of the strongest advantages of this bibliography is that both English and foreign language entries are included. It also lists a wide variety of sources: books and monographs, bibliographies, theses, yearbooks, articles, United Nations materials, and United States Government documents. There are two major weaknesses in *Law and Politics in Outer Space: A Bibliography:* (1) the entries are not annotated, and (2) the listings for national security, military uses, and political aspects are quite limited in number. It would seem that these topics would be of principal concern to most political scientists considering research in the general field of space policy.

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APSA

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No. 1 Winter, 1974

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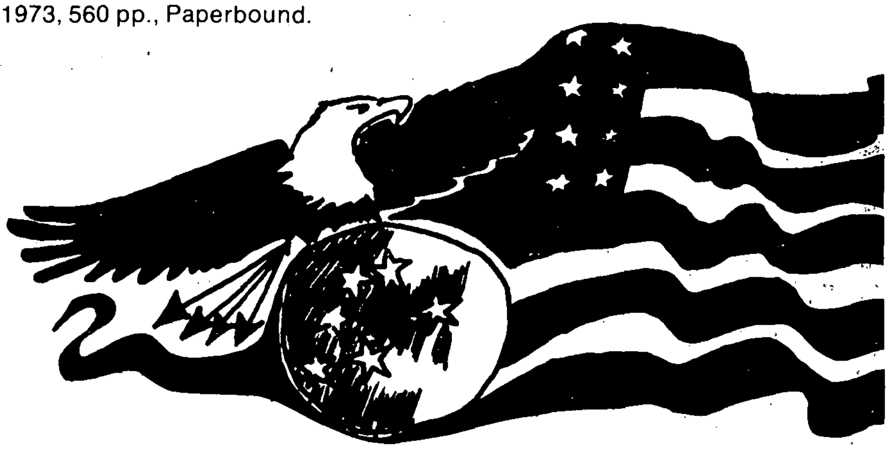

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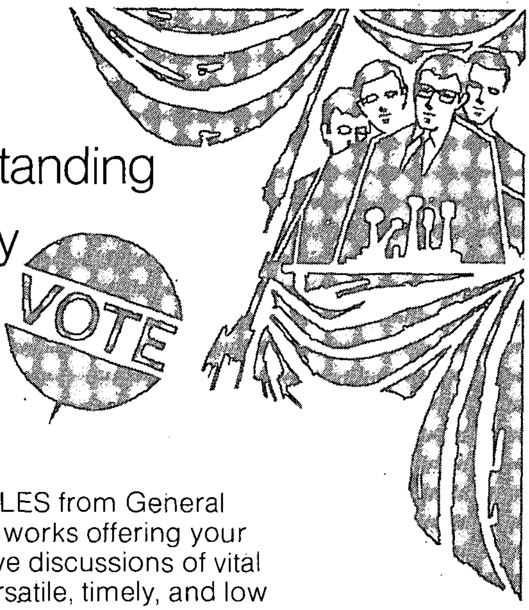

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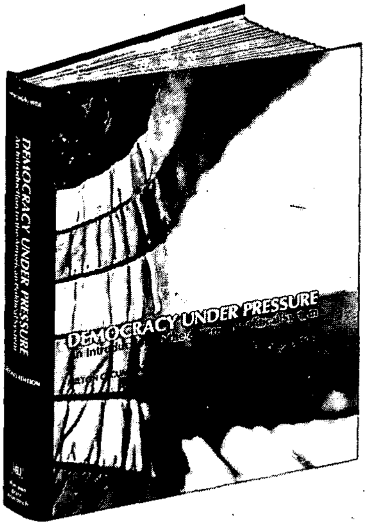

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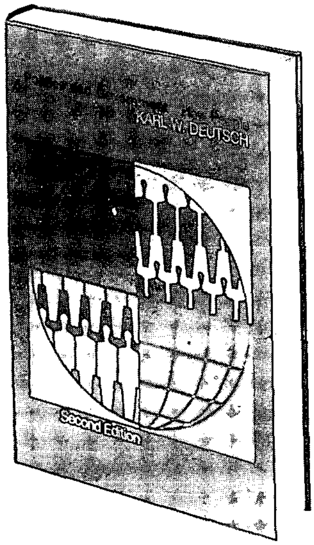

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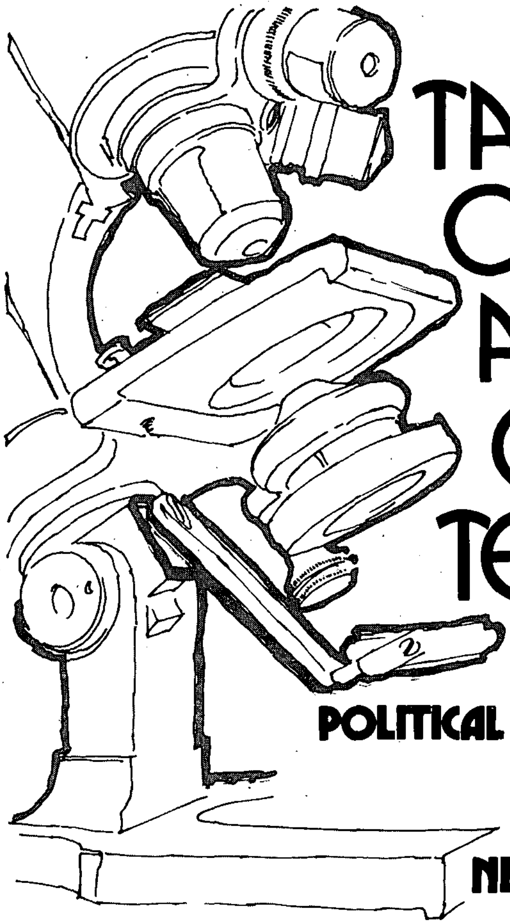

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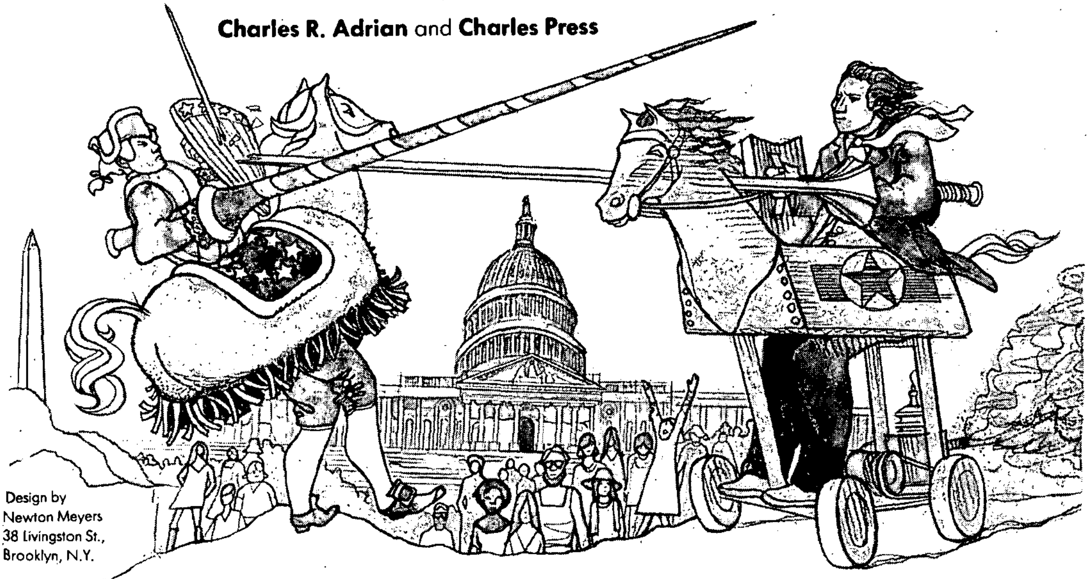

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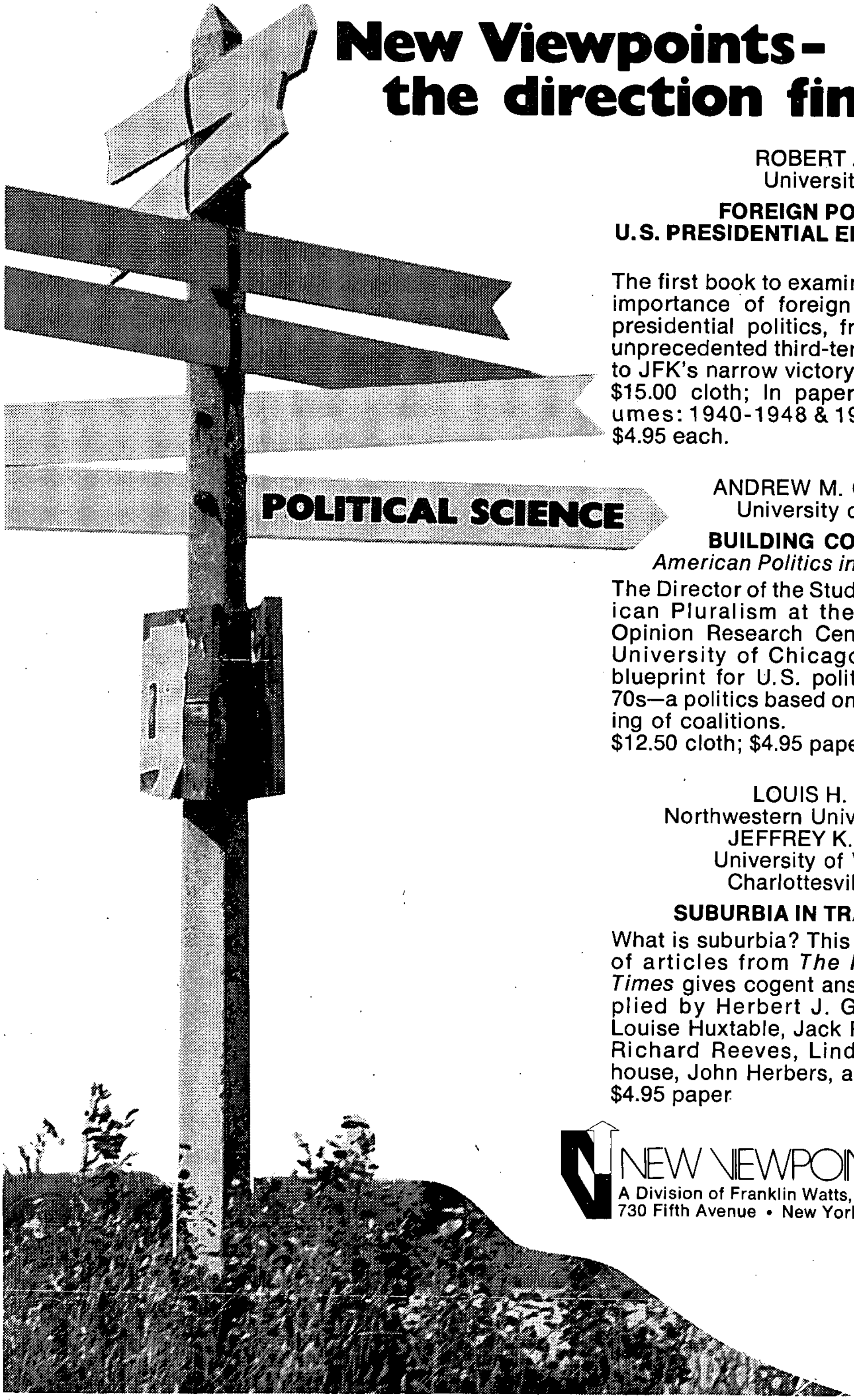

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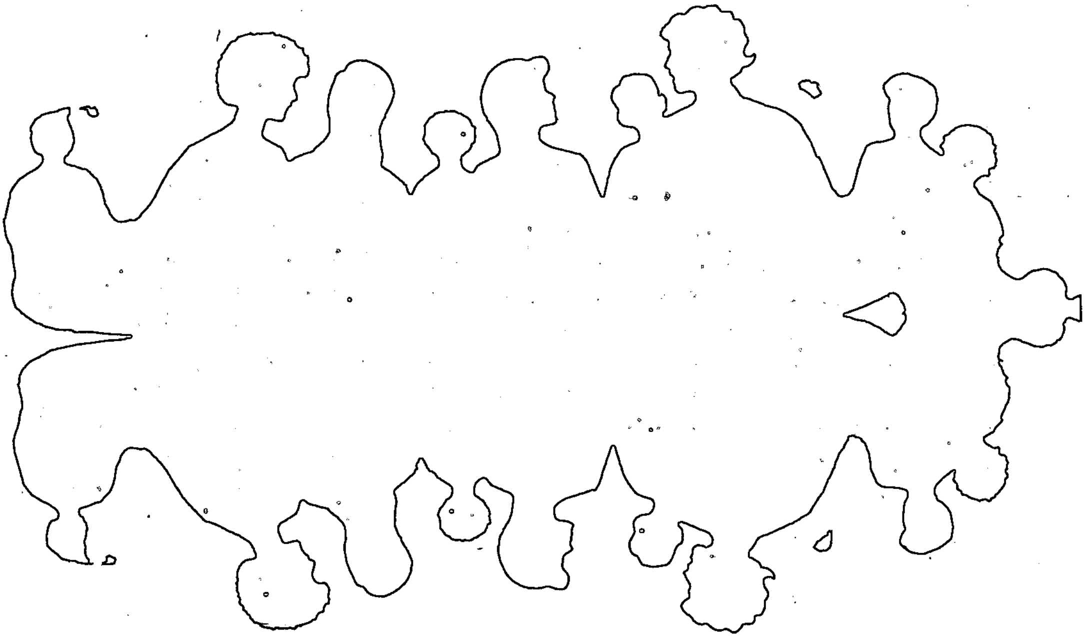

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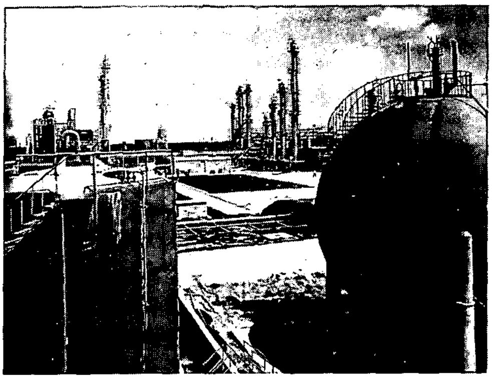

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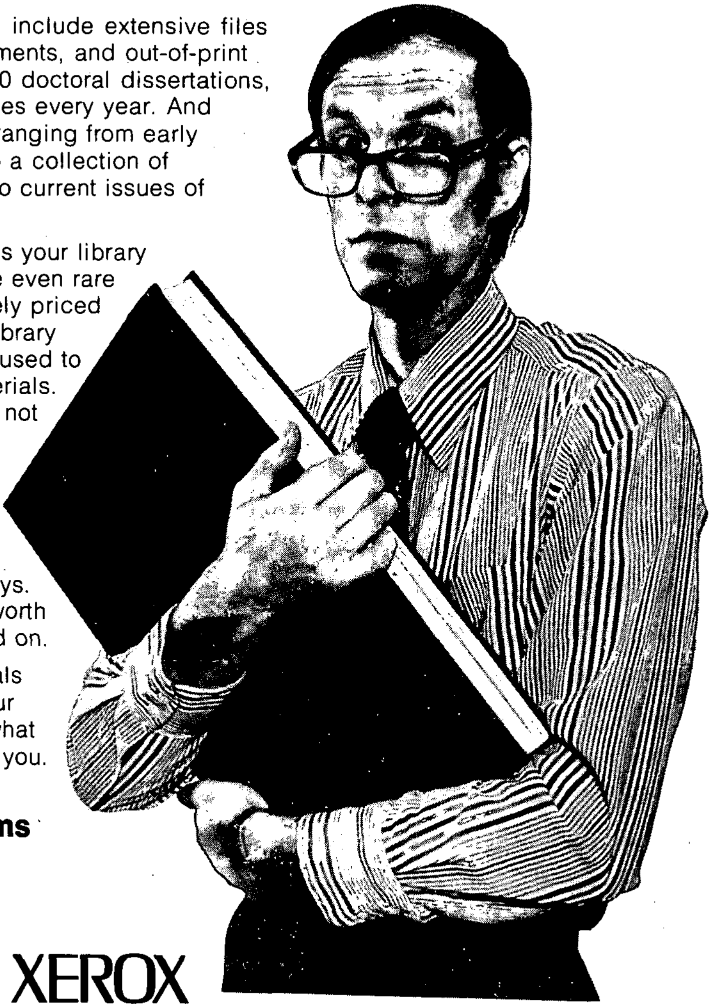

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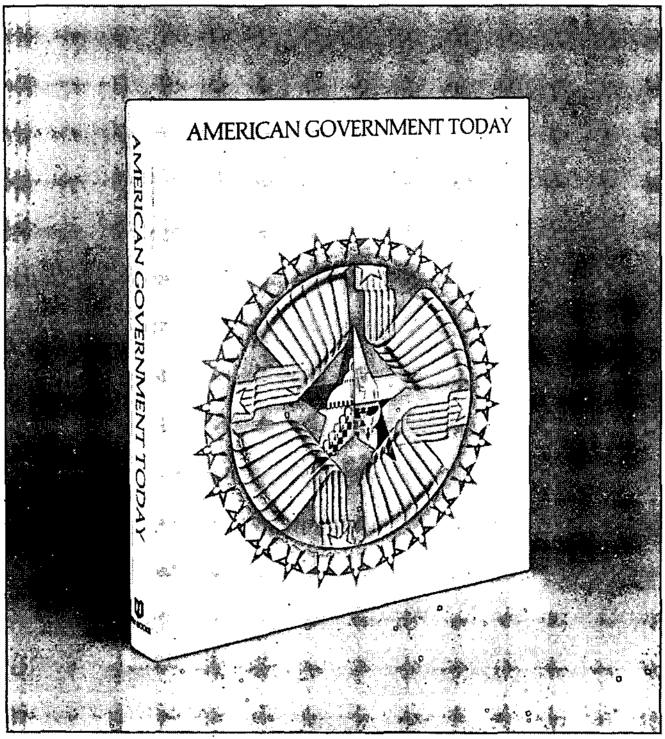

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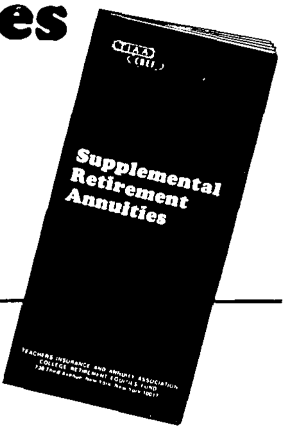
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